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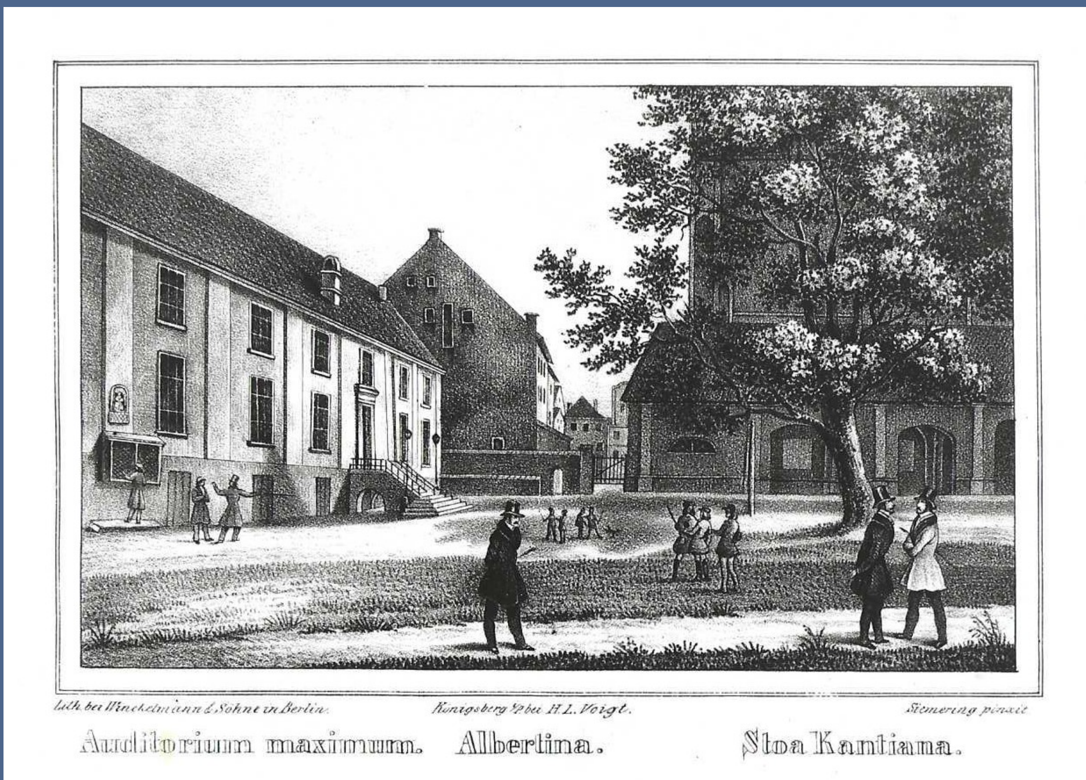

International Association of Mathematical Physics

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Contents

From the President

The new IAMP Executive Committee has been elected in October 2023. Many thanks to all of you who voted in this election and are thus actively shaping the future of our organisation. Of the previous EC, four members continue their service: Hugo Duminil-Copin, Marcello Porta, Jan Philip Solovej (Vice President) and me. The departing members are Dorothea Bahns, Percy Deift, Michael Loss, Bruno Nachtergaele, Stephane Nonnenmacher, Sylvia Serfaty, Herbert Spohn and Daniel Ueltschi. Let me thank the previous committee for all their great work and welcome the new members: Chiara Saffirio (Secretary), Sven Bachmann (Treasurer), Laszlo Erdoes, Jan Derezinski, Mathieu Lewin, Hal Tasaki, Alain Joye and Antti Knowles. I am really looking forward to working with this excellent team over the next 3 years!

The previous committee succeeded in modernising our database and introducing new payment methods to facilitate the payment of the dues. I would like to thank everyone involved in the process, especially Michael Loss, who put in a lot of time and effort to help develop the new system. Going forward, together with the new EC, I would like to continue the process of improving our database and the IAMP website. I am also happy to see that our on-line One World IAMP Mathematical Physics Seminar is still thriving, and I hope it will continue to attract excellent speakers and a broad viewership. Many thanks to all the current and past organisers and the members of the scientific committee! I encourage all our members to subscribe to the seminar's mailing list and to follow the seminar on the YouTube channel. It is a great opportunity to find out about the new results in mathematical physics, as well as to socialize with colleagues from all around the world. More information can be found on the seminar's website: https://www.iamp.org/page.php?page=page_seminar.

One of the priorities I want to focus on as the new president is to make our association more open and more diverse in all senses of the word, including gender, geographical and research-topic diversity. Let us also double our efforts to support the new generation of mathematical physicists and to build a supportive environment that will foster new ideas and creativity.

I am sure that we are all also looking forward to this year's ICMP that will take place in Strasbourg 1-6 July, preceded by the Young Researcher Symposium (28-29th of June). You can find more information about the congress and register on the event's website: https://icmp2024.org.

Thank you all for the continuing support and being a part of IAMP. Let us work together to make sure that mathematical physics continues to be an important part of both mathematics and physics, focusing on the unique strengths that the intersection of these two disciplines can offer.

Kasia Rejzner
IAMP President

The Koenigsberg Mathematico-Physical Seminar: A Nineteenth-Century Nursery for Mathematical Physics

by Kathryn M. Olesko

1 Overview

The Königsberg mathematico-physical seminar was a leading center for instruction in mathematical physics in the German states during the middle decades of the nineteenth century. Founded in 1834 at the Prussian University of Königsberg (since 1945 the University of Kaliningrad in the Russian Federation) by the mathematician Carl Gustav Jacob Jacobi (1804-1851) and the physicist and mineralogist Franz Ernst Neumann (1798-1895), the seminar was the first of its kind dedicated in part to mathematics; was the first in the German states to combine instruction of mathematics and physics; and was one of the first to implement the practice of teaching prospective secondary-school teachers in physics and mathematics not only knowledge at the forefront of the field, but also the techniques of research. By a decade after its founding, its innovative pedagogy based on systematically presenting recent developments and research methods in mathematics and mathematical physics became known throughout Europe and Russia.

The teaching of mathematics and mathematical physics at Königsberg impacted instruction in both subjects, broadly conceived, in the German-speaking world's secondary and tertiary educational systems where former seminar attendees were employed. The seminar trained mathematicians who specialized in mathematics or mathematical physics, spawned a generation of university physics professors specializing in mathematical or theoretical physics, and educated over one hundred teachers of mathematics or physics for Prussia's, and later Germany's, expanding secondary school system. In addition, concordant with the increasing technological orientation of the German economy, physicists educated in the seminar contributed to the scientific and mathematical foundations of nascent precision industries as well as of state projects on weights and measures, territorial mapping, and industrial standards.

Yet even though the Königsberg seminar played a significant role in shaping the definition of mathematical physics, the meaning of the term was never straightforward for most of the century. Early in the century it meant the abstract style of the French in applied mathematics, including mechanics or, especially in German-speaking areas, the quantitative determination of physical constants. Definitions varied. The mathematician Peter Gustav Lejeune Dirichlet (1805-1859) and the physicist Rudolf Clausius (1822-1888) each had their own ideas on what mathematical physics meant (Butzer 1987; Orphal 2023). Complicating matters from the side of physics was the common practice of using the terms "mathematical physics" and "theoretical physics" interchangeably (Olesko 1991; Bevilacqua 1995, Jungnickel and McCormmach 2017). Not until the end of the century, as a result of the work of physicists and mathematicians outside the orbit of the seminar, such as Ludwig Boltzmann, Heinrich Hertz, Max Planck, and others, did the disciplinary definitions of mathematical physics and theoretical physics congeal. This essay, which focuses on the period before 1876 when Neumann retired, takes a historical approach to the term "mathematical physics" as defined and used by those who claimed to practice it.

Ironically, the contributions of the Königsberg seminar to mathematical physics were attained in spite of, not because of, the combined teaching of mathematics and physics in a single institutional setting. Although designed as one, the seminar consisted of two separately run divisions, one for mathematics and the other for physics. The conceptual coordination of these divisions was minimal to non-existent. Directors rarely (if ever) consulted on curricular matters. The function of mathematics in each division differed. Whereas the mathematical division covered novel subjects like elliptic functions as developed by Jacobi and Niels Henrik Abel (1802-1829) that had obvious applicability to certain physical problems, the physical division, while not excluding those mathematical methods, generally fostered a more pragmatic and usable approach to the deployment of mathematics and combined it with an emphasis on the techniques of an exact experimental physics, especially methods of data reduction, error analysis, and the analysis of instruments.

It fell upon students to integrate the work of both divisions, but the original stipulation that students attend both divisions was never enforced, although about half did. Of the 272 individual students who can be identified with certainty as having attended the seminar, 221 (81%) attended the mathematical division, while 188 (69%) attended the physical division. Within each subgroup, 137 attended both divisions, 50% of the total. Students in the mathematical division attended the physical division less frequently (62%) than students in the physical division attended the mathematical division (73%). Semester attendance patterns indicate students generally favored one division over the other; of the 137 students who attended both divisions, very few attended both divisions with the same frequency. Overall, more students attended only the mathematical division (84) than attended only the physical division (51). Hence strong conclusions about the interpenetration of the subject matter of each division in the education of students cannot easily be drawn; the analysis of individual cases is more appropriate for such an assessment. Likewise, conclusions about the meaning of the seminar have to take into account other types of seminar members, including those who made the pilgrimage to Königsberg to join the seminar as an auditor or postdoctoral student without matriculating at the university.

Broadly speaking, two different kinds of mathematical physicists issued from the seminar (although some former students fell in the spectrum between them). For example, Carl Gottfried Neumann (1832-1925), Neumann's son, and Rudolph Friedrich Alfred Clebsch (1833-1872), practiced a mathematical physics with an emphasis upon mathematical techniques with minimal or no consideration of empirical results. By contrast, Oscar Emil Meyer (1834-1909) and Georg Hermann Quincke (1834-1924) prioritized the analysis of empirical results while relying on mathematical colleagues for the elaboration of mathematical methods. This group insisted that mathematical physics was inseparable from an exact experimental physics, especially the latter's deployment of error and instrument analysis. Mathematical physicists of the first type tended to occupy positions in mathematics in universities and secondary schools, while the second type could be found at universities and other scientific institutions or bureaucracies where their positions were in physics or mathematics, or at secondary schools where they taught physics, mathematics, or sometimes the natural sciences. This essay addresses the factors that played into the founding of the seminar; how the directors ran each division, especially how they achieved curricular stability; and finally, how complementary perspectives of mathematical physics were manifest in the careers of some representative students.

2 The seminar's founding

Neumann

Bessel

Jacobi

Neumann and Jacobi's seminar was part of Prussian educational reforms at the beginning of the nineteenth century. University seminars had been in existence since the eighteenth century when they were designed for the training of preachers and teachers. Rigorous instruction in intellectual subjects other than philology, history, or theology was not necessarily a part of them. Broad changes in the Prussian educational system in the first decade of the nineteenth century, including the reorganization and overall downsizing of the university system and the intellectual enhancement of instruction in the gymnasium (the classical secondary school with instruction in Greek and Latin), transformed university philosophical faculties from preparatory institutes for the three "higher" faculties (law, theology and medicine) to faculties equal to the other three, now with the responsibility for training the profession of secondary school teaching. In characteristic Prussian style, these institutional reforms included mechanisms for assuring quality control in the state's secondary schools, especially through professorial oversight. Requirements both for graduating from the gymnasium and for university matriculation were enhanced – so much so that students, including Jacobi (who was an exceptional student), complained about whether or not they could fulfill increasingly stringent requirements for gymnasium graduation and university matriculation. In 1810 the state introduced an examination for becoming a secondary school teacher (*Examen pro facultate docendi*, or *Oberlehrerexamen*), which required submission of an original essay or investigation, but only in select subjects. University professors administered the examination. Faculty participation on the state's examining board elevated the relevance of their disciplines in the minds of university students, who in turn sought courses taught by the board's members, believing that instruction under a board member would increase the chances of passing the examination. University representatives of disciplines not tested on the state's examination, noting student preferences, pleaded to be included on the board, even though membership entailed additional responsibilities.

From the perspective of the Prussian Ministry of Culture (*Ministerium der geistlichen, Unterrichts- und Medizinalangelegenheiten*, or *Kultusministerium*)–the bureaucracy responsible for overseeing instruction, certification, and hiring at all educational levels–secondary school mathematics was especially in need of reform, a situation that the state of Bavaria had already acted upon with success (Schubring 2014, 243). In Prussia where one could sometimes find philologists teaching mathematics, competent gymnasium teachers of mathematics were prime desiderata (Schubring 1991). Prussia's interest in the subject was partly ideological–it considered mathematics a part of the neohumanist curriculum (Schubring 2014)–and partly in recognition of the need for technically-oriented civil servants qualified to design, execute, and maintain a growing number of state infrastructure projects. Prussian technicians in charge of infrastructure projects were brought to the limits of their abilities in the new provinces acquired in the partitioning of Poland at the end of the eighteenth century when canal building and river rectification were needed for transportation and flood control (Olesko 2009). Shortly after 1810 university mathematicians–or their functional equivalents, such as the astronomer Friedrich Wilhelm Bessel (1784-1846)–were called to serve on the state's examining board, a task many regarded as onerous but necessary.

