IAMP Bulletin
April 2026

International Association of Mathematical Physics

Contacts. http://www.iamp.org and e-mail: bulletin@iamp.org

Cover picture: Erwin Schrödinger. Courtesy of *Österreichische Zentralbibliothek für Physik*.

ISSN 2304-7348
Bulletin (International Association of Mathematical Physics)

International Association of Mathematical Physics Bulletin, April 2026

Contents

Schrödinger's Invention of Wave Mechanics[1]

by Norbert Straumann

In the first half of the year 1926, in six publications [1, 2], Schrödinger single-handedly invented wave mechanics and developed it to an astonishing degree. The significance and intensity of this creative outburst has scarcely a parallel in the history of natural science.

In these months, Schrödinger changed physics and opened up new directions for it in a way that only few can aspire to. By around 1960 already more than 100,000 scientific publications had built directly on his work. Just counting how often authors in this auditorium have used the term *Schrödinger equation* in their publications would already give a substantial number.

Schrödinger's articles are not only important, but also written in a delightful and elegant style, and I would like to try to convey to you some of the pleasure that I had upon reading them. His own amazement and joy of working are also reflected in the letters he exchanged with others in that period, and I will give you samples of this, too. (I would like to take this occasion to thank Karl von Meyenn for the letters, which he has kindly made available to me. He is planning to edit Schrödinger's correspondence after having completed his Pauli project [3].)

Schrödinger was well-equipped for his pioneering work in every respect. One gets the impression that — questions of interpretation aside — most of it simply fell into his hands. A letter to Wilhelm Wien, dated February 22, 1926, illustrates this very nicely [4], reading:

> Time flies. Every second or third day brings another small novelty – it is at work, not I myself, and this "it" is the gorgeous classical mathematics and Hilbert's mathematics, the wonderful edifice of eigenvalue theory. They expose it all so clearly that one can simply take it without any worries, and the right things will appear by themselves when needed. I am so happy to have escaped all this terrible mechanics with its action and angle variables and its perturbation theory that I have never really understood. Now everything has become linear and superposable, calculations are really easy and convenient, as in the old acoustics. And perturbation theory is no more involved than the forced vibrations of a string.

Because matrix mechanics had already been in place for a few months, I would like to quote from that same letter also parts of the section with the subtitle "Relation to Heisenberg." It makes clear how important – indeed, productive – preferences and aversions can be also in research:

> I join Geheimrat Sommerfeld in believing in an intimate connection. It must, however, be profound because Weyl, who has studied Heisenberg's theory very thoroughly and even further developed it himself, and whom I asked to read my first manuscript, said that he did not know how to find that link. After that I have given

[1] This article is based on a lecture at the symposium *SCHRÖDINGER'S WAVE MECHANICS – 75 YEARS AFTER* at the University of Zurich, April 24–25, 2001, first published in German as arXiv:0110097. Translated by M. Salmhofer.

> up looking for it myself, all the more willingly as I found the matrix calculus unbearable long before I even remotely thought about my theory — with this I would like to say that it is not my preference for my own brainchild that makes me refuse to recognize anything else. Of course I now sincerely hope that matrix calculus will again vanish, after its valuable results have been absorbed into eigenvalue theory. I think I am being honest to myself when I say that I would no less dearly wish this to happen if the eigenvalue theory had originated with some Mr. Schultze. Indeed I cringe at the mere thought of having to present to a young student matrix calculus as the true essence of atomic theory.

Prehistory

Before I turn to Schrödinger's series of papers about wave mechanics, let me make a few remarks about the historical context.

In his fourth paper "Über das Verhältnis der Heisenberg-Born-Jordan'schen Quantenmechanik zu der meinen"[2], Schrödinger writes in a footnote *My theory was inspired by Louis de Broglie (...) and brief but infinitely-far-reaching comments of Albert Einstein (...).* In his wonderful book about Einstein [5], Abraham Pais has written about Einstein's contribution to the birth of wave mechanics. I have to restrict to the most essential things here.

Einstein received a copy of the dissertation of de Broglie from Langevin, and he was impressed. In December 1924, he wrote to Lorentz [6] :

> I believe that this is a first feeble ray of light that might illuminate this, the worst of our physics enigmas. I have found much that supports his construction.

Following Bose's new derivation of Planck's radiation formula (based on the indistinguishability of photons), Einstein wrote three papers about quantum gases. The best-known today is his derivation of Bose-Einstein condensation, which has been back at the center of attention for a few years. But more fundamental is his investigation of fluctuations in a quantum gas [7], in close analogy to his work from 1909 about fluctuations in radiation [8]. The similarity of the two famous fluctuation terms now led him to interpret the term associated to waves in radiation *in corresponding way for gases, by appropriately associating a radiation process to the gas and calculating its fluctuations by interference. (...) I expand on this interpretation because I think that his is more than a mere analogy.* At this point, Einstein refers to de Broglie's dissertation, saying that it *merits highest attention*.

Thus Einstein deduces from his analysis of fluctuations that the existence of matter waves is inescapable. Wolfgang Pauli mentions in this context in his essay "Einsteins Beitrag zur Quantentheorie" [3] [9]

> The author remembers that, in 1924 during a discussion at the physicist's conference in Innsbruck, Einstein proposed looking for interference and diffraction phenomena in beams of molecules.

[2] On the relationship between the quantum mechanics of Heisenberg-Born-Jordan and mine

[3] Einstein's contribution to quantum theory

Schrödinger's last paper before his discovery of wave mechanics is entitled "Zur Einstein'schen Gastheorie" [4] [10]. I cannot discuss it in detail here[5], but let me cite the key statement

> In other words, this means getting serious about the de-Broglie-Einstein undulation theory of moving corpuscles (...).

On November 23, 1925, Schrödinger gave a theory seminar in Zurich about de Broglie's dissertation. Many years ago, Felix Bloch gave an account of it here in Zurich and also recorded his recollections in an article in "Physics Today" [11], reminiscing:

> Debye casually remarked that he thought this way of talking was rather childish. As a student of Sommerfeld he had learned that, to deal properly with waves, one had to have a wave equation . . . Just a few weeks later [Schrödinger] gave another talk in the colloquium which he started by saying: 'My colleague Debye suggested that one should have a wave equation; well, I have found one!'

Indeed, already at the end of November (or maybe slightly later), Schrödinger first formulated a stationary relativistic equation, which is nowadays known as the Klein-Gordon equation. He solved it at the beginning of January, 1926, for the hydrogen atom. In between lies a mysterious stay in Arosa, where Schrödinger spent the Christmas break in Villa Herwig with a female friend from Vienna, while his wife stayed at home. In his excellent biography of Schrödinger, Walter Moore writes [12]

> Like the dark lady who inspired Shakespeare's sonnets, the lady of Arosa may remain forever mysterious. We know that she was not Lotte or Irene. In all likelihood she was not Felicie; her husband hat lost his fortune in the postwar inflation and had gone to Brazil, leaving her with an infant daughter. Whoever may have been his inspiration, the increase in Erwin's powers was dramatic, and he began a twelve-month period of sustained creative activity (...).

Schrödinger himself indeed was at Arosa, because a letter to Wilhelm Wien dated December 27 reveals that he worked on the eigenvalue problem for the hydrogen atom, but had not yet completely come to terms with the radial equation, maybe also because he had not brought the book by L. Schlesinger on differential equations, which he used at the time, with him to Arosa. In his letter to Wien, he sighs: *If only I knew more mathematics.*

Schrödinger returned to Zurich on January 9. To Dean Schlaginhausen's question whether he had enjoyed skiing, he replied laconically that he had been kept from it by some calculations. Back in Zurich he asked Hermann Weyl for help and then solved the hydrogen problem within days.

[4] On Einstein's theory of gases

[5] see A. Pais, pp 445-446

The relativistic equation

Before I come to Schrödinger's first publication on wave mechanics I would like to explain how he found the stationary Klein-Gordon equation. This is known from certain research notes (which Moore refers to as (N1) and (N2)).

For the sake of simplicity, I first give the nonrelativistic version of his argument. He starts from the stationary wave equation

$$(\Delta + k^2)\psi = 0. \tag{1}$$

In order to determine the wave number, he uses the first of the Einstein-de-Broglie relations

$$\mathbf{p} = \hbar\mathbf{k}, \quad E = \hbar\omega, \tag{2}$$

in the mechanical equation

$$E - V = \frac{1}{2m}\mathbf{p}^2. \tag{3}$$

When inserted into (1), this already gives the stationary Schrödinger equation for the one-electron problem:

$$(-\frac{\hbar^2}{2m}\Delta + V)\psi = E\psi. \tag{4}$$

The relativistic generalisation is immediate. Instead of (3) we have

$$E - V = E_{kin} = \sqrt{c^2\mathbf{p}^2 + (mc^2)^2},$$

and hence

$$\mathbf{p}^2 = \hbar^2\mathbf{k}^2 = \frac{1}{c^2}[(E-V)^2 - (mc^2)^2]. \tag{5}$$

Substituted in (1), this now gives the stationary Klein-Gordon equation

$$\Big[\Delta + \Big(\frac{E-V}{\hbar c}\Big)^2 - \Big(\frac{mc}{\hbar}\Big)^2\Big]\psi = 0. \tag{6}$$

Erste Mitteilung[6]

I will now start discussing Schrödinger's six papers [1] about wave mechanics, hereafter referred to as W1–W6. I shall put quite different weights on the individual articles, for good reason. In particular, I will discuss details about his second communication (zweite Mitteilung).