Ideally, testing would be based on enhanced training, but instruction was slow to change. A planned polytechnical school for training teachers in Berlin failed to materialize (Schubring 1981). University seminars in the natural sciences and mathematics for training teachers did not begin to appear until 1825. Once in operation, their educational mission had to contend with the ongoing transformation of the university professoriate. Research had by then become sine qua non for professorial appointments (Turner 1971; Turner 1981). Consequently, university seminars, like Königsberg's, tended to sideline the didactic element of teacher training in favor of the pursuit of high standards of subject matter competence, including (in some of them) the fundamentals of research methods. Yet operating German university seminars as research sites proved difficult. Gymnasium students tended to be ill-prepared in mathematics and the sciences, compelling university seminar directors to design pedagogically suitable means for introducing students to research methods. Their task was compounded by yet another development that unfolded in the decades before and after 1800: the rapid formation of scientific disciplines after natural history and natural philosophy spun off specialties like physics, chemistry, biology, and other subjects, a transformation that necessitated the concurrent development of pedagogies suited to convey the subject matter that defined them. Not all seminar directors succeeded in addressing these pedagogical challenges in a cogent fashion. Those who did transformed the subject matter they represented by creating new introductory and advanced courses, including the sequences in which they were offered; identifying canonical readings and topics; and honing standardized methods for research. This transformation relied heavily on the translation of research results into pedagogically suitable forms. Thus, seminars along with their complementary lecture courses were instrumental in shaping the scientific disciplines (Stichweh 1984; Olesko 1991).

Mathematics and the natural sciences benefitted from these institutional reforms. Over the course of the nineteenth century, fifteen seminars in the natural sciences–about half of which included mathematics–opened in the German-speaking provinces that in 1871 united to form Imperial Germany. The Königsberg mathematico-physical seminar, founded in 1834, was the third among these, preceded by two general natural science seminars (without mathematics) at Bonn and Königsberg. Only sixteen years later, in 1850 at the University of Göttingen, was a second mathematico-physical seminar founded. Six more combined seminars for mathematics and physics appeared by the end of the century, along with a short-lived physics seminar at the University of Giessen. Berlin and Giessen each had pedagogical seminars with an emphasis on the didactics of teaching mathematics and physics, but they existed outside the university in both cities. Ten seminars in mathematics were established between 1863 and 1885. In addition, private seminars with lectures and assigned problems but without state funding were organized by professors who wanted to avoid the administrative hassles of ministerial oversight, such as the mathematical seminar held by Dirichlet in his Berlin home beginning in 1834 (Merzbach 2018). Most combined seminars for mathematics and physics became seminars for mathematics alone by the early twentieth century. Among all German seminars in mathematics and the natural sciences, the Königsberg mathematico-physical seminar was singled out early on for its emphasis upon research, so much so that commentators dubbed seminar research "the Königsberg principle" (Lorey, 101), even though few publications issued directly from seminar exercises (less than half of the 272 seminar attendees ever published at all). From the start, then, the story of the seminar was cast in terms that accentuated its novelties rather than its everyday operation wherein assigned problems, similar to those that were later found in textbooks, were more common than independent projects. Nevertheless, despite contemporary and later historical misconceptions, the Königsberg seminar became known as an incubator of a school in mathematical physics (Olesko 1988; Olesko 1991, 1-18).

Understanding why Neumann and Jacobi decided to establish a mathematico-physical seminar begins with their personal experiences as students at the University of Berlin, itself a product of Prussian educational reforms in 1810 when, finally, Prussia established a university in its capital. Having drawn its faculty in mathematics and physics locally, instruction in these subjects at first leaned toward the practical concerns that had arisen in state projects. Neumann thought initially that he would study theology, but turned to mineralogy under Christian Samuel Weiss (1780-1856), who viewed the subject through the lenses of physics and geometry. Like Neumann, Jacobi began his university years as a student of the humanities. Initially he studied philology under August Leopold Boeckh (1785-1867), who introduced Jacobi to the seminar experience; the significance of that experience for the Königsberg seminar is still unclear. Both equivocated over their choices and pursued other interests independently. Neumann and Jacobi were autodidacts in mathematics who had shunned Berlin's mediocre lecture courses in mathematics. Jacobi, a virtuoso calculator who had begun teaching himself mathematics while a gymnasium student, immersed himself in the classical works of Leonhard Euler (1707-1783), Joseph-Louis Lagrange (1736-1813), and Pierre Simon Laplace (1749-1827). Jacobi passed the *Oberlehrerexamen* in 1824 under the board member and mathematician Friedrich Theodor Poselger (1771-1838), who taught at the *Allgemeine Kriegsschule* (War College), and then earned his doctorate in 1825. His written submissions for both were in mathematics. Weiss sparked Neumann's interest in physics and mathematics, which was deepened by a fortuitous encounter with a mining official in Berlin who recommended Joseph Fourier's (1768-1830) work on the analytical theory of heat (Fourier 1822), just a year after it was published. Fourier made an indelible impression on Neumann's investigative style. He copied the entirety of Fourier's treatise in a reading notebook from 1823-26 (F. Neumann 1826). The notebook also included passages on the method of least squares, which was then spreading from astronomy to other sciences, and on Siméon-Denis Poisson's (1781-1840) thermal studies and his integration of the wave equation (Poisson 1823). So alone and on his own, Neumann tackled some of the latest quantitative techniques used in the physical sciences. By the time he wrote to the Ministry of Culture in January 1825 to apply for an academic position, his independent studies had reshaped his intellectual profile. He expressed an interest in teaching not only minerology from a mathematical and physical perspective, but also "those sections of physics have that have received a higher mathematical treatment or that are now capable of being treated mathematically" (L. Neumann 1904, 226). He completed his dissertation in 1826 under Weiss's direction. Ironically, even though their interests strongly overlapped while in Berlin, there is no indication that Neumann and Jacobi met one another until both were called to Königsberg in mid-1826 (Koenigsberger 1904, 6-17; L. Neumann 1904, 89-146, 225-27).

A second factor shaping the contours of the early stages of their seminar concerns ministerial decisions regarding the University of Königsberg. The Ministry of Culture had specifically chosen the university as a center for the promotion of instruction in mathematics and the natural sciences. The choice was strange, but in at least one respect geopolitically strategic. Königsberg was a distant outpost on the Baltic Sea in East Prussia, a province that had not been linked by a land bridge to Berlin until the addition of the province of West Prussia, acquired in the Polish partition of 1772. Other provinces acquired in the partitions of 1793 and 1795 were subsequently lost in the Napoleonic Wars (1806-1815 in the German states), and then only partially regained with the Congress of Vienna in 1815. Yet it was during the Napoleonic Wars–a period marking Prussia's largest territorial loss to date–that the Ministry offered Bessel, who did not have a doctorate, a position at the university and promised him a new observatory. Its reasons for doing so during a period of wartime stress and financial constraints are not entirely clear. One reason might have been the symbolic enhancement of the city as the second Prussian capital. Königsberg was the traditional site of Prussian coronations and it was where the monarchy had retreated during the Napoleonic Wars. Another reason, more likely, was geopolitical. Along Prussia's eastern border the Russian Empire was at the time surveying its western border to certify its claim to territorial acquisitions from the partitioning of Poland. One of the functions of the new Königsberg observatory under Bessel's direction was to determine its meridian–a normal enough task for any observatory, but in this case, the meridian was also needed for the complementary triangulations that would be used to settle territorial and border claims.

Bessel was an ally of the Ministry on another matter: he had a keen interest in promoting, besides astronomy, mathematics (especially applied mathematics) and the natural sciences. Much to his delight, the Ministry made several new appointments in the natural sciences and mathematics at Königsberg in the mid-1820s, including Jacobi and Neumann. It did not take the group long to realize that there was a critical mass of instructors that could service a seminar, which would be in the state's interest for the promotion of teacher training. Moreover, a seminar would bring funding for student stipends or premiums, scientific equipment, and perhaps even extra stipends for faculty or faculty assistants. Representatives of the natural sciences at Königsberg were not in agreement, however, about what this seminar would cover, or even what purpose it served. Bessel refused to participate, preferring instead to train students in astronomy through apprenticeship and independent work. With the exception of Neumann, Jacobi, and Bessel, Königsberg natural scientists shunned mathematics as a part of instruction in the natural sciences. Yet because he taught mineralogy in addition to physics, Neumann initially thought that it was to his advantage to remain part of the group's 1833 proposal for a natural sciences seminar. In the end, though, the explicit exclusion of mathematics from this seminar led Neumann to change his mind. Instead, he paired with Jacobi to propose a mathematico-physical seminar, which was approved in 1834. The rift between the two camps of natural scientists over the role of mathematics in the sciences would taint professional relations between them for decades (Olesko 1991, 21-60).

3 The operation of the seminar

Working through how the seminar would be taught, what students' work would be, and how it would relate to each director's lecture courses was a difficult process of fits and starts from 1834 to the early 1840s when, finally, the seminar's operation stabilized. Developments before the founding of the seminar suggested that its operation should have proceeded smoothly. The eight years from 1826 to 1834 marked an intensely creative period in teaching and research for Neumann and Jacobi when their lecture courses moved almost lockstep with their respective research interests. These initial pedagogical efforts eventually became some of the building blocks of their seminar instruction, but they were not all that was needed.

Once at Königsberg Jacobi immersed himself in number theory after his reading of Carl Friedrich Gauss's (1777-1855) *Disquisitiones Arithmeticae* (1801). He began a nearly daily exchange with Bessel on several mathematical matters, including Bessel functions, the method of least squares, and even problems in triangulation and geodesy. His crowning achievement in the late 1820s, though, was his theory of elliptic functions, the inverse of elliptic integrals, including the elliptic theta function. Elliptic functions were relevant to problems in physics, especially the pendulum. His investigations led to a priority dispute with Abel, who had proved the double periodicity of the inverse function of elliptic integrals in 1825, which Gauss also claimed to have done. The controversy subsided, but did not disappear, when Abel died in the year Jacobi published his results (Jacobi 1829). Only decades later, when Jacobi's letters to Adrien-Marie Legendre were first published, were Jacobi's contributions confirmed (Pieper 1998). Jacobi proceeded to apply elliptic functions to geometry, analytic mechanics, number theory, and cyclotomy (the division of a circle). Before the founding of the seminar, he also worked on the theory of determinants, differential geometry, partial differential equations, variational calculus, and equilibrium states in rotating fluids. Adopting the mathematical formulations of Hamilton and Lagrange, he transformed the mathematical treatment of problems in mechanics where, in addition, he developed computational methods for perturbations in planetary paths (Koenigsberger 1904, 18-161; Pieper 1998). Although the preponderance of Jacobi's research was in pure mathematics, it was not entirely devoid of applications.

His lecture courses followed suit. Between 1826 and 1834 they included trigonometry and analytical geometry, cyclotomy, conic sections, number theory, hyperelliptic integrals, Bessel functions, Fourier series, elliptic functions and transcendentals, higher calculus, the analytical theory of probability, the theory of partial differential equations, and the application of several mathematical techniques–including Hamiltonians and Lagrangians–to problems in mechanics and astronomy. As student notebooks from these courses attest, Jacobi did not use textbooks for these courses; he developed his lectures from scratch and, for the most part, from his own research (Jacobi 1866; Jacobi 1996; Jacobi 2007). He tried to help his students master these difficult and new subjects by offering, in private, exercise sessions in which he assigned problems as aids to learning. (Exercise sessions were not ordinarily offered as a part of lecture courses at the time.) He mentored students for the *Oberlehrerexamen* for which he was a board member. And he was generous with his teaching hours. His first lecture course on elliptic transcendentals in the summer semester of 1831 met for eight hours a week; most other courses met for three to six hours (Koenigsberger 1904, 18-161).