Purportedly, there was a first version of Schrödinger's first Mitteilung, which contained the relativistic equation and its application to the hydrogen atom. Because the spin-zero equation is known to give the wrong fine structure, Schrödinger is said to have retracted it. I should say that I am not aware of unambiguous documents supporting this.[7]

You would probably expect that Schrödinger starts out with a motivation for this stationary wave equation along the lines I sketched above. Instead, he bases it on a hypothesis that I find obscure, and I don't see what's behind it. He opens triumphantly with

[6] this translates to "first communication". We keep the German word "Mitteilung" here also for the three further papers that have this word in the title

[7] However, much later, Schrödinger confirmed the existence of this manuscript. In a letter to W. Yourgrau dated January, 1956, he writes [14]: My paper in which this is shown has . . . never been published, it was withdrawn by me and replaced by the non-relativistic treatment.

> 1. *In this communication, I would like first to show, for the simplest case of the (nonrelativistic and unperturbed) hydrogen atom, that the usual prescription for quantization can be replaced by a different requirement, in which there is no more mention of "integer numbers". Rather, integers appear in the same natural way as in the number of nodes of a vibrating string. This new point of view allows for generalization and, as I believe, it profoundly touches the true nature of the quantum conditions.*

Then he recalls the stationary Hamilton-Jacobi equation, and rewrites it in terms of $\psi = e^{S/K}$, where K is later put to what we today call $\hbar$. For the particular case of the one-electron problem in the Coulomb field, he obtains

$$(\nabla\psi)^2 - \frac{2m}{\hbar^2}\Big(E + \frac{e^2}{r}\Big)\psi^2 = 0. \tag{7}$$

Now he postulates that the left hand side should be taken as a density for a variational principle,

$$\delta \int \Big[(\nabla\psi)^2 - \frac{2m}{\hbar^2}\Big(E + \frac{e^2}{r}\Big)\psi^2\Big] d^3x = 0. \tag{8}$$

The corresponding Euler-Lagrange equation can immediately be read off, and it coincides with (4) for $V = -e^2/r$. In (W2), Schrödinger himself admits that taken as a reasoning rather than a mere postulate, this is incomprehensible, and one can regard it as very formal at best.

Solving (4) for a centrally symmetric potential by a separation ansatz is of course no problem for him, and the main task is to find everywhere finite solutions of the radial equation for the hydrogen atom. He starts with the remark that the latter has, in the complex r plane, singularities at $r = 0$ and $r = \infty$, and characterises the nature of these singularities. In a footnote, he acknowledges *I am greatly indebted to Hermann Weyl for instructions on how to deal with equation (...)* (and he also refers to the already mentioned book by Schlesinger). The way he works out the solution subsequently indeed gives the impression of Weyl looking over his shoulder. Schrödinger's treatment is much more rigorous than in most textbooks, exhibiting his love and talent for mathematics.

In the last section he makes a few preliminary remarks about interpretation and mentions in passing the result for the relativistic equation.

First reactions

This completely new explanation of the hydrogen spectrum made the world listen up. The paper was submitted on January 27, 1926, via W. Wien to *Annalen der Physik*, with the request to show it also to Sommerfeld. Two days later, Schrödinger reports to Sommerfeld in a letter [15] on further results about the linear oscillator and the rotator, and he announces pending investigations, among others, about the Stark effect.

Sommerfeld's reply, dated February 3rd, starts out with [16]

> *What you write is terribly interesting, both in your treatise and your letter. (...) I was just about to make a concept for lectures in London (this March) which played the earlier tune. And then your manuscript came like a thunderclap.*

A bit later, Sommerfeld says: *It is a bit rich that [the result about the rotator] should just come from the good old* $n(n+1)$ *of the spherical harmonics*. Here are some further quotes from this interesting letter:

> *Of course I cannot yet fathom mathematically how all this fits together, but I am convinced that something entirely new will come out of it, something that may do away with the contradictions that are vexing us now. (...) It is peculiar how, from such disparate starting points, you and Heisenberg have obtained the same results.*

The very same day, Sommerfeld wrote in a letter to Pauli [17]

> *We have received a manuscript by Schrödinger for the* Annalen*. He seems to find exactly the same results as Heisenberg and you, but by a completely different, absolutely crazy, path, by boundary value problems instead of matrix algebra. I am sure that all this will soon lead to something reasonable and definitive.*

On April 12, shortly after Schrödinger's first Mitteilung had appeared, Pauli wrote a long and remarkable letter to Jordan [18], in which he reports results from some considerations *that I have taken in connection to Schrödinger's paper "Quantization as eigenvalue problem" in Annalen der Physik.* Pauli hastens to assert: *I believe that this is among the most important works that have been written recently. Read it attentively and contemplate.*

The next sentence hints at the main contents of the letter:

> *Of course I have immediately asked myself how his results relate to the Göttingen mechanics. I believe I have now completely understood this.*

You see how news was spread at that time. I will return to further reactions of important theorists below.

Zweite Mitteilung

Schrödinger submitted his second 'Mitteilung' to the *Annalen* just four weeks after the first one. In his own words, it is about *elucidating the general relation between Hamilton's partial differential equation for a mechanical problem and the 'associated' wave equation*.

He devotes the larger part of this lengthy article (about 40 pages) to Hamilton-Jacobi theory, in particular Hamilton's analogy of mechanics and optics (§1). In justification of this long exposition, he says

> *Unfortunately, in modern treatments, this powerful and seminal circle of ideas of Hamilton has mostly been stripped of its beautiful intuitive raiment, which is apparently regarded as a useless addition and replaced by a rather pale exposition of the analytic background.*

And he mentions in a footnote that Felix Klein had emphasized the great importance of Hamilton's optical treatises already much earlier, but to disappointingly little effect.

Schrödinger's exposition of these well-known connections is beautifully written. His style of writing scientific prose has long disappeared from physics journals. He describes the mathematical facts completely informally, nevertheless precisely – and of course without all the pedantry that one finds necessary nowadays. At this point it is necessary to recapitulate the contents briefly in order to understand his justification of his wave equation, also for many-particle systems. By the way, I would recommend his exposition (with a few modifications) even for present-day teaching.

The starting point of §1 is the Hamilton-Jacobi (HJ) equation for an autonomous Hamiltonian system, for which the action function $S(q^i, t)$ can be chosen of the form

$$S(q^i, t) = W(q^i) - Et \tag{9}$$

and $W(q^i)$ satisfies the reduced HJ equation

$$H\Big(\frac{\partial W}{\partial q}, q\Big) = E \ . \tag{10}$$

Schrödinger then focuses on Hamiltonians of the form

$$H(p, q) = T(p, q) + V(q) \ , \tag{11}$$

where the kinetic energy T is a quadratic form in the canonical momenta

$$T(p, q) = \frac{1}{2}\sum_{i,j} g^{ij}(q)p_i p_j \ . \tag{12}$$

In the Lagrangian, it becomes

$$T(q, \dot{q}) = \frac{1}{2}\sum_{i,j} g_{ij}(q)\dot{q}^i\dot{q}^j \ . \tag{13}$$

As Schrödinger emphasizes with a reference to H. Hertz, the $g_{ij}(q)$ define a non-Euclidian Riemannian metric

$$ds^2 = g_{ij}(q)dq^i dq^j \tag{14}$$

(where $g^{ij}(q)$ in (12) is the inverse matrix). Differential operators in q-space – such as the Laplacian – are to be defined using this metric. In his words: *we impose that in the following, all geometric statements in q-space are to be understood in this non-Eudlidian sense*.

In the sequel, Schrödinger writes the reduced HJ equation for (11) (his equation $1''$) as

$$\|\nabla W\|^2 = E - V \ . \tag{15}$$

He interprets the surfaces $S = const$ as *a system of wave fronts of a propagating, but stationary wave motion in q-space* and he determines their phase velocity.

Before we go through this, I should like to recall the following facts from HJ theory. To any solution $W(q)$ of the partial differential equation (PDE) (10), it is natural to associate the following ordinary differential equation (ODE) in q-space (configuration space):

$$\dot{q}(t) = \frac{\partial H}{\partial p}\Big(\frac{\partial W}{\partial q}(q(t)), q(t)\Big) \ . \tag{16}$$

Every solution to this equation describes a mechanical trajectory in configuration space, hence is a solution of the Euler-Lagrange equation. Indeed one can show that by (16) and the HJ equation (10), $q(t)$ and

$$p(t) := \frac{\partial W}{\partial q}(q(t)) \tag{17}$$

satisfy Hamilton's equations. For the class of Hamiltonians (11), (16) reads

$$\dot{q}(t) = \nabla W(q(t)) \qquad \Big(\dot{q}^i(t) = g^{ij}\frac{\partial W}{\partial q^j}\Big) \ , \tag{18}$$

hence the trajectories of mechanics are perpendicular to the family of surfaces $W = const$. (In mathematics, they are called the characteristics of a first-order nonlinear PDE.)

Following Schrödinger, we now consider the surfaces of constant phase $S(q,t) = const$. This implies that $W = const + Et$. Given the constant we consider the surfaces at times t and $t + dt$ (see Figure 1), as well as a trajectory $q(t)$ that we can also parametrize by its arclength s given by the metric (14).

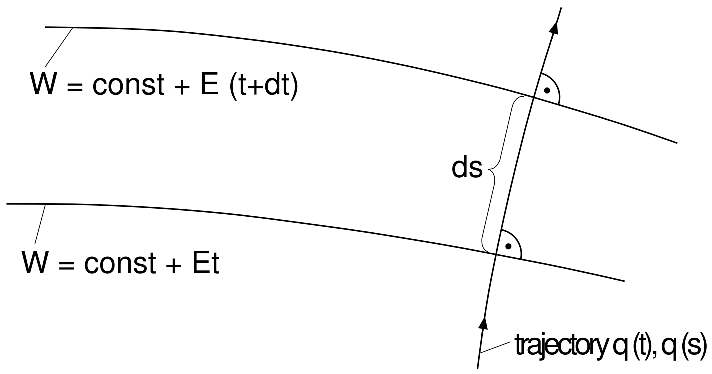


Figure 1: Surfaces of constant phase and orthogonal trajectories defining mechanical motions

Along the trajectory one has on the one hand, by the chain rule,

$$\frac{dW(q(s))}{ds} = E\frac{dt}{ds}$$

and on the other hand, because $\dot{q}(t)$ and ∇W are parallel,

$$\frac{dW(q(s))}{ds} = \Big\langle dW, \frac{dq}{ds}\Big\rangle = \Big(\nabla W, \frac{dq}{ds}\Big) = \|\nabla W\|$$

(where $\|\frac{dq}{ds}\| = 1$ has been used in the last equality). Thus the phase velocity is (Schrödinger's equation 6)

$$u := \frac{ds}{dt} = \frac{E}{\|\nabla W\|} = \frac{E}{\sqrt{2(E-V)}} \, . \tag{19}$$

This implies, as Schrödinger emphasizes, that <u>Hamilton's principle in the form of Maupertuis and Fermat's principle are the same</u>.

Let me briefly recap the former. Consider a mechanical trajectory $q(t)$ on the corresponding surface of constant energy in phase space (see Figure 2) and embed it into the family of phase trajectories at the same energy and the same inital and final points. Then the reduced action $\int p\, dq$ is stationary for the actual trajectory $q(t)$, hence the variational principle

$$\delta \int p\, dq = 0 \tag{20}$$

holds [19].

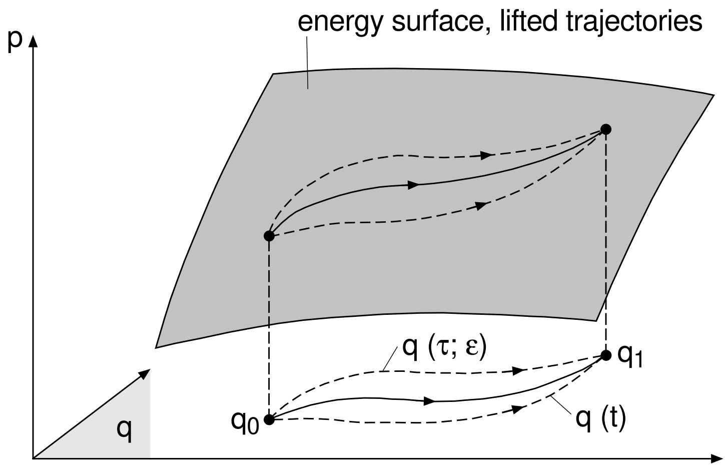


Figure 2: The principle of Maupertuis (Euler-Lagrange-Jacobi).

For the comparison family, the time parameter τ has to be chosen such that

$$H\Big(\frac{\partial L}{\partial \dot q}(\tau), q(\tau)\Big) = E \, .$$

Thus, using

$$\frac{\partial L}{\partial \dot q}\, \dot q = 2T = 2(E-V) \quad \text{and} \quad 2T = (\frac{ds}{d\tau})^2 \, , \tag{21}$$

we have

$$d\tau = \frac{ds}{\sqrt{2(E-V)}}. \tag{22}$$

Thus we can rewrite the reduced action as

$$\int p\,dq = \int \frac{\partial L}{\partial \dot q}\,\dot q\,d\tau = \int 2(E-V)d\tau = \int \sqrt{2(E-V)}ds = E\int \frac{ds}{u}\ , \tag{23}$$

and see that <u>the reduced action is proportional to the action in Fermat's principle</u>.

This is a particularly nice manifestation of the mechanico-optical analogy.

Schrödinger closes Section 1 with various remarks that give a summary and outlook. First, he rightfully emphasizes (and it seems necessary to reiterate this also today)

> *Although the above considerations have been formulated in terms of wave fronts, propagation speed and Huyghens's principle, one should properly view them as providing an analogy between mechanics and <u>geometrical</u> optics, not <u>wave</u> optics.*

A bit later, he adds

> *... and the system of S-surfaces, interpreted as wave fronts, is connected to mechanical motion only more loosely, insofar as the the image point of the mechanical system on the ray does by no means move by the wave velocity u. Quite to the contrary, its velocity is (at fixed E) proportional to $1/u$.*

After all, it is obtained directly as

$$v = \frac{ds}{dt} = \sqrt{2T} = \sqrt{2(E-V)}\ . \tag{24}$$

After Schrödinger reiterates that up to this point, things have revolved exclusively about geometrical optics, he sees the parallel with mechanics not just as a *pleasant means of providing intuition*. For, he says, *already the very first attempt at implementing a wave theory leads to so striking results that one may suspect something totally different: we do know today that our classical mechanics fails for very small-scale and for very strongly curved trajectories. Maybe this is in full analogy to the failure of geometrical optics*. After expanding further on this point, he concludes with the statement

> *Thus we need to search for an "undulatory mechanics", and the most obvious path for this seems to be a wave-theoretic version of the Hamiltonian picture.*

This is now developed in detail in §2, under the title 'geometric' and 'undulatory' mechanics. I have to be brief here.

In a wave-theoretic context, there are the notions of frequency and wavelength. Following Einstein and de Broglie,

$$\nu = \frac{E}{h} \tag{25}$$

and for the wavelength λ, (19) and (25) strongly suggest

$$\lambda = \frac{u}{\nu} = \frac{h}{\sqrt{2(E-V)}}\ . \tag{26}$$

The wave number $k = \omega/u$ then becomes

$$k = \frac{1}{\hbar}\sqrt{2(E-V)}\ . \tag{27}$$

This, together with (25), implies the dispersion law

$$\omega = \frac{1}{\hbar}\left[\frac{1}{2}(\hbar k)^2 + V\right]\ , \tag{28}$$

which Schrödinger deems to be of great interest. The main point is that the associated group velocity $d\omega/dk$ coincides with the mechanical velocity:

$$\frac{d\omega}{dk} = \hbar k = \sqrt{2(E-V)} = v\ . \tag{29}$$

After somewhat lengthy considerations Schrödinger finally concludes

> *This makes me certain that the following should be true: What really happens here mechanically is correctly captured by the wave phenomenon in q-space.*

Again, he continues with far-reaching deliberations, after which he poses himself the real task:

> *One has to replace the fundamental equations of mechanics by a wave equation in q-space and look at the manifold of processes it admits.*

For simplicity, he proposes the following. The usual stationary wave equation in q-space is

$$(\Delta_g + k^2)\psi = 0\ , \tag{30}$$

where Δ is the Laplacian for the metric (14). Inserting the expression (26) for k gives

$$\Big(-\frac{\hbar^2}{2}\Delta_g + V\Big)\psi = E\psi\ . \tag{31}$$

For the special case of a particle of mass m, $\Delta_g = \frac{1}{2m}\Delta$, where Δ is the Laplacian on three-dimensional Euclidian space, and (31) reduces to (4).