After his arrival at Königsberg, Neumann's research interests shifted from the geometry to the physics of crystals and minerals with an emphasis on carrying out "accurate quantitative measurements of the various physical characteristics of inorganic nature" (F. Neumann 1829). He was thinking in particular of the measurement of specific heat. In 1831 he published two papers on the topic, one on the specific heat of minerals and the second on the specific heat of water using Fourier's analysis as the mathematical foundation of his investigation. He then turned to problems in optics in 1832, writing on the theory of double refraction as derived from the equations of mechanics and on the elliptical polarization of light. Further crystalline properties occupied him over the next two years including the thermal and optical axes of crystals; elasticity in crystals; and the behavior of polarized light in crystals (Volkmann 1896, 39-40). Of note in these publications, unlike his earlier ones, Neumann exhibited a greater concern for the analysis of instruments, errors, and measurements to the point where the rendering of his experimental procedure and the analysis of errors could occupy an entire publication (Olesko 1991, 77-81). This change in his investigative style was a result of his adaptation of Bessel's style in experiment, especially Bessel's seconds pendulum investigation (Bessel 1828), which Neumann witnessed firsthand when he arrived at Königsberg. Precision measurement, the calculation of accidental errors through the application of the method of least squares, and the analytical determination of constant errors were mandatory elements of this style, which was also known as exact experimental physics (Olesko 1991, 68-72). Like Jacobi, Neumann taught a wide variety of courses in eight years. Following the direction of his research, most concerned the physical properties of crystals and minerals. But he began to break out and offer courses in physics proper, including experimental physics, the theory of light, the theory of heat, and a general course on physics. A further indicator of this shift occurred in the summer semester of 1832 when he offered for the first time a course on topics in mathematical physics, a course that allowed him to introduce new research without committing to a full-blown lecture course (Volkmann 1896, 56; Olesko 1991, 464-68).

As commendable as Jacobi's and Neumann's lecture courses were as novel systematizations of their own research combined with the related research of others, they were not as successful as either one had hoped. Attendance at Jacobi's lectures usually numbered 5-6 students; rarely did his enrollment reach ten. Although many of Neumann's courses had similar low enrollments, his teaching proved to be more attractive to students than Jacobi's. General courses on the physics of the earth, the physical characteristics of minerals, general physics, and mineralogy all attracted more than ten students. Surprisingly, courses based on his reading of Fresnel and Fourier on the theory of light and the analytical theory of heat–which required knowledge of advanced mathematics–also drew ten or more students. Also unexpected but welcome were the ten students who enrolled in his course on topics in mathematical physics (Volkmann 1896, 56; Olesko 1991, 464-68).

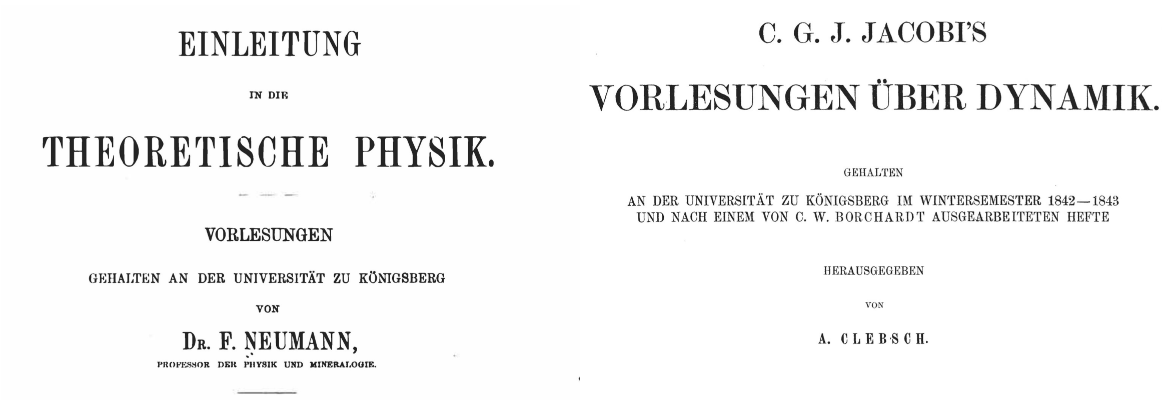
All things considered, both Neumann and Jacobi appeared to have been well-prepared to undertake the pedagogical challenges of their new seminar in 1834. Ideally students too should have been ready for the seminar's challenges. Upon graduation from the gymnasium, students would have covered mathematics to the level of analytical geometry, algebra, conic sections, spherical trigonometry, the theory of series, and the fundamentals of probability theory. A few progressive gymnasiums taught calculus. In 1834, the year the seminar opened, conic sections and spherical trigonometry were removed from the list (Schubring 2014, 243). Neumann and Jacobi furthermore expected students to know physics at the level of the influential and popular textbook, Ernst Gottfried Fischer's *Lehrbuch der mechanischen Naturlehre* (Fischer 1826-27). Knowledge of differential and integral calculus was a prerequisite for the seminar. First year students initially were not allowed attend the seminar but were expected to fill in the gaps in their learning, especially calculus and the basic elements of physics. Employed teachers and postdoctoral students without the necessary background could join the seminar as auditors. A sequence of courses in mathematics or physics was not prescribed at the time of the seminar's opening. Both Neumann and Jacobi described the seminar's activity as hierarchical, starting with student-led reports on recent literature and practical exercises and culminating in an original investigation, which students ideally would submit to one of three journals: *Journal für die reine und angewandte Mathematik*, *Astronomische Nachrichten*, or the

Annalen der Physik und Chemie. Neither Neumann nor Jacobi committed to running supplementary exercise sections; rather, a *Privatdozent* (assistant professor) would–in Latin. Upon publication of their work, students would receive a modest monetary premium. In physics the cost of equipment would be reimbursed. Thus, the initial plan for the seminar as indicated in the statutes envisioned it as a training ground for research, not for learning how to teach, which the seminar statutes did not even mention. The unstated assumption, though, was that most seminar members were destined to become secondary school teachers (Olesko 1991, 99-104, 461-63).

Their expectations and plans proved naïve. In the first year, Jacobi assigned problems on conic sections and spherical trigonometry, but not everyone could complete them. One student was actually held back for poor performance; others were deemed flat-out deficient. Neumann managed to mentor one of the seminar students in an original investigation on the optical properties of thin films. The others he had to coach individually on various topics in physics, a time-consuming task. He was outspoken in expressing his deep regret that students did not know as much physics as he thought they would. The entrance examination made no sense in light of their experiences, so they abandoned it after the first year. From the start, then, both Neumann and Jacobi were drawn more deeply into teaching and into figuring out what, exactly, would make the seminar work as they had planned. The reason Neumann gave for the students' poor knowledge of physics was the state examination for teachers, which did not require written work in the subject as it did for mathematics. The Ministry of Culture thought otherwise, blaming Neumann's lectures for the students' lack of preparation (Olesko 1991, 103-106).

Problems in the operation of the seminar continued as Neumann tested various means for improving student performance. He began to align the seminar topics and exercises with his lecture courses from the previous semester. He added written assignments of the type he hoped would be a part of the state's teaching examination; these were also based on his lecture courses. Gradually he learned how to select material his students could handle. But he had to recreate his seminar pedagogy each semester, trying each time to figure out what worked best for students. Jacobi in the meantime continued to offer advanced courses, and tried to lower the level of the work in the seminar, but soon returned to his exposition of more difficult topics from his own research. Those courses took up a considerable amount of Jacobi's time, so he began to cancel the meetings of the mathematical division of the seminar. He moved his pedagogy strongly in the direction of a pure mathematics, further distancing his division from Neumann's. By the summer semester of 1836, seminar attendance dropped to three students in each of the divisions, with only one overlapping student registering for both. Over the next few years, Neumann and Jacobi struggled to keep the seminar open (Olesko 1991, 106-127).

Not until the early 1840s did the physical division achieve stability under Neumann, while the mathematical division did not stabilize until Jacobi's first doctoral student from 1831, Friedrich Julius Richelot (1808-1875), replaced him formally as director of the mathematical division in 1844 after Jacobi left Königsberg for a position he had longed for at the Academy of Sciences in Berlin. (Jacobi's negotiations with the Ministry about this move, which was ostensibly occasioned by his failing health, began in 1841.) In the winter semester of 1838/39 Neumann offered a new course that was not based on a topic drawn from his research but was closely related to his developing investigative style. Entitled "Introduction to Theoretical Physics," it was a course on mechanics, but not in the way French mathematical physicists would have conceived it. To date mechanics had entered his teaching and his research primarily as a means to analyze instruments that were used in physical investigations. Whereas his earlier lecture courses, and even the conception of the seminar, were inspired by his research, the motivation for this course came from his teaching and from his experiences in learning what students needed in order to pursue the study of physics.



EINLEITUNG

IN DIE

THEORETISCHE PHYSIK.

VORLESUNGEN

GEHALTEN AN DER UNIVERSITÄT ZU KÖNIGSBERG

VON

DR. F. NEUMANN,

PROFESSOR DER PHYSIK UND MINERALOGIE.

C. G. J. JACOBI'S

VORLESUNGEN ÜBER DYNAMIK.

GEHALTEN

AN DER UNIVERSITÄT ZU KÖNIGSBERG IM WINTERSEMESTER 1842—1843
UND NACH EINEM VON C. W. BORCHARDT AUSGEARBEITETEN HEFTE

HERAUSGEGEBEN

VON

A. CLEBSCH.

Neumann's Introduction to Theoretical Physics and Jacobi's Lectures on Dynamics

Neumann opened with the statement that this course was an "introduction" because mechanics was the foundation of physics. But he did not mean that mechanics was the conceptual foundation of physics. He referred to this course as an introduction to "practical physics." The course had seven units: applications, gravity, hydrostatics, aerostatics, the conservation of living force (a later addition for the published version), hydrodynamics, and aerodynamics. One-third of the course covered gravity. Here he introduced the equations of motion. But it was the pendulum that dominated this unit and occupied one-quarter of the course overall. His discussion of the pendulum brought to the practice of physics the exacting methods Bessel had used in his seconds pendulum investigation. Neumann noted there was (at the time) no other instrument that was as accurate as the pendulum. And gravity, which was measured by determining the length of the seconds pendulum, was the physical quantity that (at the time) was known with the greatest precision. So while he exposed students to several mathematical techniques in this unit (series expansions, approximation techniques, solutions to simultaneous equations, and so on), it was primarily the methods of an exact experimental physics that he was interested in conveying to his students, including error analysis for the determination of constant or systematic errors, the method of least squares for determining accidental errors, measuring techniques, instrument construction and analysis, and the finer details of experimental protocols, such as coincident observations, which had also been developed by Bessel. Of note: Neumann took special care to explain Bessel's reduction of pendulum vibrations to a vacuum by the use of a hydrodynamic correction for the motion of a pendulum in air, which yielded higher degrees of accuracy than Newton's hydrostatic correction for the pendulum in air. (Indeed, one could say that the sequence of units in this course took cues from the divide that Bessel had created in experimental procedure by surpassing the accuracy of Newton's hydrostatic correction with his own hydrodynamic one.) This course, which all of his seminar students took, had a profound impact on their investigative styles (Peters 1870/71a; Neumann 1883; Olesko 1991, 128-171).

A notebook from the seminar on theoretical physics by Paul Peters in the winter semester of 1870/71 illustrates the types of problems that Neumann used to follow up on his lecture course (Peters 1870/71b). They included: the equations used to determine the influence of gravity and roll in Atwood's machine; free fall and vertical motion in a resisting medium; the cycloidal and ballistic pendulums; the analysis of bifilar suspension; the determination of small intervals of time with a pendulum; and hydrostatic corrections for capillary action in a tube. The ultimate objective in all of these exercises was to achieve analytical expressions suited for the determination of constants through measurements or suited for comparing theoretical values to empirical ones. The determination of accidental and constant errors was essential in all exercises assigned in the seminar; students faltered most on this component. Through these exercises, students learned physics, to be sure, but more to the point: students engaged with physics, learning how to do physics, how to deal with the mathematical formulas of physics, how to execute experiments, and how to make results tractable. These seminar exercises–indeed all of Neumann's seminar exercises–inculcated standards of judgment and normalized that judgment. Simply put, errors of measurement had to be as small as possible–which was a way, too, of measuring students' abilities and comparing them to one another.

His course on mechanics as an introduction to theoretical physics not only created a foundation for his curriculum, both in lecture courses and in the seminar; it also shaped a particular approach to the practice of theoretical or mathematical physics. The subsequent history of the physical division of the seminar revolves around this course on theoretical physics and its accompanying exercises, which became the foundation for the physics curriculum at Königsberg. At a time when mechanics was more a part of mathematics than physics, the course moved mechanics closer to physics. This course was required. Neumann offered it every four or five semesters over the ensuing decades in order to guarantee that every incoming cohort had an opportunity to take it. It provided essential conceptual tools not just for the seminar, but also for his other lecture courses, which systematically covered branches of mathematical physics (F. Neumann 1881-94). This course sequence was the first of its kind in mathematical physics; for a long time it was the only such sequence in German-speaking Europe. The course on mechanics as an introduction to theoretical physics was the foundation for making physics instruction a matter of *Ausbildung*, or systematic training, by preparing students for the measuring and mathematical techniques that Neumann developed in his lectures and applied in his seminar exercises. Finally, and perhaps most importantly, this course made it possible to bring to the practice of physics not only those who were talented for it, but also those who could be trained.

Neumann's view of the dependency of mathematical physics upon an exact experimental physics was enriched by his willingness to develop novel applications of mathematics to physical problems–in other words, to make mathematics serviceable to physics. He applied Laplacian operators to geographical longitude and latitude, to equilibrium conditions in theories of diffusion, and to the study of potential functions, noting cases where boundary conditions had to be fulfilled. His early work on crystals was geometric but later he was especially successful in applying Navier's elasticity equations to the theory of double refraction in crystalline media. Having learned about Bessel functions while at Königsberg, he applied them to the analysis of colors in polarized light. Students remember that he often spoke of other mathematical applications in the seminar that he had not published. Sometimes he would pass these applications on to students to use in their own research (Wangerin 1910).

Unlike Neumann, Jacobi never selected a "foundation" for his lecture courses. Central to his offerings, though, was his course on and seminar exercises in dynamics, which was first offered in the winter semester of 1837/38 (Jacobi 1866). They were the principal venues through which students learned the mathematical methods of mechanics, including applications of Jacobi's elliptic functions to physical problems. They stood in sharp contrast to Neumann's course on mechanics as an introduction to theoretical physics. His course on dynamics was considered the first innovative standard work on analytical mechanics since Joseph Louis Lagrange's *Mécanique analytique* of 1788; both he and Lagrange had taken an axiomatic approach to mechanics, which began with first principles, eschewed philosophical considerations, and viewed analytical mechanics as a part of mathematics. His course was less an introduction to mechanics than a transformation and integration of the fundamental equations of dynamics. Jacobi's course began with the differential equations of motion, equilibrium, small vibrations, virtual velocities and their significance for machines, and the principle of least action. It quickly turned to mathematical methods–Lagrangian multipliers, Hamiltonian integrals, elliptic integrals and elliptic functions, and so on–that could be applied to increasingly complex forms of motion from the motion of a point in a plane to the motion of planets using polar coordinates (Jacobi 1866). To Bessel he reported that his students found the course rather difficult (Koenigsberger 1904, 242). One wonders if Neumann's course, offered one year later, was conceived as a deliberate counterpoint to Jacobi's, which would not have served the students of physics well given the direction in which Neumann's teaching and research were moving. Had Jacobi's course on dynamics become the introduction to theoretical physics, the seminar exercises in the physical division would have been different, and so too would have the standards of judgment that guided the practice of physics.

It would be misleading, though, to characterize Jacobi as viewing the use of mathematics in physics only as an analytical, technical exercise divorced from applications. Although he is generally regarded as unconcerned with the empirical side of mathematical physics or with applications of mathematics until his Berlin lectures on analytical mechanics in the winter semester of 1847/48 (Jacobi 1996; Pulte 1998), where he criticized Lagrange on several counts, before then he did take an active interest in the applied work of his colleagues and acquaintances. For instance, he wrote to Bessel on several occasions about the application of the method of least squares to sets of empirical data; discussed problems in physics with his brother, the experimental physicist Moritz Heinrich Jacobi; became actively engaged in the interpretation of Neumann's difficult-to-understand electrodynamical studies; and supported Hermann von Helmholtz's pathbreaking and controversial study of the conservation of force. Significantly his Berlin course on analytical mechanics removed mechanics from the realm of pure mathematics by rejecting the notion that analytical mechanics, purely on the basis of its mathematical foundation, could lay claim to absolute certainty and mathematical exactitude. Instead, mathematics as it is applied to mechanics could only lead to probable truth. Like Neumann's pedagogical reconfiguration of mechanics as the introduction to theoretical physics, Jacobi's initial formulation of dynamics and revised view of analytical mechanics were parts of the nineteenth-century turmoil over the disciplinary role mechanics played in physics and mathematics and over what kind of pedagogical rendering of mechanics was appropriate and fitting for teaching both subjects. While Jacobi's later course on analytical mechanics was not offered at Königsberg, it was nonetheless known to seminar students who journeyed to Berlin to learn from him.

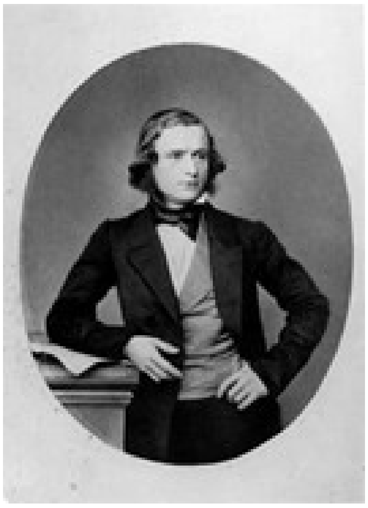
Stability in the mathematical division arrived with a change in leadership. Richelot took over the directorship of the mathematical division of the seminar in 1844 when Jacobi left Königsberg permanently for Berlin after having arranged an appointment at the Academy of Sciences. Richelot's 1831 doctoral dissertation under Jacobi was on the division of a circle into 257 equal parts. He had been on the faculty at Königsberg since then and often assisted Jacobi in the seminar, which he directed when Jacobi was on leave. Under Richelot's directorship the mathematical division of the seminar underwent major transformations that led to higher enrollments over the next three decades; whereas Jacobi only had a handful of students in the seminar, Richelot's enrollment by the 1850s grew to twenty and sometimes thirty students a semester. Whereas Jacobi had successful translated his research interests into advanced courses, Richelot drew more students to the study of mathematics by being more attentive to their needs as beginning students, reserving the more difficult courses for advanced students. Whereas Jacobi offered courses with no particular sequencing among them, Richelot created an intellectual hierarchy of courses, choosing as the primary introduction to the mathematical division analytical geometry, and then augmenting it at the introductory level with courses on trigonometry, the theory of algebraic equations, and introductory calculus. (He had first tried to use dynamics as an introduction to the mathematical division, but that did not work.) His more advanced courses were nonetheless similar to Jacobi's: the application of elliptic transcendentals to physical problems (e.g., a pendulum with finite vibrations or a planet in a resisting medium); the application of higher mathematics to potential theory; Cauchy's analytical algebra and Euler's finite analysis; the application of elliptic functions to analytical mechanics; topics from Jacobi and Lagrange; series and their convergence; and other topics (Olesko 1991, 141, 197, 212-19, 252-54).

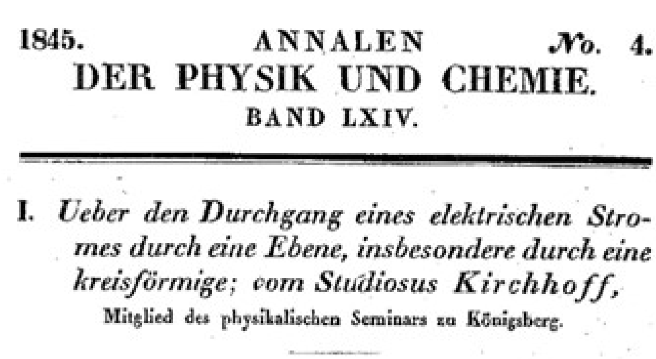
In order to accommodate these changes, Richelot split his division into two sections, one for beginning students, and the other for advanced students; the beginner's section was designed to correct for the gaps in the students' knowledge of mathematics from the gymnasium, which Richelot often found was wanting. For instance, when Paul Gordan (1837-1912) entered the seminar as an auditor after spending nearly two years at Berlin and Breslau, Richelot was unimpressed by his preparation; he made Gordan start learning mathematics all over again. The change in the seminar's organization allowed first-year students to register, which the original statutes had forbidden. His hierarchical organization of the seminar and of its topics resulted in higher enrollments by the 1850s, including more students capable of taking on advanced projects. (Although some of this increase was undoubtedly due to the overall increase in the university student population.) But there were consequences of these changes for him, and for the allocation of his time. The extra section added to his contact hours with students, but so did the hours he spent going over homework problems, constructing appropriate exercises and problems for the introductory section, and preparing informal lectures on subjects the students needed. To alleviate his burden he asked Königsberg's astronomer, Eduard Luther (1816-1887), also a former seminar student, to offer the introductory lectures and exercises in analysis and integral calculus so that students could move on to elliptic functions and analytic mechanics. He also relied upon two other Königsberg mathematicians who were associate professors (*Außerordinarien*) and former seminar members for assistance in the seminar, especially when the enrollment increased markedly. He realized that he could assign students outside reading and encourage them to lecture to one another on the reading so that they, not he, were responsible for fulfilling the gaps in their knowledge. Eventually he found that students became sufficiently motivated to read original papers in mathematics on their own (Olesko 1991, 141, 197, 212-19, 252-54).

Throughout all of these pedagogical changes, Richelot kept his line of sight on what he believed was the professional purpose of the seminar: the training of secondary school teachers. He did not, however, abandon the notion that the proper way to train teachers was through the cultivation of mastery of subject matter, not pedagogy and didactics. Accordingly, he played a major role in the 1866 revision of the *Oberlehrerexamen*. Richelot successfully argued for the enhancement of the original written work on the examination by specifying the subject areas where a research capability in mathematics had to be demonstrated: higher geometry, higher analysis, and analytical mechanics. He also–and perhaps more importantly–successfully deepened the mathematical knowledge needed for the physics part of the examination. Candidates who wished to teach physics in the upper classes of the gymnasium–prima and secunda–had to master the methods of mathematical physics and understand how to use physical instruments as well as the exercises that were appropriate for them. He also insisted, again successfully, that teaching candidates in physics be competent in mathematical geography and astronomy. In his explicit promotion of research and of mathematical physics, Richelot, an advocate of disciplinary specialization in the teaching profession, incorporated into the state examination the practices that had defined the Königsberg seminar, thereby further extending its reach (Kultusministerium 1867). When speaking of the Königsberg seminar, most commentators refer to Neumann and Jacobi as its directors. Yet Richelot directed the mathematical division for over thirty years; Jacobi, for less than ten (if we subtract his medical leaves).

That persistent historical elision does a profound injustice to Richelot. It contributes to the underestimation, if not masking, of the crucial roles Richelot played in stabilizing the curriculum of the mathematical division of the seminar and in upgrading the training of secondary school teachers along the lines of the Königsberg curriculum in mathematical physics.

Seminars were demanding. They required students to submit written work; lecture courses did not. While students had to attend lecture courses to graduate from the university, they did not have to sign up for a seminar. Enhanced curricula alone did not attract students to them. Seminar directors everywhere recognized that students had to be enticed to attend. Neumann and Richelot considered the premiums that they were able to offer the best six or so students at the end of the semester–usually between 15 and 45 thaler (a full professor's salary was around 900 thaler)–as carrot sticks used not only to draw students to the seminar, but also to encourage them to compete with their peers by demonstrating excellence. Premiums also contributed to the diversification of the seminar's clientele: students with limited financial means who might not have enrolled viewed the awards as enticements to join. Each director assigned specific problems that they used to gauge student performance to determine the award of premiums, which they usually shared between the two divisions. A premium was a badge of honor. Students could also distinguish themselves–and win a larger premium–though the annual prize question posed by the philosophical faculty, a competition that fed directly into seminar activity because answering it necessitated original research. Sometimes winning submissions became doctoral dissertations. With so many disciplines represented in the philosophical faculty, though, prize questions for physics or mathematics were infrequent. They could nonetheless be professionally consequential.





1845. ANNALEN №. 4.
DER PHYSIK UND CHEMIE.
BAND LXIV.

I. *Ueber den Durchgang eines elektrischen Stromes durch eine Ebene, insbesondere durch eine kreisförmige; vom Studiosus Kirchhoff,*
Mitglied des physikalischen Seminars zu Königsberg.

Kirchhoff and the published version of his seminar exercise on the distribution of an electric current on a circular plane (1845). Note he identifies himself as a member of the seminar.