Equation (31) now also provides the foundation for the treatment of several particles, albeit without spin. The author says about the Schrödinger equation

> *Our ansatz is again dictated by simplicity, which might, however, be misleading.*

But it is very encouraging that *in all cases of classical dynamics that I have investigated so far, equation (31) encompasses the quantization condition*.

Schrödinger ends this most important part of his paper with remarks on matrix mechanics.

> *At this point I should not like to leave unmentioned that presently an attempt to resolve the quantum difficulty by Heisenberg, Born, Jordan, and a few other excellent researchers, is underway. It has already achieved so remarkable successes that one can hardly doubt it to contain at least part of the truth. In tendency, Heisenberg's proposal is very close to the present one, as we have mentioned already above. From the point of view of method it is of so very different nature that I have not yet been able to find a link. I definitely hope that these two proposals will complement one another, in the sense that one approach may help where the other fails. The strength of Heisenberg's programme lies in its promise to yield the line intensities, a question that we have avoided so far here.*

The further chapters of Schrödinger's second Mitteilung are devoted to the harmonic oscillator and the rotator as sample applications. Moreover, he announces perturbative investigations, in particular of the first order Stark effect.

At this moment, I will omit his preliminary remarks about the interpretation of wave mechanics. Schrödinger was so intensely focused on the development of his theory that he postponed questions connected to interpretation. He did, however, cling to a <u>classical</u> wave picture. Yet he was well aware of its inherent difficulty, namely that his wave function lived on a higher-dimensional configuration space. I shall return to interpretational questions at the end of this article.

The continuous transition from micro- to macromechanics

This is the title of a short paper [1] that Schrödinger published in "Die Naturwissenschaften". In it, he proves for the harmonic oscillator *that a <u>group</u> of eigenoscillations of high order n ('quantum number') and relatively small differences in order can represent a 'point mass' which 'moves' according to the laws of ordinary mechanics*. He concludes this simple investigation with the quite astonishing remark

> *One can now say with certainty that in a very similar way one can also construct the wave packets that move on Kepler ellipses at high quantum number and hence constitute the wave mechanical picture of the hydrogen atom; merely the calculational difficulties are much greater there than in the particularly simple, pedagogical example treated here.*

Not everyone agreed, however. In particular, H.A. Lorentz immediately showed to Schrödinger that this was wrong. Already in a first of two long letters, still before Schrödinger's note (W3), Lorentz writes under Item 4 [20]

> *If we decide, so to speak, to dissolve the electron entirely and to replace it by a wave pheonomenon, then this has a disadvantage and an advantage. The disadvantage, and a very serious one, too, is as follows: what we assume about the electron of the hydrogen atom, we will have to assume about all electrons in all atoms; we have to replace them by wave systems. How should I then understand the phenomena of*

> *photo-electricity and the escape of electrons from metals upon heating? Here the particles emerge nicely and whole; how did they, once dissolved, manage to pull themselves together again?*

In a second letter, Lorentz writes [21]

> *You have delighted me by sending me your note 'The continuous transition from micro- to macromechanics', and when I had read it, my first thought was that a theory that counters an objection in such a surprising and beautiful way must lead to the right path. Unfortunately, my delight was short-lived. I cannot see, for instance, how, in the case of the hydrogen atom, you would be able to construct wave packets that move like an electron (I am now thinking of the high Bohr orbits). You don't have the necessary short-length waves available. I had touched on this point already in my first letter, and I would like to expand a bit on it now.*

There follows a twelve-page calculation of the 73-year-old Lorentz which results in the impossibility of achieving this for the hydrogen atom.

H.A. Lorentz's letters are very impressive in many other respects as well. No other colleague devoted himself in such detail to the difficulties that still remained. I really should spend more time on these letters.

Wave mechanics and matrix mechanics

From the next paper [1] "On the relation of the Heisenberg-Born-Jordan quantum mechanics to mine" I first cite a few passages taken from the introduction. Besides introducing the problem, they also give an impression of Schrödinger's style.

> *Given the disparate starting points and concepts of Heisenberg's quantum mechanics on the one hand, and on the other hand the theory called 'undulatory' and 'physical' mechanics and exposed here recently, it is quite peculiar that these two new quantum theories agree, as far as their results have been developed, also in the realm where they differ from the old quantum theory. Let me highlight in particular the striking 'half-integer-valuedness' for the oscillator and the rotator. This really is remarkable since the starting point, the intuitions, the methods, the entire mathematical apparatus, seem to be totally different indeed. Most importantly, however, the two theories seem to depart from classical mechanics in diametrically opposed directions. Following Heisenberg, the continuous classical variables are replaced with discrete systems of numbers (matrices) which, depending on a pair of integer indices, are determined by algebraic equations. The authors themselves call it 'truly a theory of the discontinuum'. By way of contrast, undulation mechanics implies a step from classical mechanics to a continuum theory (...). In what follows, the intimate relation of Heisenberg's quantum mechanics and my undulation mechanics will be uncovered. From the formal mathematical point of view one will have to call this relation the identity (of both theories).*

I won't need to say much about how this is done since we are all familiar with the abstract formulation of quantum mechanics by Dirac, Weyl, and von Neumann, which allows for a diversity of isomorphic representations. This is the subject of the so-called transformation theory.

Schrödinger utilized the mathematical fact, proven by F. Riesz in 1907, that every complete orthonormal system $\{u_n\}$ in the L^2-space over configuration space defines a Hilbert space isomorphism to the ℓ^2 space:[8]

$$L^2 \ni \psi = \sum c_n u_n \ \longrightarrow \ \{c_n\} \in \ell^2. \tag{32}$$

This isomorphism induces a map that takes every (bounded) operator to a matrix, and this mapping respects all algebraic operations, in particular the canonical commutation relations of position and momentum operators (Schrödinger imposes sufficient decay of the u_n so that for these unbounded operators, no boundary terms occur upon integration by parts.) Crucially, the energy matrix becomes diagonal if the u_n are chosen as the eigenfunctions of the quantum Hamiltonian. (Schrödinger was well aware of the complications entailed by continuous spectrum.)

Because Schrödinger did not yet have the <u>time-dependent</u> wave equation at his disposal, the way in which he attempts to relate to Heisenberg's equation of motion for matrices is still a bit awkward. It actually works only when using an orthonormal basis $\{\varphi_n(t)\}$ of solutions to the time-dependent Schrödinger equation

$$i\hbar\dot{\varphi}_n = H\varphi_n, \qquad (\varphi_n, \varphi_m) = \delta_{nm}, \tag{33}$$

which shows up only later in (W6). Then the matrix of a Schrödinger operator F^{op} is given by

$$F_{nm} = (\varphi_n, F^{op}\varphi_m) \ , \tag{34}$$

and it satisfies Heisenberg's equation of motion

$$\dot{F} = \frac{i}{\hbar}[H, F] \ . \tag{35}$$

For the particular case of stationary eigenstates $\varphi_n = u_n e^{-\frac{i}{\hbar}E_n t}$ of the Hamiltonian, the associated matrix is diagonal

$$H_{nm} = E_n \delta_{nm} \tag{36}$$

and the equation of motion reads

$$\dot{F}_{nm} = \frac{i}{\hbar}(E_n - E_m)F_{nm} \ , \tag{37}$$

with solution

$$F_{nm}(t) = F_{nm}(0)\, e^{\frac{i}{\hbar}(E_n - E_m)t} \ . \tag{38}$$

[8] As von Neumann notes in Remark 35 of his classic book "Mathematische Grundlagen der Quantenmechanik" [23], Schrödinger really used a slightly weaker statement which was proven by Hilbert in 1906 [24]. Hilbert showed that L^2 is isomorphic to a subspace of ℓ^2.

In his above-mentioned letter to Jordan [18], Pauli developed all this completely independendently. He also obtained the correct time dependence of the matrices.

Calling this an 'equivalence', resp. 'identity', of the two formulations is a bit exaggerated, among other things because in the original formulation of matrix mechanics, the notion of a stationary state was not even introduced. This was pointed out in particular by van der Waerden in a well-known paper. But this may be a bit pedantic. We may forgive Schrödinger for the following slip of tongue, probably in his exuberance about the discovery:

> *In this context, the following extension of the above-mentioned proof of equivalence is of interest: this is <u>really</u> an equivalence, also valid in the <u>converse</u> direction.*

Once more, I postpone Schrödinger's comments on the interpretation of the wave-mechanical formalism, except for the following, which hints at his ideas. In the last section of the paper we are discussing, he makes the following point:

> *A very important question, indeed, probably the cardinal question of all atomic dynamics, is obviously: how is what happens to the atom coupled dynamically to the electromagnetic field, or the quantity that has to replace it. (...) Here, the matrix representation of atomic dynamics has led to the conjecture that indeed also the electromagnetic field has to be treated differently, namely by matrices, so that this <u>coupling</u> can be formulated mathematically. Undulation mechanics shows that one is not forced to do that because the mechanical field scalar (that I have denoted by ψ) is perfectly well-suited to enter as a source term in the otherwise unchanged Maxwell-Lorentz equations for the electromagnetic field vectors. And conversely, the electrodynamical potentials enter in the coefficients of the wave equation that determines the mechanical field scalar.*

You see from these remarks that Schrödinger hopes for a classical-realistic interpretation. But he mentions again that for multi-electron systems, the wave function is a function on configuration space, which leads to difficulties with this interpretation.