The most well-known prize question in physics appeared in 1846 when Neumann asked students to determine, using Neumann's own electrodynamic research from 1845 and 1846, the constant upon which an induced electric current depended (usually referred to as the induction constant), which he had not yet solved. The answer required both a theoretical and mathematical understanding of Neumann's research and experimental measurements. Gustav Robert Kirchhoff (1824-1887), then a 22-year-old native of Königsberg, wrote the prize-winning essay (Kirchhoff 1846) a year after distinguishing himself in the physical division of the seminar. Kirchhoff, who originally had intended to study mathematics when he entered the university in 1842 at the age of 18, had begun his studies at a time when Jacobi, suffering from diabetes and often on medical leave, did not direct the mathematical seminar for several semesters. Between 1843 and 1847, Kirchhoff registered for the physical division for seven semesters, and for the mathematical division for two and possibly three semesters when Richelot directed it. While in the physical division of the seminar, Kirchhoff had completed an original investigation on electric currents that Neumann had assigned as a seminar exercise. Published in the *Annalen der Physik und Chemie* in 1845, Kirchhoff's seminar results became known as Kirchhoff's Laws (Kirchhoff 1845). Kirchhoff's response to Neumann's 1846 prize question on the induction constant became his doctoral dissertation in September 1847 (Kirchhoff 1847), which in turn was further developed in several publications that led to exchanges with the physicist Wilhelm Weber on electrodynamics and changes in Kirchhoff's research agenda (Kirchhoff 1849). Kirchhoff's reputation soared after these achievements, which contributed significantly to the national and international reputation of the seminar, for which Kirchhoff became the leading student exponent.

No student essay from the mathematical division had the impact on the seminar's reputation that Kirchhoff's did on the physical division. Most prize questions in mathematics were more pedestrian results of seminar teaching. In 1848 for instance, Richelot wrote the prize question in mathematics, one that required students to solve the most general case of "the rotation of a solid body about a fixed point, subject to no accelerating forces" using the "new methods discovered by the most illustrious [William Rowan] Hamilton and Jacobi" (Richelot 1848). In comparison to Neumann's earlier prize question which involved original research, Richelot's question asked for the application of mathematical methods to a specific problem and therefore was closer to an advanced seminar assignment (or advanced textbook problem in a later era). Both seminar premiums and prize questions in all disciplines died out in the 1880s or so when university enrollments grew, instruction became more formalized, and university instruction, especially in mathematics and the natural sciences, relied on canonical problems now found in textbooks that synthesized disciplinary knowledge. Unsurprisingly, around the same time students became less motivated to attend seminars, which metamorphosed into or were replaced by the larger disciplinary institutes that transformed German universities in the late nineteenth century (Cahan 1985; Schalau 1989).

4 Beyond Königsberg

Former seminar students Carl Neumann, Alfred Clebsch,
Oskar Emil Meyer, Georg Hermann Quincke.

Influence studies are fraught with difficulties in history, and even more so in the history of science and mathematics, two disciplines where disciplinary practices–including mentoring, joint publishing, the conferral of awards like the Fields Medal or the Nobel Prize, and the never-ending construction of intellectual genealogies down several generations–encourage the conferral of status merely by association, thereby suggesting influence. Nonetheless the intellectual profile and professional habits of many former seminar attendees bear the marks of their educational experiences, especially in the early stages of their careers. Even when we look at the publication records of secondary school teachers of mathematics and physics who attended the Königsberg seminar, we see the cumulative impact of how seminar topics and exercises from both divisions shaped the little research they could accomplish while teaching. From Gert Schubring's comprehensive bibliography of German school programs–programs that appeared annually and usually included an original publication by a member of the school's faculty–we find that 41 former seminar students published 77 works between 1834 and 1875. Only 16 of these 41 had a doctorate (Olesko 1991, 322). Using Schubring's assigned classifications, almost half (36) were in branches of mathematics, of which topics from higher geometry, higher arithmetic, analysis, and number theory dominated. These included, inter alia, works involving elliptic transcendentals, elliptic functions, hypergeometric series, the Theta function, and the mathematical representation of the Riemann P-function. The next largest group of publications was in mathematical physics (19), which covered problems in analytical mechanics that were clearly inspired by the mathematical division of the seminar (e.g., moment of inertia, the equilibrium of an elastic body of rotation, application of elliptic integrals to problems in mechanics) or by exercises assigned in Neumann's physics section of the seminar (e.g., analysis of instruments, the vibration of the aether in polarized light, the theoretical derivation of the law of double refraction in a two-axis crystal, Newton's color rings, remarks on the new theory of heat). The remaining twenty-two publications covered a wide variety of topics ranging from science and mathematics pedagogy to meteorology, botany, mineralogy, mathematical geography, chemistry, technology, and other related topics (Schubring 1986). Besides publishing in school programs, secondary school teachers also published in scientific journals, bringing the total number of their publications to about 100. Interestingly, journal publications in physics did not necessarily stem from access to equipment or laboratories. Not all teachers had the material resources to produce their own data, so Neumann generously either gave them some of his own or allowed them to use the data he had gathered in his own experiments or enterprises, like the Königsberg geothermal station he had established in the 1830s that was staffed largely by seminar students (Olesko 1991, 323, 332, 337, 348-60).

Practicing research while a secondary school teacher brought with it a certain degree of professional cognitive dissonance and so regret. These former students identified with the practice of mathematics or physics, but their workaday world was school teaching where their class hours–sometimes thirty or more–left little time for other pursuits. Of the more than one hundred or so who became secondary school teachers, very few moved on to positions where they could make greater use of their knowledge of mathematics and physics. Five became university professors after teaching secondary school; an additional seven became professors at technical or vocational schools (both in some locations were regarded as equivalent to secondary schools though). Four later worked in precision industries. Many of those who remained teachers used their specialized knowledge to improve mathematics and physics pedagogy. Ernst Schindler (1821-?) bemoaned the lack of time for research, the lack of instruments, and the absence of conversation with like-minded colleagues at one of his early positions, but eventually found a congenial environment at the Joachimsthal Gymnasium in Berlin where he hoped he could inspire students to study physics. Albert Wangerin (1844-1933), while teaching secondary school in Berlin, delighted in the scholarly stimulation that the Berlin Physical Society provided, but admitted that the demands of teaching gave him little time for following through with the ideas he had developed in the seminar. Gustav Louis Baumgarten's (1846-?) position at a Dresden gymnasium included use of a physical cabinet, which enabled him to continue his research. Mandatory textbooks in both mathematics and physics made it difficult for secondary school teachers throughout the German states to introduce much that was distinctive from their own training, but some ignored regulations and taught as they liked. Karl Johann Hermann Kiessling (1839-1905) thought it unjustified to use mechanics as an introduction to the study of physics because so many hypotheses had to be introduced. Instead, he considered the most appropriate introduction to physics to be through measurement and observation along the lines Neumann taught in the seminar. While teachers contributed little to the definition of mathematical or theoretical physics, they demonstrate how the intellectual culture of the seminar permeated secondary school teaching where they could cultivate enthusiasm for the study of mathematics and a mathematically-oriented physics. (Olesko 1991, 317-365).

Beyond Königsberg, former seminar students who worked in academic positions did the most to shape the definition and practice of mathematical or theoretical physics. A few examples will suffice. Among the mathematicians who practiced mathematical physics, consider Carl Neumann (F. Neumann's son) and Alfred Clebsch, best friends and classmates at the Alstädtisches Gymnasium in Königsberg where their mathematics teacher was Julius Heinrich Carl Eduard Schumann (1810-?), an auditor in Neumann's seminar in the late 1830s. Both entered the University of Königsberg in 1850. Although seminar attendance records are sketchy for the early 1850s, we know that Neumann registered for the physical division under his father for at least two years, 1851-1853, and for the mathematical division under Richelot, at least for a year. Clebsch participated in both divisions for at least one semester more, the winter semester of 1850/51. Both must have advanced quickly through the seminar material because they petitioned F. Neumann and Richelot to hold special sessions for them outside the seminar; both C. Neumann and Clebsch produced independent albeit unpublished mathematical investigations as a result. Clebsch, whose doctoral advisor was F. Neumann, subsequently wrote his 1854 dissertation on the motion of an ellipsoid in a resisting medium using elliptic functions. In the same year he qualified as a secondary school teacher and submitted his dissertation for publication (Clebsch 1856). After teaching at Berlin secondary schools between 1855 and 1858 he landed a position at the Polytechnic in Karlsruhe. In 1863 he moved to the University of Giessen where he founded a mathematical seminar on the Königsberg model, sans the physical division. After 1863 he worked primarily on variational calculus, partial differential equations, elliptic functions, invariant theory, and algebraic geometry, where his concern for physical issues was minimal to non-existent (Olesko 1991, 425; Schubring 1992). His lecture course on analytical mechanics shunned the advanced mathematics used by Jacobi and Richelot, used only algebra and geometry, and was confined to applied topics of interest to the engineers at the Karlsruhe Polytechnic, for whom it was taught (Clebsch 1859a; Clebsch 1859b).

Carl Neumann received his doctorate under Richelot in 1855 with a dissertation on hyperelliptic integrals applied to mechanical problems (C. Neumann 1856). He cited as his teachers not only Richelot and the second mathematician at Königsberg, former seminar student Otto Hesse (1811-1874), but also Jacobi, whose work Richelot had included in his lecture courses and in the seminar. In the year before graduating, he prepared for the qualifying examination for secondary school teachers, which he also passed in 1855 with an essay on the application of elliptic functions to mechanics (Olesko 1991, 217). Because he did not become *Privatdozent* until 1858 at the University of Halle with a *Habilitationsschrift*–the second dissertation necessary for obtaining the legal right to teach at a university, the venia legendi–on the mathematical treatment of the polarization of light by electricity and magnetism (C. Neumann 1865a), it is likely that he taught secondary school between 1855 and 1858. (He would not have been permitted to hold university courses at Königsberg without becoming *Privatdozent*.) At Halle he offered courses in the winter semester on analytical geometry and chapters from mathematical physics, while in the summer semester he taught differential and integral calculus and the application of Bernhard Riemann's general principles to the theory of elliptic functions (Olesko 1991, 362-63). He later became full professor (*Ordinarius*) at Basel (1863), Tübingen (1865), and finally Leipzig (1868). In 1869 Neumann and Clebsch founded *Mathematische Annalen* expressly as a venue for publishing articles in weeks rather than years (it had taken two years for Clebsch's dissertation to appear in print). The journal quickly rose to be among the most prestigious in mathematics.

Both Neumann's and Clebsch's careers demonstrate the appeal of scholars skilled in attracting young students to mathematics by cultivating linkages between university and secondary school instruction. While at Basel, Neumann was obligated to teach mathematics six hours a week at the local *Pädagogium* (where a few years later the philosopher Friedrich Nietzsche would also teach while a philosophy professor at the university). In 1864 when he received a call from Breslau, he told Basel authorities that he would not accept the offer unless he could also teach physics at the *Pädagogium* in order to elevate physics instruction at the university in the same way he had done for mathematics. A year later, however, he admitted that this dual position proved onerous (despite a high salary) so he left this time not for Breslau, but for Tübingen. Nonetheless, he urged Swiss authorities to continue their promotion of the continuity of instruction between the university and the *Pädagogium* in order to improve the quality of university instruction. To that end, he recommended another Königsberg seminar graduate and his father's student, Paul du Bois-Reymond (1831-1889), who had taught for many years at a Berlin gymnasium and knew how to integrate the two levels of instruction (Neumann 1865). Neumann was known as a stimulating teacher with many thankful students, "especially among the older teachers [of mathematics] in Saxony" (Lorey 1916, 167), who appreciated the clarity of Neumann's lectures as a model for their own (Dürll 1926).

Clebsch was hired at Giessen partly on his ability to train future secondary school teachers (Olesko 1991, 425). When the Göttingen mathematician Bernhard Riemann died in 1866 at the age of forty, the university considered both Neumann and Clebsch as possible replacements. Göttingen's physicist, Wilhelm Weber, attested to Neumann's productivity in both mathematics and physics, praising his "significant talent [for taking up] new questions and standpoints in higher mathematics and the theoretical natural sciences." Weber also noted Neumann's talent for preparing younger students for the study of mathematics (Weber 1866). Clebsch was the successful candidate; he moved to Göttingen in 1868 where he taught for four years until his death in 1872. Neumann instead accepted a call to Leipzig in 1868, only to find a year later that he "had nothing to do in Leipzig . . . because southern Germany appears to be a lost cause for mathematics and mathematical physics (C. Neumann 1869)." His complaint was premature. His career thrived at the University of Leipzig where, starting in the early 1870s, he and his two colleagues in mathematics who had also attended the Königsberg seminar, Karl Vondermühll (1841-1912) and Adolph Mayer (1839-1908), successfully cultivated the subject. Of note: his courses on analytical mechanics and potential theory, his specialty, attracted Woldemar Voigt (1850-1919). Acting on Neumann's recommendation, Voigt went to Königsberg where he became F. Neumann's last doctoral student. Indicative of the fluidity of the terms "mathematical physics" and "theoretical physics" at the end of the century, Voigt's appointment at Göttingen in 1883 was designated as one in "theoretical (mathematical) physics" (Kultusministerium 1883).

Carl Neumann's persistent concern for the state of mathematical physics offers an opportunity to see how a former seminar student reflected on the philosophical meaning and conceptual foundation of mathematical physics, perspectives that were not cultivated in the seminar. One of Neumann's fortes was to demonstrate the linkages between different mathematical tools, especially ones that could be used to explore physical issues. He developed the potential function by using series expansions; introduced the logarithmic potential; expanded the use of the Dirichlet problem in potential theory and elliptic partial differential equations; and in general broadened the use of mathematical methods in electrodynamics, hydrodynamics, magnetism, and crystallography. His 1865 textbook on Abelian functions was the first to treat elliptic and hyperelliptic integrals using Riemann's work (C. Neumann 1865b; Lorey 1916, 136). He recognized the transformative nature of new discoveries–that they could destroy carefully erected conceptual structures–but believed the empirical work of physics was not the responsibility of the mathematical physicist whose function was not to explain, but to construct theory on the basis of the fewest number of unexplained principles, principles he initially identified as attraction and inertia (C. Neumann 1865a, 17; C. Neumann 1896, iii-vi). That this task sometimes proved intractable he attributed to insufficiencies in the development of ideas of space and time (C. Neumann 1865a, 32). He wanted to unite the separate areas of mathematical physics, but in the end doubted that all areas of physics could be reduced to mechanical principles because there was no clear path to doing so, a situation he analogized to the impossibility of finding "streets in a primitive forest" (Neumann 1893, iv). Yet for all of his originality, he rejected certain novelties in physics, such as the work of Michael Faraday and James Clerk Maxwell, and at times engaged in heated debates on contested formulations in physics, as he did with Hermann von Helmholtz on electrodynamics (Pulte 2004) and Clausius on the study of heat. In the end, C. Neumann's intolerance led to the demise of his influence on the field, despite his successes in training students, in developing mathematical methods, and in applying those methods to physics.

In contrast to Neumann and Clebsch, former students who identified with the physical side of mathematical or theoretical physics emphasized empirical results as laundered through the methods of an exact experimental physics. The purification of empirical results mattered more than the mathematical expression of theory. Oskar Emil Meyer attended only the physical division of the seminar for six semesters between 1856 and 1859 after abandoning the study of medicine at Heidelberg and Zürich. In 1857 he took up Neumann's prize question to the philosophical faculty: determine the viscosity of liquids by using methods developed by Charles Augustin de Coulomb (1736-1806). Meyer tried to solve the problem by working out a theory of the experimental apparatus in the hopes of modifying Coulomb. He theoretically analyzed his instruments and experimental protocol, and conducted an analysis of errors along the lines he had been taught in the seminar. Even though his measurements were wanting, he won the prize in January 1858. Having invested so much work in his prize essay, he decided to use it for his dissertation, but not before improving his data. After reworking the experiment again and still finding the data were lacking, he decided to turn it in for his dissertation anyway, which was awarded in 1860. He then separated the theoretical and experimental parts of his investigation and in 1861 submitted the theoretical part "essentially unchanged" to the *Journal für die reine und angewandte Mathematik*. Still, the shortcomings of the experimental part plagued him. Part of his experiment involved determining the viscosity of mixtures of salt solutions, but his observations lacked the accuracy that was needed and he couldn't determine if chemical changes in the solution had occurred. He asked Neumann to take similar measurements so that they could compare the two sets. Neumann did (Olesko 280-285).

Then other experimental concerns arose. Even though the experimental side of his investigation was unfinished, Meyer decided to present it to the Berlin Physical Society. It flopped. He later explained to Neumann that he "was not well understood because in mentioning partial differential equations, I assumed too much mathematics on the part of the audience." Lukewarm reassurances came from Philip Wilhelm Brix (1817-1889), a former seminar student in the audience who was Neumann's first doctoral student in 1841, who told Meyer that at least his thermometer calibrations were correct (Meyer 1861a). By later in 1861, Meyer thought he had sufficiently good data to feel confident sending it to the *Annalen der Physik*. But he found, on the basis of the reaction he received to his presentation, that he had to dilute the mathematical content. Berlin colleagues now understood him. What he did not do, however, was abandon the strategy he had learned in the seminar, which placed the calculation of error as the culmination of a physical investigation. He believed that observation was the only way to find the "law of viscosity," but he actually used hypotheses and analogies to guide his derivation. For instance, he viewed the coefficients of internal and external viscosity as analogous to internal and external thermal conductivity in Fourier's theory of heat. Other similar comparisons entered his framing of the experimental determination of viscosity. In the end his experiment, which he acknowledged could not be completely represented mathematically, was based on deft use of a hypothetico-deductive method. His corrections for constant errors guided the course of his investigation. They included corrections for: air resistance (modelled on Bessel's seconds pendulum investigation), the moment of inertia, the finite thickness of the disk, and other factors. Several trials and subsequent analyses later, he still could not free his experiment from the conditions of the apparatus and instruments, but his data fell reasonably within theoretical predictions, so he was satisfied. Only briefly in the course of this investigation did he discuss a theoretical issue, Clausius's view that molecules not only vibrated, but moved along a free path until a collision occurred. He was satisfied that his results suggested his method was better than others (Olesko 1991, 280-285).

Georg Quincke's experiences and investigative style were similar to Meyer's. He attended the physical division for three semesters in 1853 and 1854, then both divisions in the summer semester of 1856. In between he spent three semesters at Heidelberg, where he studied with Kirchhoff. He received his doctorate in Berlin in 1858 with a thesis on a Königsberg topic, the capillary constant of mercury. He is another example of how complicated the investigative style of the seminar could become (Olesko 1991, 241-253, 375, 391). At Neumann's suggestion, and after three semesters in the seminar, Quincke studied mechanics independently. The subject was too difficult for him to handle, so he returned to Königsberg in the summer semester of 1856 because Kirchhoff could not teach him what he needed: more mathematics. Ironically Richelot's seminar that semester was on analytical geometry for beginning students, but Quincke remained enrolled in both divisions of the seminar anyway. In 1856 he took up Kirchhoff's 1845 study of the distribution of electric current on a metal plate–whose origin was in seminar exercises–with the intention of changing experimental conditions (different size and composition to the metal plates) and also of improving the agreement between theory and experiment. He complained to Neumann that he was immersed in "very large and unpleasant numerical calculations," but stuck with the project because "the subject, on which two students of yours have worked, is interesting. . . . [It] is a new proof for the mathematical theory of the electric current" which to date had only been proven by Kirchhoff for a circular disk with two electrodes at the periphery (Quincke 1856a). Quincke compensated for more constant errors, accounted for the thermoelectricity generated by the heat of his hands, and produced more data than Kirchhoff had. He was unsatisfied with the way Kirchhoff had represented experimental values graphically because he suspected that Kirchhoff had idealized his data. So he set out to compare theoretical and experimental values more thoroughly and clearly, and indeed his graphs did render the difference between theory and experiment better than Kirchhoff had done. He appeared to be confident using interpolation in this study, but when he turned to determining the capillary constant of mercury, a topic that piqued his interest at Königsberg and that would become his Berlin dissertation, he expressed doubts about interpolation, and needed help from the mathematical side. "I am still much behind in the integration of the differential equations of the mercury drop," he wrote to Neumann. He explained that he had sought Clebsch's help, but "Clebsch has been sick, and therefore my study of elliptic functions has been neglected" (Quincke 1856b). His data proved consistently inconsistent. The standards of precision that Neumann held up in the seminar proved unattainable, so he worried that Neumann would not approve of his results. In the end his study of capillarity yielded nothing new, but only the limits of what was known. Like others Quincke was caught up in the pursuit of precision. Despite his patchwork education at three German universities, he considered his closest intellectual affiliation to be with Neumann, writing to him in 1861: "I have always striven to follow the path which you laid out for me . . . because I was so happy to hear your lectures and to be your student. Unfortunately, my abilities have not allowed me to improve my mathematical resources as much as I would have liked. But at least I try, as best I can, to schlep slowly along" (Quincke 1861).

There was more than one deleterious consequence to former seminar students' obsession with the quality of their data and with conducting exhaustive analyses of constant and accidental errors. The search for a flawless experiment was inhibiting. As students became skilled at discerning errors, the gap widened between the mathematical expression of a law and the numerical data of experiment. By training, and then by choice, former seminar students tried to fill that gap with the never-ending analysis of errors. As Bessel claimed in 1844: "the task of the present-day art of observation" was "to eliminate the apparatus from the results" (Felber 1994, 73). Bessel meant that instruments and apparatus had to be expressed analytically, in mathematical terms, so that their influence on the experiment could be expressed quantitatively. Hence when Meyer remarked that a theory and its assumptions were confirmed only "to the limits of the accuracy of observations," he meant that the worth of a theory was measured by the size of its experimental errors (Meyer 1861b, 235). Since the focus of an investigation was on measured data, interpolated data were problematic for former seminar students (as well as for Neumann) because, strictly speaking, they had not been measured. Although Meyer interpolated values to reduce his observations to the same temperature, he called Poiseuille's Law (the relation between the drop in pressure of a fluid in a tube with the area of the tube and the fluid's viscosity) an "interpolation formula" because the temperature range in Poiseuille's original experiments was too small (Olesko 1991, 383). Former students from both sides of the divide in mathematical physics, Kirchhoff and Carl Neumann, found themselves embroiled in controversy with opponents who had used interpolated data (Olesko 1991, 383-85, 387).

5 Epilogue

Richelot died in 1875. Neumann retired in 1876. Neumann's last doctoral student Woldemar Voigt (1850-1919) assumed Neumann's position and ran the physical division of the seminar. When Voigt left for Göttingen in 1883, his student, Paul Volkmann, was offered the position in theoretical physics, which he occupied for thirty years. Richelot's position in mathematics was occupied first by former seminar student Heinrich Martin Weber (1842-1913), who had attended the seminar in the mid-1860s. In 1883 Ferdinand Lindemann (1852-1939), a student of Felix Klein, replaced Weber; Lindemann became the first non-seminar student to direct the mathematical division of the seminar. Before departing in 1893, Lindemann mentored the doctoral dissertations of David Hilbert (1862-1943) and Hermann Minkowski (1864-1909). Hilbert and Minkowski became close while at Königsberg, where they befriended the *Außerordinarius* in mathematics, Adolf Hurwitz, in 1884. Hilbert and then Minkowski took over Lindemann's position very briefly in the mid-1890s; both left after just a few years. Prominent names among the physics students at the end of the century included Arnold Sommerfeld (1868-1951), who earned his doctoral under Lindemann, not Volkmann.

Relations between the physical and mathematical divisions of the seminar were strained by the end of the century. Volkmann opposed theoretical physics becoming a mathematical subdiscipline and his physical exercises were a shadow of what they had been decades before. The exact experimental components of mathematical physics as practiced at Königsberg fell by the wayside as a powerful experimental physics developed. By 1937 the mathematico-physical seminar that Neumann and Jacobi had established over a century before became one for mathematics alone. It did not survive the Second World War.

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Biographical Sketch

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Most of her publications can be found here:

https://www.researchgate.net/profile/Kathryn-Olesko/research

Volodja Zakharov: the Scientist, the Poet and the Man

by Alan C. Newell

It is no accident that mathematics and poetry rhyme. Each, using its own language, seeks to strip away the superfluous from some underlying notion or set of ideas and, in doing so, reveal universal truths. And if the truth can also strike the chord of beauty, then so much the better. It becomes a joy forever.

Volodja Zakharov was a man who excelled at both. But he was so much more. He was an honorable man of enormous integrity who stood up to bullies and demagogues and purveyors of pseudoscience nonsense and spoke truth to power, stands that often cost him great personal freedoms.

He was a good friend for more than forty years.

In the next fifty minutes or so, I hope to be able to pass on to you an appreciation of this remarkable individual, his contributions to science, his love of literature and his rock solid integrity.

At the same time, I know Volodja would not begrudge us remembering in passing two other fallen pioneers of the soliton world, David Kaup and Hermann Flaschka, each of whom he greatly admired.

There were four landmark developments in the nonlinear sciences in the latter half of the twentieth century, wave turbulence, solitons and integrable systems, collapse singularities and chaotic dynamics. Volodja Zakharov played a central role in breakthroughs in three of them. Indeed these three featured in the titles of all the Kiev and follow on conferences held in Russia from the 1980s to the 2020s.

Wave turbulence

Turbulence is about understanding the long time statistics of irregular flows and in particular about being able to calculate transport properties; for examples, how much water goes down a pipe per second as a function of pressure head or how much energy is transferred from large scales where it might be injected into a flow to small dissipation scales where that energy is removed? In the thirties, Taylor tried to suggest an easier task and introduced the concept of homogeneous turbulence, in which correlations between many points depend only on their relative geometry. One would then form, from the Navier-Stokes equations, equations for the evolutions of the statistical moments or cumulants. But, because the equations were nonlinear, the resulting hierarchy, the BBGKY hierarchy of cumulant equations, was unclosed; the rates of change of second order cumulants depended on third and so on. All efforts to effect a consistent closure failed. In his 1958 lecture notes, Saffman summarized the efforts to that time with the quote from Macbeth 'full of sound and fury, signifying nothing'. Not too much has changed since.