There were various reactions to this early on. Already on May 24, Pauli writes to Schrödinger in a very substantial letter [25]

> *In any case, I have the strongest doubts that a purely continuous field theory of de Broglie radiation can really stand on its own. One will have to introduce some essentially discontinuous elements into the description of quantum phenomena. I know that this point of view will meet your most vivid protest, and I am therefore very much looking forward to talking to you about it in detail during my visit in Zurich at the end of June (I have already accepted Debye's invitation).*

It may be noted that a lecture series took place from June 21 to 26 in Zurich. On this occasion, Schrödinger heard from Pauli and Sommerfeld about Born's proposal of a statistical interpretation of the wave function.

A bit later Pauli coined the term 'Zürcher Lokalaberglaube' (local Zurich superstition), which quickly became common usage. In a much later letter in November to Schrödinger [26], he soothingly wrote

> *As to my remark about the 'Zürcher Lokalaberglaube', I would really like to ask you not to understand it as a personal offense but rather as an expression of my rational conviction that some aspects of the quantum phenomena in nature cannot be captured by concepts of continuum physics (field physics) only. But please don't think that I have come to this conviction lightly; I have been having a truly hard time with it, and I don't expect this to change in the near future.*

But let us return to Schrödinger's sequence of papers.

Dritte Mitteilung – wave-mechanical perturbation theory

I won't need to say very much about the third *Mitteilung* in the series 'quantization as an eigenvalue problem'. In 53 pages, Schrödinger develops stationary perturbation theory and applies it to calculate the level splitting by the Stark effect. It is too early to explain the anomalous Zeeman effect. In view of the latter, he notes

> *One will have to attempt to include the idea of Uhlenbeck and Goudsmit into wave mechanics.*

As we know, this was achieved some time later by Pauli.

Vierte Mitteilung – the time-dependent wave equation

I now turn to Schrödinger's last article in his series on wave mechanics. Finally, he also finds the time-dependent equation. You may already have wondered what took him so long. His main difficulty was that for a realistic wave-mechanic theory of atoms, he preferred a real-valued wave function. I substantiate this with a quote from the final section of this long paper:

> *Clearly, it is still somewhat hard to use a complex wave function. If this were* fundamentally *inevitable and not just a mere device for calculational simplification, this would mean that there are in principle two wave functions that* only together *allow us to know about the state of the system. This slightly repellent conclusion allows (as I believe) for the much more attractive interpretation that the state of the system is given by a real function and its time derivative. That we have not been able to make this specific has to do with the pair of equations (4") where we only have the – calculationally very convenient – surrogate of a real wave equation of probably fourth order which I was, however, not able to establish in the non-conservative case.*

I will have to explain this last statement. In §1, Schrödinger starts out with conservative systems. For them, his previous papers imply that the stationary states satisfy the eigenvalue equation $Hu = Eu$, hence the fourth-order equation

$$H^2 u = E^2 u \; . \tag{39}$$

Let us follow Schrödinger and consider a real wave equation with a harmonic time dependence

$$\psi = \mathrm{Re}\,(ue^{\pm i\omega t}), \qquad \omega = E/\hbar\,. \tag{40}$$

Then multiplication by E^2 is equivalent to $-\hbar^2\partial^2/\partial t^2$, and Schrödinger gets instead of (37) the equation

$$H^2\psi = -\hbar^2\frac{\partial^2\psi}{\partial t^2}\,. \tag{41}$$

He concludes: *Hence this equation is the unifying and general wave equation for the field scalar* ψ, and he points out that it is of fourth order – just like for many problems of elasticity (e.g. the vibrating plate).

The use of (39) for non-conservative systems is, however, not acceptable, because in that case it would have to contain a term containing $\partial V/\partial t$. For these types of systems, Schrödinger first sticks to complex wave functions. If one drops the real part in (40), multiplication by E is equivalent to $\pm i\hbar\partial/\partial t$, leading him to the pair of equations

$$H\psi = \pm i\hbar\frac{\partial\psi}{\partial t}\,. \tag{42}$$

He remarks:

> *We shall require that the complex wave function* ψ *satisfy one of the two equations. Since the conjugate-complex function* $\overline{\psi}$ *will then satisfy the other equation, one may take (if needed) as a real wave function the real part of* ψ.

In possession of this fundamental equation, Schrödinger now develops time-dependent perturbation theory and applies it to dispersion theory. He also perturbatively calculates the induced dipole moment of an atom in a time-dependent field, and in this way he gives a foundation to the formulas of Kramers and Heisenberg, who had obtained them in 1924 by considerations based on the correspondence principle. (He also dwells on the resonant case, yet without proposing a solution; the latter was found only much later by Weisskopf and Wigner.)

In a later section (§6), Schrödinger obtains, very formally, what he calls the *'relativistic-magnetic generalization of the field scalar'*.

The last section, §7, is entitled *On the physical meaning of the field scalar*. This leads us to questions of the interpretation of the new, mathematically fully formulated theory.

The controversy about the interpretation

In the above-mentioned concluding paragraph, Schrödinger interprets $\bar{\psi}\psi$ as 'a kind of weight function on configuration space', and he shows that it satisfies the by-now familiar continuity equation in configuration space. In the sequel, he gives a few vague hints towards a realistic electromagnetic interpretation, ending with the words:

> *I hope and believe that the above proposals will turn out to be useful to understand the magnetic properties of atoms and molecules and furthermore to explain electrical conduction in solids.*

While the formalism of wave mechanics was gratefully received by everyone, Schrödinger's physical intuitions met the resistance of most of his established colleagues. I have already cited some of it. In a letter dated June 8 to Pauli, Heisenberg vented his annoyance:

> *As nice as I find Schrödinger as a person, I find his physics just as weird: listening to it, one suddenly feels 26 years younger. Schrödinger throws everything 'quantum theoretic', namely the photoelectric effect, Franck's collisions, the Stern-Gerlach effect, etc., over board (...).*

There were some other voices as well, in particular those of more senior accomplished scientists like W. Wien and at first also Einstein. Yet the latter soon had his doubts.

The hardest confrontation took place in Copenhagen at the end of September in N. Bohr's house. (Heisenberg and Pauli were at this time at Copenhagen, too.) Heisenberg reported on these intense discussions in his book 'Der Teil und das Ganze'[9] [29]. Here is a small part of it:

> *The discussions of Bohr and Schrödinger started already at the Copenhagen train station, and they continued every day from early morning to late night. Schrödinger stayed in the house of the Bohr family, so that there was no respite due to external reasons. And although Bohr was otherwise very considerate and amiable in his dealings with other people, here he appeared to me like an unrelenting zealot that was not willing to take a single step towards his discussion partner nor to admit the slightest lack of clarity. It is almost impossible to convey how fervently both sides were invested in these discussions and how deeply rooted the convictions both of Bohr and Schrödinger were, as you could infer them from what was said. So the following can only be a very pale image of those conversations, in which they spent all their strength wrestling with the interpretation of the newly obtained mathematical representation of nature. (...) Thus the discussions lasted over many hours of day and night without any agreement. After a few days, Schrödinger fell ill, maybe as a consequence of the enormous exertion; he was bedridden with a feverish cold. Mrs. Bohr took care of him and brought tea and cake, but Niels Bohr sat on the edge of the bed and kept talking to Schrödinger: "But you must accept that . . ."*

Schrödinger eventually understood that he could not keep up his original conceptions.
The following passage is taken from a letter he wrote to Sommerfeld on April 29, 1927 [30].

> *As to the interpretation of quantum mechanics, I am less certain than ever before. The works of Unsöld and Pauling may lend some new credibility to 'smeared electrons', but the problems of the continuum interpretation cannot be denied. I can even understand a little bit the annoyance that I have caused among many people by shouting victory to a large, not very critical crowd: 'Down with quanta! Continuum physics saved!' I must appear to the other experts a bit like a demagogue who speculates on the gullibility of the masses and caters to their preferences. – Well, it will be clarified. The foaming grape must that we are all slurping right now will [?] eventually become a quite decent wine.*

[9] The part and the whole

However, Schrödinger simply could not get comfortable with the 'Copenhagen interpretation' of quantum mechanics. Like Einstein he was unwilling to give up the notion of reality that underlies classical physics. But I think that their harsh critique has also had positive consequences. With their analyses and thought experiments they have put life into the discussion about questions of interpretation to the present day. Schrödinger's 'entanglement of states' and 'Schrödinger's cat experiment' are discussed everywhere, and they have eventually led to actual experiments that probably will even have important impact on technology. Others will report on that at this symposium.