There was one success, however: the Kolmogorov spectrum, which was analytically reasonable and experimentally observable. Kolmogorov suggested that there should be a solution of the hierarchy that corresponded to a constant flux transport of spectral energy $E(k)$ from the large scales at which energy was inserted to the much smaller dissipation scales. Dimensional analysis then suggested that such a spectrum should satisfy $E(k) = cP^{2/3}\ k^{-5/3}$, where P is the constant dissipation rate and k the local wavenumber.

In contrast to the absence of a consistent closure of fully developed homogeneous turbulence, wave turbulence, the longtime behavior of a sea of weakly nonlinear, dispersive, random waves has a natural asymptotic closure. The reasons are fairly simple. In any patch of the sea surface, linear propagation carries waves from distances at which the surface elevations are uncorrelated so that via the central limit theorem and mathematically by the application of the Riemann-Lebesgue lemma, the sea surface in that patch relaxes to a joint Gaussian distribution. But, because the equations are weakly nonlinear, on longer time scales the third order and higher order cumulants, which have relaxed to zero under the linear propagation, are regenerated. The secret of the natural closure is that, in this regeneration which involves the higher order moments and cumulants, it is the products of the lower order moments that dominate the dynamics. For example, in the regeneration of the third order cumulant by a fourth order moment by, say, a quadratic nonlinearity, the decomposition consists of two parts, a fourth order cumulant and a product of two second order ones. The latter dominate the long time dynamics. Closure is achieved and leads to a kinetic equation for the spectral particle density $n(k,t)$, the Fourier transform of the two point correlation representing energy divided by the frequency w. For ocean gravity waves, one has to proceed to a higher order and, in the decomposition of sixth order moments, it is the product of three second order cumulants that dominate. The result is a kinetic equation for the Fourier transform $n(k)$, the waveaction (particle) density, of the two point function

$$\frac{dn(k,t)}{dt} = T[nk] = \int L^2\ n\ n_1\ n_2\ n_3\ \left[\frac{1}{n} + \frac{1}{n_1} - \frac{1}{n_2} - \frac{1}{n_3}\right]$$
$$\delta(k + k_1 - k_2 - k_3)\ \delta(w + w_1 - w_2 - w_3)\ dk_1\ dk_2\ dk_3$$

plus a nonlinear frequency modulation. The nature of the transfer term captures the mechanism for transfer, namely, in this case, four wave resonances by which energy and number density are shared throughout the wave-vector spectrum.

Many folk were involved in the derivation, starting with Hasselmann, and followed by contributions from Benney, Saffman, Zakharov and myself. But it was Zakharov who figured out the long time, relevant statistical steady state. All of us in the West concentrated on the obvious equipartition, thermodynamic solution; $n = T/(b + \vec{a} \cdot \vec{k} + w)$, $w = \sqrt{gk}$, with T the temperature and b the chemical potential corresponding to equipartition of number density or spectral energy despite the fact that the observational evidence taken from sea surface data suggested a spectrum closer to k^{-4} and $k^{-9/2}$. And it is doubly surprising that those dealing with turbulence theories for fluids should not have been aware of the role of the Kolmogorov finite constant energy flux P solution $E(k) = (P)^{2/3}\ k^{-5/3}$ and guessed that somehow there must be an analogue in wave turbulence to that and to the corresponding inverse energy flux in two dimensional fluids. In two dimensions, there are two motion constants for the Euler equations, energy and mean squared vorticity and the former gives rise to an inverse flux and the latter to a direct flux. And for water waves, there are also two formal motion constants, total waveaction or particle number and total energy, and so one might have surmised that there would be Kolmogorov like solutions to the kinetic equation corresponding to each invariant.

But only Zakharov seemed to have thought of this and, using ingenious methods and the exploitation of non-obvious symmetries, he found solutions corresponding to both a finite waveaction flux Q and energy flux P. The latter behaved as k^{-4}, close to what was being observed for ocean waves. For almost twenty years, Volodja had been a lone and frustrated voice crying in the Soviet wilderness advocating for these solutions. Those in the West had moved on and were more interested in other things. The oceanographers had even adopted alternative arguments for ocean wave spectra which were not based on the Hasselmann kinetic equation. (These may turn out to be relevant after all and can be understood in the context of a kinetic equation with dissipation added.) It was not until the early nineties that Zakharov's solutions were recognized as being correct and furthermore relevant not only for water waves but also for plasma waves, elastic waves, Alfvén waves, Rossby waves (for which there are three finite flux solutions), indeed for any weakly nonlinear dispersive wave system. Because of their connection with Kolmogorov's work, these finite flux solutions are now known as KZ or Kolmogorov-Zakharov solutions. For this work and in particular for his discovery of inverse flux solutions, he was awarded, along with Kraichnan, the Dirac Medal in 2004.

Solitons and integrable systems

The first time I recall registering Zakharov's name was at the Potsdam NY conference on Nonlinear Waves in 1972 sponsored by AMS and SIAM, the two mathematics societies. We had brought a stellar line up of principal speakers together, Brooke Benjamin, Dave Benney, Martin Kruskal, Gerry Whitham and we had left the afternoons free for serendipitous informal sessions. During one of these, Fred Tappert told us about a new paper by two Russians, Zakharov and Shabat, purporting to solve what Tappert then called the Benney-Newell equation (that moment of fame was fleeting) but which became known later as the nonlinear Schrodinger (NLS) equation. The solution method followed the same prescription, later called the inverse scattering transform or IST, as Gardner, Greene, Kruskal and Miura (GGKM) had used to solve the Korteweg-deVries equation. Namely they had found a "Lax" pair, L and B, such that the NLS equation could be written as $dL/dt = [B, L]$ with antisymmetric B which guaranteed that the spectrum of the operator L, which depended on the solution $u(x, t)$ of the NLS, remained invariant as one of its coefficients, $u(x, t)$, evolved (deformed) according the NLS equation. Associated with the operator L is a scattering problem where waves from $x = +\infty$ are partially reflected from and also transmitted to $x = -\infty$ through the graph of the space-time dependent solution of the nonlinear equation. There is thereby a map, called the Inverse Scattering Transform (IST) from the solution of the nonlinear equation to the scattering data and any associated bound states. Zakharov, a great fan of Hamilton[1] and canonical transformations, had previously shown with Faddeev that the IST, the transformation that Kruskal et al. had used to solve the KdV equation, was simply a canonical transformation to action angle variables where the action variables were encoded in what became known as the transmission coefficient $a(k)$ of the associated scattering problem, a motion constant for integrable systems. The angle variables which, for integrable systems, vary linearly with time, were the phases of the reflection coefficient. The bound state eigenvalues, related to the zeros of the transmission coefficient, corresponded to solitons, fully nonlinear localized stable pulses that often dominate the solution field.

What was going on? Somehow, hidden in these two equations there were new sets of coordinates, the action-angle variables, whose time evolution was trivial. But how could one find what all these hidden symmetries were and then how could one then find the right coordinates in which the equations became so simple. IST or the Inverse Scattering Transform is how.

The Zakharov-Shabat work had monumental consequences. For one thing, it showed that the GGKM scheme for KdV was not a fluke, a one off as had been surmised. It was simply one of a class of exactly integrable partial differential equations which turn up many times in describing the physical world. KdV is the universal equation describing the propagation of weakly dispersive, weakly nonlinear waves along characteristics of some underlying hyperbolic system. Among its many applications are shallow water waves and the continuum limit of the famous Fermi-Pasta-Ulam problem. NLS is the canonical equation describing nonlinear wave envelopes of weakly nonlinear, strongly dispersive systems and occurs in many contexts from deep water wave envelopes to packets of light waves. Very soon after the Potsdam meeting, and stimulated by noting that a Bäcklund-like transformation Kruskal had suggested for solutions of the sine-Gordon equation could be linearized to give almost the Zakharov and Shabat operator L, AKNS solved the sine-Gordon equation and introduced whole classes of integrable systems which could be solved by IST. Almost overnight, the number of integrable systems exploded from a small number, which literally could be counted on one hand, for examples the two body problem and certain spinning tops, to infinite sets of pde's. Soliton factories sprang up all over the world! And there was no factory more important and successful than the Zakharov School, Kuznetsov, Shabat, Rubenchik, Manakov, Mikailov, Falkovich, Gabitov, Dyachenko, Pushkarev, Nazarenko and Turitsyn, Isospectral, and their first cousins, isomonodromic deformations (which are relevant for analyzing self-similar solutions of soliton equations), following the pioneering works of Sato, Miwa and Jimbo (Hermann Flaschka and I also had a useful paper on the subject) became one of the hot topics of researchers in the seventies and eighties.

[1]Volodja very much admired Hamilton and religiously cast all problems in dynamics on which he worked into their Hamiltonian formulation whenever possible. But he was a better poet than Hamilton. Hamilton was a wannabe poet and often communicated various poems he wrote to his great friend Wordsworth. Lore has it that Wordsworth, in as kind a manner as possible, advised Hamilton to stick with his day job.

Collapse and singularity formation

At about the same time, Zakharov introduced another idea, the notion that certain equations of NLS type in dimensions greater than one, can have localized, "soliton" like solutions that become infinite, "collapsed", in finite time. Indeed in the context of light physics, a linear subject until the advent of the laser which allowed for intensities sufficiently large that nonlinear effects were measurable, a light wavepacket envelope evolves according to the NLS equation in materials where the refractive index is intensity dependent. If the refractive index increases as the light intensity increases, then one can imagine the following situation. Consider a constant amplitude or intensity light front is perturbed so that locally the intensity is increased. Its refractive index increases at that spot and thereby attracts more light to that region which again amplifies the intensity. In one dimension, the growth is balanced by diffraction and leads to soliton formation but in two and higher dimensions, diffraction is not sufficient to arrest the growth which becomes unbounded. One can turn the equation into hydrodynamic form by a simple transformation and, in this form, the corresponding Bernoulli equation indicates that the relation between pressure and density is such that, as the density decreases, the pressure increases, a certain recipe for growth. Locally the light intensity can become so great so as to crack glass vessels. With a cubic nonlinearity, the nature of the collapse becomes more and more violent the higher the spatial dimension. In three dimensions, all the light would get sucked into the collapse, a veritable black hole. Collapses and the way they can occur offer an analogue for approaching the challenge of dark energy in which we also have some universal substance between galaxies of ordinary matter whose pressure increases as its density decreases leading to an ever accelerating expansion of the universe. In some sense, the light collapse in which all the energy and the expanding universe are similar except that the time evolves in opposite directions. The reversal of the expansion would be the big crunch, the black hole. The detailed analysis of collapse is still a challenging subject to this day and, again, it is the students of Zakharov, and in particular Pavel Lushnikov, who have led the charge towards a more thorough understanding. Although much progress has been made in each of these three areas, I would not want to leave the impression that their stories are over. Far from it! There remain many open questions and challenges. But those are stories for another day.

The man

I first met Zakharov in 1979 in a meeting in Kiev that the two Academies, Russian and American, had organized. The line-up was stellar in that almost all of the leading scientists in Russia at the time were present. He was genuinely delighted to meet the AKNS four as we all had been friendly rivals for the preceding decade. I registered the impression he made on me in a book on Solitons that I wrote shortly thereafter.

'Before I leave this section, I want to tell you a bit about a giant in the field, V.E. Zakharov. He has contributed in so many areas: the Zakharov equations of plasma physics, his papers with Shabat outlining for the first time a general prescription for handling Lax pairs in both one and higher spatial dimensions, his work on the selffocusing singularity, and his papers on the dressing method for building hierarchies of solutions. He seems to have the knack for being the first at getting to all the good problems. He is a genius, brilliant and intuitive. A wild bull of a man of great good humor and appetites, he has a deep and abiding love for poetry and literature as well as physics. On one of our meetings in the hotel Ukraina bar, he recited and acted out with great relish the opening scene ("when shall we three meet again?") from Macbeth. You will often come across his name.'

Thus began a friendship that has lasted for more than forty years.