What is here to stay is Schrödinger's mathematical formulation of the theory of wave mechanics, in particular his fundamental dynamical equation. It stands unchanged, and it will be taught as long as humans do science.

References

[1] All of Schrödinger's papers on quantum theory can be found in Volume 3 of his collected works (Gesammelte Abhandlungen, Ref. (2).)

His six original articles[10] on wave mechanics, referred to as (W1)–(W6) in the text, are

- (W1) E. Schrödinger, *Quantisierung als Eigenwertproblem* (Erste Mitteilung), Ann. Phys. **79**, 361-76 (1926).
- (W2) E. Schrödinger, *Quantisierung als Eigenwertproblem* (Zweite Mitteilung), Ann. Phys. **79**, 489-527 (1926).
- (W3) E. Schrödinger, *Der stetige Übergang von der Mikro- zur Makromechanik*, Die Naturwissenschaften, 14. Jahrg. Heft 28, S. 664-666 (1926).
- (W4) E. Schrödinger, *Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zu der meinen*, Ann. Phys. **79**, 734-56 (1926).
- (W5) E. Schrödinger, *Quantisierung als Eigenwertproblem* (Dritte Mitteilung), Ann. Phys. **80**, 437-90 (1926).
- (W6) E. Schrödinger, *Quantisierung als Eigenwertproblem* (Vierte Mitteilung), Ann. Phys. **81**, 109-39 (1926).

[2] E. Schrödinger, Gesammelte Abhandlungen. Verlag der Österreichischen Akademie der Wissenschaften, Friedr. Vieweg & Sohn, Braunschweig/Wiesbaden, Wien 1984, 4 Bde.

[3] Karl von Meyenn (ed.) Eine Entdeckung von ganz außerordentlicher Tragweite. Schrödingers Briefwechsel zur Wellenmechanik und zum Katzenparadoxon: Berlin: Springer, 2011. Two volumes. ISBN 978-3-642-04334-5.

[4] E. Schrödinger, Letter to W. Wien, 22. Feb. (1926), Reg. (3).

[5] A. Pais, Albert Einstein, Eine wissenschaftliche Biographie, Vieweg (1986).

[10] Mitteilung = communication

[6] A. Einstein, Letter to H.A. Lorentz, 16. Dez. (1924).

[7] A. Einstein, Sitzungsb. Preuss. Akad. Wiss. 1925, S. 3.

[8] A. Einstein, Phys. Zeitschr. **10**, 185 (1909); **10**, 817 (1909).

[9] W. Pauli, *Einsteins Beitrag zur Quantentheorie*, in Physik und Erkenntnistheorie, Vieweg, Braunschweig (1984).

[10] E. Schrödinger, Phys. Z. **27**, 95-101(1926).

[11] F. Bloch, Physics today **29**, (December), 23-7 (1976).

[12] W. Moore, *Schrödinger, Life and Thought*, Cambridge University Press (1989), p. 195.

[13] E. Schrödinger, Letter to W. Wien, 27. Dez. (1925), Ref. (3).

[14] E. Schrödinger, Letter to W. Yourgrau, Jan. 1956, Ref. (12), p. 196.

[15] E. Schrödinger, Letter to A. Sommerfeld, 29. Jan. (1926), Ref. (3).

[16] A. Sommerfeld, Letter to E. Schrödinger, 3. Feb. (1926), Ref. (3).

[17] A. Sommerfeld, Letter to W. Pauli, 3. Feb. (1926), in *Wissenschafticher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, Band I: l919-1929. Edited by A. Hermann, K. v. Meyenn und V. F. Weisskopf. Springer, New York/Heidelberg/Berlin 1979. Letter [118a], printed in Volume II.

[18] W. Pauli, Letter to P. Jordan, 12. April (1926), Ref. (17), Dok. [131].

[19] See, e.g., V. I. Arnold, Mathematical Methods of Classical Mechanics, Graduate Texts in Mathematics 60, Springer-Verlag, New York/Heidelberg/Berlin (1978); in particular §45.

[20] H. A. Lorentz, Letter to E. Sommerfeld, 27. Mai (1926), in *Briefe zur Wellenmechanik*, K. Przibram (ed.), Springer-Verlag, Wien (1963), p. 41

[21] H. A. Lorentz, Letter to E. Schrödinger, 19. Juni (1926), Ref. (20), p. 61.

[22] F. Riesz, C. R. Acad. Sci., Paris **144**, 615-619 (1907).

[23] J. von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Springer-Verlag, Berlin/Heidelberg (1996).

[24] D. Hilbert, Gött. Nachr. (1906).

[25] W. Pauli, Letter to E. Schrödinger, 24. Mai (1926), Ref. (17), Dok. [134].

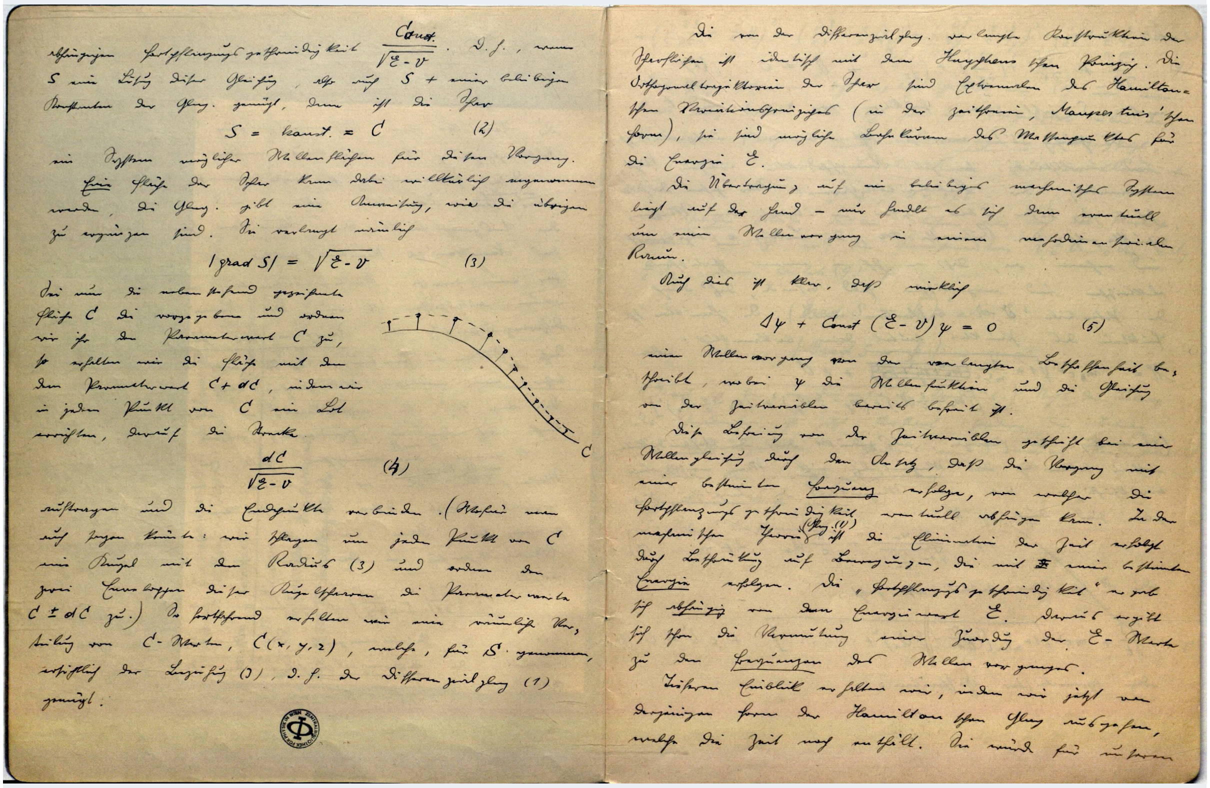
[26] W. Pauli, Letter to E. Schrödinger, 22. Nov. (1926), Ref. (17), Dok. [147].

[27] W. Heisenberg, Letter to W. Pauli, 8. Juni (1926), Ref. (17), Dok. [136].

[28] W. Heisenberg, Letter to W. Pauli, 28. Juli (1926), Ref. (17), Dok. [142].

[29] W. Heisenberg, *Der Teil und das Ganze*, Piper-Verlag, München (1969); Chap. 6.

[30] E. Schrödinger, Letter to A. Sommerfeld, 29. April (1927), Ref. (3).