I met him again in 1983 at the Kiev conference at which all the Soviet luminaries were present, Zeldovich, Gelfand, Sagdeev, Zakharov, Arnold, Sinai, but which was poorly attended by Western colleagues because of the rather fraught relations brought about by the downing of the Korean airplane which had strayed over or close to Soviet territory. There was initially much tension in the air but it rapidly dissolved under the joint influences of the science, the drink and the brotherhood that developed. I got to spend a lot of time with Volodja at that meeting and, on several occasions, when we took long walks, caused considerable consternation among the minders (KGB guys who were attending as participants). I learned then of his many scrapes with the controlling regime. Because of his stand against the invasion of Czechoslovakia in '68, he had been denied permission to visit countries outside the Soviet block and enjoyed very few visits even within the block. How small were the minds of the authorities! Volodja was a man of enormous integrity who would never betray his beloved Russia. He was Russian to the core. He treasured its many good qualities, the science, the arts, the companionship of close friends but he was never shy about speaking truth to power. And for that he paid a price. I also learned at that time of his great love for the outdoors and for the mountains of central Asia.

I also began to understand why our Russian friends treasured their science and their arts, the company of close and trusted friends with whom they would spend many hours around the kitchen tables sipping vodka and simply talking without fear. It was a way to escape the oppressive and stifling atmosphere of the political system.

Finally, in the mid to late eighties, a more enlightened regime ruled Russia but it wasn't to last. Sadly, after a decade of personal freedoms and instability caused by an inability to deal with the new freedoms and the ensuing chaos, the period of personal freedoms ended. Putin came to power. Recent developments were to be a great disappointment to Volodja as they have been to many of our Russian colleagues. "Plus ça change, plus c'est la même chose."

He finally made his first trip to the West to visit along with a Soviet delegation the Santa Fe Institute in 1987. I recall Khalatnikov, his boss in the Landau Institute in Chernogolovka at the time, wagging his finger at me in warning to be careful when we took off one day to explore the wonderful desert landscapes of New Mexico. Volodja liked the open spaces. We enjoyed ourselves and stayed out of trouble.

After the fall of the Soviet Empire, he came again to America, to Chicago, at the invitation of Leo Kadanov. Shortly thereafter, he accepted a half time Professorship at Arizona and brought with him a team of student colleagues, Sasha Dyachenko, Andrei Pushkarev, Vladimir Shvets and later Sasha Balk and Sergey Nazarenko. They had an enormous impact on the intellectual life of the department. He became a full time Professor at Arizona after he stepped down from the Directorship of the Landau in about 2000. He became a Regents Professor in 2007. He finally retired from the University of Arizona in 2021 and then was invited to take up a post at the new Institute, Skoltech, in Moscow. The Russian invasion of Ukraine has led to his resignation from that position.

Volodja and his family, Sveta, Alexei, Dima (Ilya was already married with a son and lived in Moscow) became active Arizonans and eventually took American citizenship along with their Russian citizenship. Sveta and Volodja bought their present house in the lower foothills in part because of the wonderful friendship they developed with Brendan Phibbs and his wife Leana. Brendan was a great man, a pioneer heart surgeon and expert, a prize winning author of considerable stature and a wannabe Irishman who was one of the dominant and larger than life figures in the medical and literary worlds of Tucson.

In recent years, Volodja took great delight in his grandchildren and enjoyed sharing with each of them the joys of childhood.

There are many other stories I could tell, involving long generously lubricated dinners in our respective houses, Volodja as a dab hand at lamb stew, adventures in Denmark, to broken legs in Tucson due to a collison with a car on 5th Street after which Fortov suggested, tongue in cheek, that its location would be an ideal place for a memorial statue. I remember so many adventures in Tucson and in Moscow and Chernogolovka and in particular a trip in 1991 to Georgia and the Caucasus mountains, full of feasts, friends and Kindzmarauli wine. I also remember a dead of night, pitch dark drive back to Chernogolovka from their house on the Volga without any headlights through bible black, narrow roads, clinging to the wakes of large trucks for light and guidance. Several times we lost power and were restarted with a push from local bandits. And to cap it all, the gear stick came adrift from its moorings just as we entered the parking lot of their apartment building. But these are stories the details of which are best suited for another day and well lubricated and less critical audiences.

Poetry

Volodja loved his poetry and indeed could recite thousands of poems, often without any prompts and/or encouragement from his audience. Once in poetic mode, his voice (he was not a natural singer) would take on a wonderful cadence, rising and falling in theatrical fashion. His lifelong muse was Sveta who matched him and sometimes even bettered him in recalling certain poems and certainly in recalling any musical works. Volodja was not into music. Sveta also helped him with his science. Many works would have remained unpublished but for her contributions. He published a book of poems which was translated into English as “Paradise of Clouds” and his and Sveta’s great work was their anthology of six volumes on ancient Russian poetry. His photograph on the front of Volume Number Four was taken in Ireland.

His poems were often quite simple. He liked his “Bumblebee.” Although the poem I quote now is not great verse, it holds a great truth. First what is in “Paradise of Clouds” and then another translation.

Never seen in any seedy places,
Pure mathematician Dr. Hardy
Liked to walk a college lawn in Cambridge
With Ramanujan-but much more often
On his own, in lengthy ruminations
On the numbers: prime and also perfect.
One world war was followed by another,
Epochs came along and tumbled over,
But prime numbers are as prime as ever,
And, my friend, the perfect ones are holding
Every bit of their divine perfection.
Truly there are sturdy things in nature!

In these days of great uncertainties and change, there are so few constants. Indeed, from recent observational data we cannot know for sure that even the laws of physics are not changing. It is therefore indeed comforting to know that at least there are some things that stay constant. We salute you, Volodja, for reminding us of that truth and for all the other truths you have taught us. Goodnight old friend. Although greatly missed, you will be also greatly remembered.

A second translation by Brendan Phibbs:

He did not waste his time in dives,
Doctor Hardy, pure mathematician.
On green lawns of Cambridge,
He ambled, together with Ramanujan.
Or by himself, and thinking mostly of the numbers,
The primes and the perfect numbers.

The First World War, the Second,
New epochs rose and fell,
But, the prime numbers are still prime,
And the perfect numbers, my friend,
Lost none of their divine perfection.

Some things are stable in this world.

XXIst International Congress of Mathematical Physics in Strasbourg: The registration is open!

We are excited to announce that the XXIst International Congress of Mathematical Physics (ICMP) will take place in **Strasbourg, France** from **July 1st to July 6th 2024**. It will be directly preceded by the Young Researcher Symposium (YRS) which will take place from **June 28-29th 2024**, at the **University of Strasbourg**.

The **registration for both events is open now!** We invite you to register as soon as possible to benefit from our early-bird discount on registration fees. IAMP members in good standing will also get a discount on the fees.

Program of the ICMP: It is our pleasure to announce that the ICMP 2024 will have keynote lectures from 16 distinguished plenary speakers:

- Scott Armstrong (New York University)
- Jaqueline Bloch (Université Paris-Saclay)
- Jian Ding (Peking University)
- Vojkan Jakšić (McGill University) & Claude-Alain Pillet (Université de Toulon)
- Karol Kozlowski (ENS de Lyon)
- Eugenia Malinnikova (Stanford University)
- Phan Thành Nam (LMU Munich)
- Hermann Nicolai (MPI for Gravitational Physics, Potsdam)
- Leonid Parnovski (UCL, London)
- Daniel Remenik (Universidad de Chile)
- Steve Shkoller (UC Davis)
- Maryna Viazovska (EPFL, Lausanne)
- Michael Walter (Ruhr Universität Bochum)
- Lauren Williams (Harvard University)
- Jiangong You (Nankai University)
- Maciej Zworski (UC Berkeley)

The plenary talks of the ICMP 2024 will be complemented by 12 thematic parallel sessions, with 6 talks per session, and 120 contributed talks covering a diverse spectrum of state-of-the-art topics in modern mathematical physics. The conference will also include a public lecture by Serge Haroche (Collège de France, Nobel Laureate 2012) and various prize announcements and talks from laureates: Henri Poincaré Prize, Early Career Award, as well as AHP, IUPAP and JMP prizes.

Program of the YRS: The Young Researcher Symposium aims to offer a unique opportunity for young scientists to present and discuss their research in an international scientific context, gain visibility and discover new perspectives and ideas from senior researchers.

We are excited to announce that the YRS will offer 2 thematic mini courses held by

- Bernard Derrida (Collège de France and École Normale Supérieure)
- Laure Saint Raymond (Institut des Hautes Études Scientifiques)

Additionally, the YRS offers 4 parallel thematic sessions with about 50 contributed talks.

Registration and practical information: To find out everything about the ICMP 2024 such as information about the program, registration, abstract submission, and other practical issues, like access and accommodation, we kindly invite you to visit the website of the conference

`https://icmp2024.org`

which will be frequently updated with any important developments and news.

We would like to highlight some (limited) funding opportunities available for PhD students, postdocs or other participants who cannot secure sufficient funding from other sources and would otherwise be unable to participate. To be considered for funding, please apply as soon as possible through the following link: `https://icmp2024.org/support.html`.

We hope to see you all in Strasbourg in June/July!

For the local organizing committee:

Nalini Anantharaman, Semyon Klevtsov, Clément Tauber and Martin Vogel.

The Ladyzhenskaya Lectures

by Darya Apushkinskaya and Alexander I. Nazarov

In the second half of the XX century, the progress in the theory of partial differential equations and mathematical hydrodynamics is deeply associated with the name of outstanding Russian mathematician Olga Alexandrovna Ladyzhenskaya (1922-2004). She occupies a very special place in the history of mathematics and mathematical physics in St. Petersburg, Russia, and worldwide.

In 2022, several remarkable events were dedicated to the centenary of Ladyzhenskaya. The Ladyzhenskaya Medal in Mathematical Physics was established to recognize revolutionary results in or with applications to mathematical physics. The first medal was awarded to Svetlana Jitomirskaya (University of California, Irvine, USA). Among the other events, we mention the Ladyzhenskaya centennial conference on PDEs, which was held in July 2022 in St. Petersburg in a mixed format.

Also, the annual Ladyzhenskaya Lecture has been established by the Euler International Mathematical Institute, St. Petersburg Mathematical Society and St. Petersburg seminar on Mathematical Physics named after V.I. Smirnov.

Lectures take place in early March in a mixed format (online/in presence) at one of the meetings of the St. Petersburg seminar on Mathematical Physics. Their topics include PDEs and related issues.

The first Ladyzhenskaya Lecturer was Professor Ari Laptev from Imperial College London, UK. On March 6, 2023, he gave the lecture “A survey on current results in Theory of Lieb-Thirring inequalities”.

Professor Susanna Terracini from University of Torino, Italy was elected the Ladyzhenskaya Lecturer 2024. She will give her lecture on March 11, 2024.

Call for bids to organise the next EMS-IAMP Summer School in Mathematical Physics

by Kasia Rejzner and Jan Philip Solovej

The call is open to submit bids for the next summer school in mathematical physics organized with support from EMS and IAMP. The last one took place in 2022 and we would like to have the next one in 2025.

The school should be one-week long and intended for PhD students, postdocs, and young researchers. It should include 2-4 advanced graduate-level courses that will introduce some important techniques relevant for modern mathematical physics.

If you are interested in hosting the school, please submit a single PDF file (1-2 pages) containing the following information:

- a description of the planned event and of its scientific scope
- provisional dates and location of the event
- scientific committee (if there is one) and local organising committee
- a (provisional) list of courses and who will give them
- the expected number of participants
- a financial plan, detailing other sources of support and expected expenses.

Applications should be sent to Kasia Rejzner (president@iamp.org) and Jan Philip Solovej (vicepresident@iamp.org).

Time's Arrow

Scientific anniversaries

1824. Nicolas Léonard Sadi Carnot published his theory of the efficiency of heat engines in his book *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance*.

1924. Hermann Weyl published *Was ist Materie?* Die Naturwissenschaften 12:561-568, 585-593, 604-611.

1974. Stephen W. Hawking published his article *Black hole explosions?* in Nature 248, 30-31.

1994. Peter Shor discovered his algorithm for factorization of large integers on a quantum computer.

Personal celebrations

Yvonne Choquet-Bruhat turned 100 on December 29, 2023.

Lost luminaries

James W. York (1939-2023) passed away on December 17. He received the Dannie Heineman Prize for Mathematical Physics in 2003 for his contributions to General Relativity.

Readers are encouraged to send items for "Time's Arrow" to salmhofer@uni-heidelberg.de.

News from the IAMP Executive Committee

New individual members

IAMP welcomes the following new members

1. Prof. Christopher Fewster, University of York, UK
2. Prof. Ryan Grady, Montana State University, USA
3. Dr. Daan Janssen, University of York, UK
4. Mr. Anouar Kouraich, Technical University of Munich, Germany

Recent conference announcements

Open Communications in Nonlinear Mathematical Physics - 2024

June 23 - 29, 2024, Bad Ems, Germany.

Venice 2024 - Quantissima in the Serenissima V

August 12 - 23, 2024, Venice, Italy

Open positions

For an updated list of academic job announcements in mathematical physics and related fields visit

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

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