Two pages from Schrödinger's scientific notebooks, from the section titled "Für die zweite Mitteilung" ("for the second communication"). This image of his manuscript (W33-725/2, *Österreichische Zentralbibliothek für Physik*) is reproduced with permission from his family and the Austrian Central Library of Physics, Vienna. We would especially like to thank Dr. Helen Piel, curator of the Erwin-Schrödinger archive, Vienna, for her help in obtaining both the image and the permission to use it.

Konrad Osterwalder
June 3, 1942 – December 19, 2025

by ARTHUR JAFFE and JÜRG FRÖHLICH

Arthur Jaffe

In the spring of 1968, a family of mathematical physicists invited me to visit their Zurich home at Hochstrasse 60. That family, the "Seminar für Theoretische Physik," was headed at the time by Res Jost and Markus Fierz, along with recent additions of Walter Baltensperger and Klaus Hepp. They had ample assistance from Rosemarie Hintermann, and I joined as guest professor. That marked the beginning of my long relationship with the ETH.

To my enormous fortune, the family elders assigned a graduate student assistant to me named Konrad Osterwalder. He followed my course and wrote copious, detailed notes. I still do not understand why we never published them. So now I tell my students to publish their work right away, or you might never get around to doing it.

Konrad and I became good friends that summer, and I have learned a tremendous amount from him in the 57 years since. Right at the start, during a student outing on the lake of Zurich to the Rapperswil Castle, Konrad taught me the joy of fondue—especially that it goes better with Kirschwasser, rather than with tap water.

Konrad 1970 at Les Houches.

Konrad with Jürg Fröhlich, 1980's.[11]

Konrad told me that summer that he would like to become my graduate student; I suggested that it would be much easier for us to work together after he finished his doctorate with Hepp and Jost. That happened three years later when Konrad joined Robert Schrader as my second postdoctoral fellow.

[11] All pictures in this article are courtesy of Arthur Jaffe

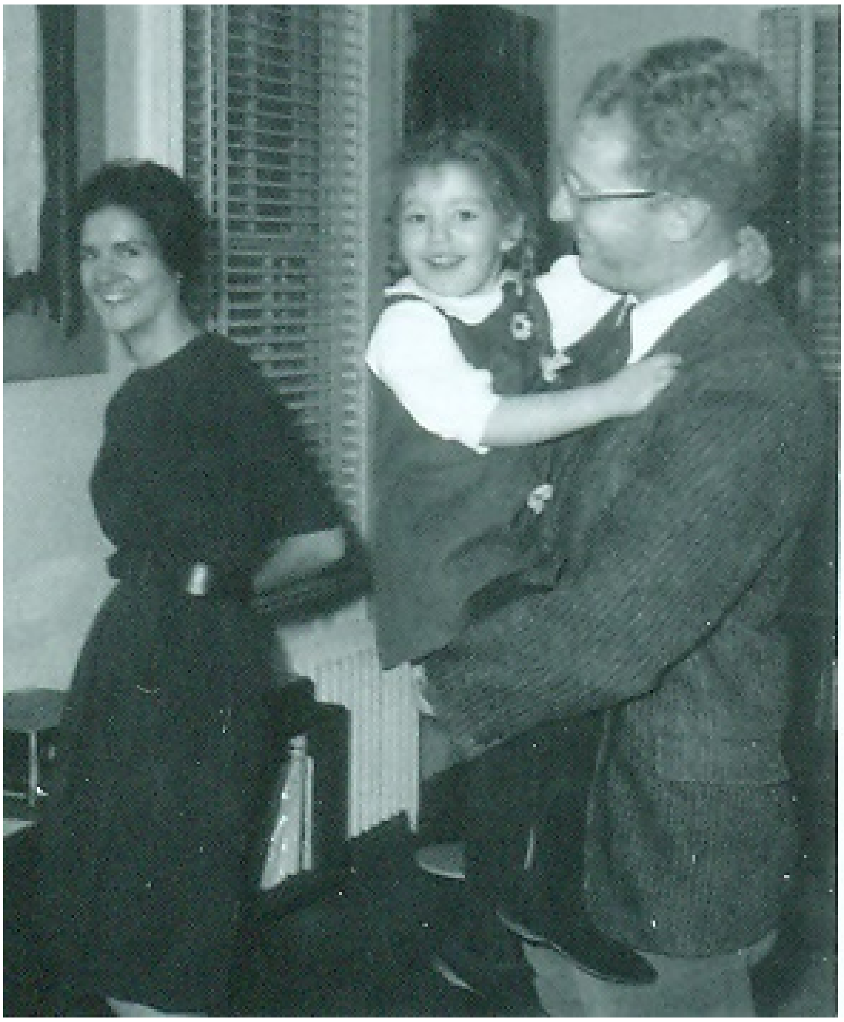
Konrad with his family in Winchester, Massachusetts, 1973.

A major scientific happening occurred in 1972, when I received a preprint from Edward Nelson with his Markov field reconstruction theorem. The possibility to go from Euclidean fields to relativistic fields had been a major obstacle that fascinated me, and I began to discuss the new paper with Konrad. I was sure that there must be a more general construction, not founded in probability theory. A week later, Robert Schrader returned from vacation visiting his friend Krista in East Germany, as I left for several days to attend a conference at the University of Chicago. I had a long discussion with Robert asking him to discuss the reconstruction theorem with Konrad. On my return, Konrad and Robert showed me their discovery of reflection positivity. The resulting theory of Osterwalder-Schrader quantization should appear in every modern textbook on probability theory, on operator algebras, and on quantum theory.

This work eventually led to Konrad becoming a faculty member at Harvard in physics and mathematics, and in 1976 a professor of mathematics at the ETH. That 1976 summer Konrad and Vreni hosted a wonderful party at their Newton home, where Jürg Fröhlich also came from Princeton to wish Konrad good luck in Zurich.

1976 Farewell Party at the Osterwalder home in Newton, Massachusetts.
Behind: Catherine Sénéor, Joel Feldman, Maureen Seiler, Vreni Osterwalder, Eva Fröhlich, Frieda Sénéor
Front: Roland Sénéor, Konrad Osterwalder, Erhard Seiler, Arthur Jaffe, Jürg Fröhlich

Over the next ten years, Konrad helped bring many extraordinary persons to Zurich. This included my best friend in graduate school, Oscar Lanford (see image below), and Jürg Fröhlich. Their presence guided a period of renewal for the ETH on the world stage in mathematical physics, following the period of Jost and Fierz. Eventually Konrad became the mathematics department chair. Around 1987, the mathematics department underwent an evaluation, led by Fritz Hirzebruch. I was happy that Konrad had me appointed to that committee, for Konrad also arranged that I have a private meeting with the influential former ETH president Heinrich Ursprung. He asked advice on how to ensure the continued excellence of ETH in mathematical physics.



The Lanford family in Princeton 1966.

Around that time, Konrad sent Andrew Lesniewski to be my postdoctoral fellow, and eventually the three of us began a collaboration. This ended up with our discovery of an explicit connection between supersymmetry in physics and the mathematics of entire cyclic cohomology. That was the encapsulation of invariant theory in non-commutative geometry, invented by Alain Connes. With Konrad, we found an explicit formula for a cocycle in the theory, which became useful in studying invariants in this geometry. This also led to one of Konrad's unfinished projects, a supersymmetric version of the Osterwalder-Schrader reconstruction theory.

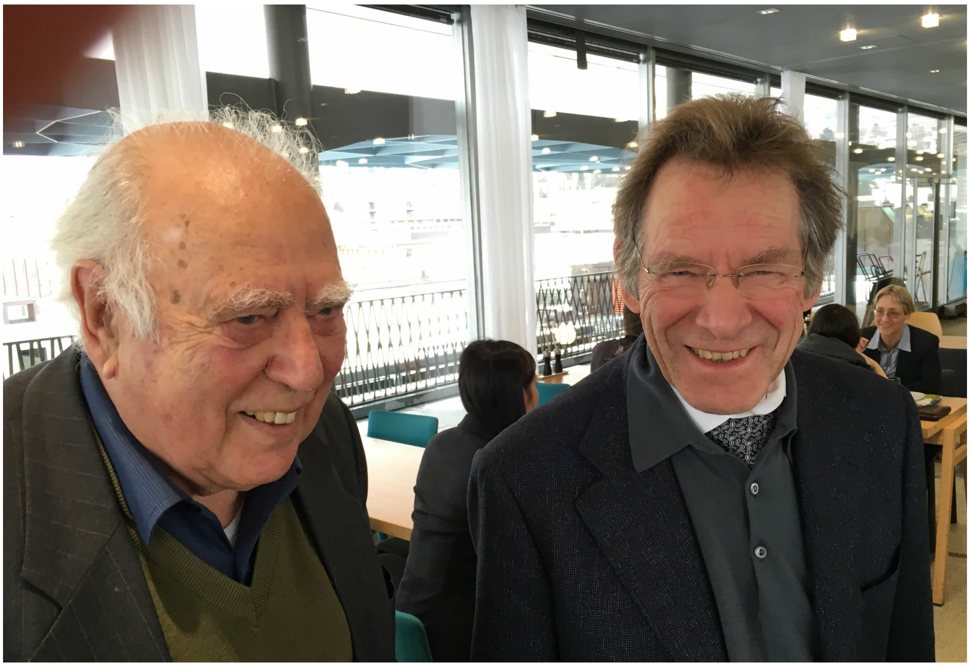
My friendship with Konrad flourished and that same fall Konrad came from Zurich to Connecticut to be the best man at my wedding in Connecticut.

Konrad with Arthur in Sharon, Connecticut 1992

Konrad with Robert Schrader in Salisbury, Connecticut

Konrad's research career ended in 1995, when he was elected Rektor at the ETH. Konrad knew that there was a summer course in the Harvard School of Education, intended for university administrators. He decided to take it. In those days one had to check email with a computer wired to the internet, and at every lunch time Konrad came to my office to use my computer. And each day Konrad told me, "Arthur, you cannot believe what they are teaching in that course!" Finally, Konrad took me there to visit the course, and I couldn't believe it!! Konrad introduced me at the break to two friends there, one from Ireland and the other from Australia. They explained that the three of them decided never to speak up in class discussions, as they would be attacked, both by the other students as well as the very articulate teachers. Konrad opinion was that he learned in the Harvard course, "how NOT to run a university." The course reflected the leading edge in a world-wide trend taking place in universities, in which there was a transition from the primary goal of the university being "to shape ideas," to the primary focus of the university being "to grow a business." On a related topic, Konrad once joked his "worst nightmare" would be to arrive on the campus of a university, and see many students rushing by. Stopping one he asked, "Where are you going?" The response, "I am late for my course on evaluating evaluators."

When Konrad left ETH in 2007, the Swiss government was able to arrange that he become Rector of the United Nations University, whose main location is in Tokyo; UNU also has collaborations with numerous research institutes around the world. Konrad changed the UNU from mainly giving courses that would benefit UN employees, often working on agriculture or energy, to a degree-granting institution of higher learning. Jean-Pierre Bourguinon told me that after Konrad left, he had to remain on the UNU board, in order to attempt to prevent all the good things started by Konrad from being dismantled.



Konrad at ETH in 2016 with Hans Bühlmann, mathematician and former ETH president.

Being Rector of UNU also comes with the appointment as an Under-Secretary-General of the United Nations. Because of Konrad's scientific background, Konrad was appointed the liaison between the Japanese government and the UN over the aftermath of the nuclear accident in Fukishima. Konrad shared with me his frustration about this. He related that assignment was the most vexing one he had ever dealt with in his lifetime. Konrad was terrifically upset that major decisions concerning the Japanese population were made on political, rather than scientific grounds, resulting (in his opinion) in potential grave danger for the Japanese people. Some years later, Konrad's assistant at the UN in New York, Luna Al-Husseini Abu Khadra, told me that Konrad was considered a hero by the New York workers at the UN, as he was the only person at the UN who would and did stand up to the top leadership.

After that Konrad returned to Switzerland, at which time Konrad and I began to meet more often. I attempted to get Konrad back to doing research. We sometimes met at Harvard, and other times in Switzerland. I went on several trips with Konrad to visit potential houses that he considered buying; we always ended up having a wonderful dinner at a restaurant Konrad knew about nearby. Eventually Konrad moved to a modern house in Bieberstein, a village outside of Aarau.

Konrad always thought about the humanity and well-being of the world. His dream was to enable the creation of a new international organization, something like CERN, but with a focus on solving problems of energy. This never came to pass. I miss Konrad, as we all do. His vivid picture as a modest and charming man remains engraved in my mind.

Arthur Jaffe
Harvard University

Jürg Fröhlich

In this text, I share some presonal reminiscences of Konrad Osterwalder, a long-term member of our scientific family of mathematical physicists and former president of the IAMP.

I would like to begin my remarks with the observation that the two colleagues who had the greatest influence on Konrad's scientific development and academic career are Klaus Hepp of ETH Zurich, Konrad's PhD advisor, and Arthur Jaffe of Harvard University. They have been life-long friends and confidants of Konrad. (My text accompanies the one of Arthur.)

I first met Konrad casually when I followed an undergraduate course on analytical mechanics taught at ETH by the late Walter Hunziker during the winter 1966/67, with Konrad as one of his assistants. A little after the beginning of the semester, Walter and Konrad had returned from military training in the south of Switzerland, the Ticino, to Zurich, Walter as a captain (later to become a militia brigade general) and Konrad as a lieutenant of artillery in the Swiss army. The students in that course perceived both of them as very clear, systematic and pedagogical teachers, a quality that Konrad would continue to excel in.

The next occasion when Konrad and I met each other was in a seminar on Baruch Spinoza's *'Ethics'* organized by the philosopher Gerhard Huber. Spinoza's thinking became a "cantus firmus" among Konrad's life-long philosophical interests.

In 1968, on rare excursions to the "Seminar für theoretische Physik" (then the official denomination of the Institute of Theoretical Physics of ETH Zurich) I ran into Konrad again; but I also caught a glimpse of another brilliant youngster: Arthur Jaffe. On invitation by Klaus Hepp and Res Jost, Arthur taught a course on Constructive Quantum Field Theory at ETH; and Konrad wrote lecture notes for his course. This must have been beneficial for Konrad's work on a PhD thesis under the supervision of Klaus, analyzing the $\lambda\phi^3$- theory with a spatial cutoff in four space-time dimensions. Konrad showed that the Hamiltonian of this theory, after proper renormalization, is a symmetric operator on a dense domain of definition in Fock space (constructed with the help of so-called 'dressing transformations,' which Jim Glimm had previously devised to analyze $\lambda\phi^4$-theory in three dimensions). Unfortunately, $\lambda\phi^3$-theory is not a physical theory; it does not have a vacuum state. Some of the young Zurich mathematical physicists, including Robert Schrader, Konrad and myself, idolised Jim and Arthur, and, at the beginning of our careers, we greatly profited from their inspiration and support.

At the celebrated 1970 Les Houches Summer School on *"Statistical Mechanics and Quantum Field Theory,"* organized by the late Cécile De Witt and Raymond Stora, I finally got to know more closely Konrad, my PhD-advisor Klaus, Arthur and Jim and quite a few other luminaries of mathematical physics, including Elliott Lieb, David Ruelle, Robert Griffiths, the late Oscar E. Lanford III, fellow students, such as Alain Connes and Barry Simon, and others. Konrad and I had the duty to polish the notes of Klaus' famous lectures on renormalized perturbation theory in relativistic quantum field theory. For, Klaus could only spare two out of eight weeks to spend at Les Houches and hence was not able to provide the students with a polished version of his lecture notes before leaving that uniquely beautiful place in the French Alps. – Well, I could go on to write much more about the memorable 1970 summer, which had a lasting impact on the research interests of some of us youngsters..

In the fall of 1970, Konrad, his wife Vreni and their baby Kathrin left for the United States.

Konrad became a postdoctoral researcher in Jim Glimm's group at the Courant Institute. There he wrote two papers on Haag duality for free fields, one in collaboration with Jean-Pierre Eckmann, wherein they used some elements of Tomita-Takesaki theory, a theory in the field of operator algebras with origins in Jost's analysis of the CPT theorem, a paper of Haag, Hugenholtz (who recently died at the age of 101 years) and Winnink on equilibrium states in statistical mechanics, and Tomita's subsequent magical ideas. As is well known, Haag duality played an important role in the Doplicher-Haag-Roberts analysis of superselection rules in algebraic quantum field theory and has now a renaissance in the analysis of certain quantum lattice systems representing topological insulators. Tomita-Takesaki theory would turn out to play a crucial role in deep mathematical discoveries of that brilliant student of the 1970 Les Houches school, Alain Connes.

In 1971, the Osterwalder family moved on to Cambridge MA, where Konrad joined Arthur and his group at Harvard. Konrad was to advance to the positions of Assistant Professor and Associate Professor at Harvard. The family grew: A second daughter, Rachel, was born, to be followed, in 1973, by a son, Andreas.

Konrad started to collaborate with the late Robert Schrader, Klaus' first PhD student at ETH, whom Konrad knew from Zurich, on a variety of problems related to the Euclidian-field-theory description of fermions and, in particular, on a Feynman-Kac formula for quantum field theories of fermions.

Arthur then suggested to them to study the general connection between Euclidian (imaginary-time-) field theory and real-time relativistic theories, à la Wightman. This would lead to Konrad's and Robert's very highly cited and best known work, namely the **Osterwalder-Schrader axioms** and the **Osterwalder-Schrader reconstruction theorem**.

In the fall of 1973, I was invited to join Arthur and Konrad at Harvard as a research fellow, just when some of the crucial final touches had to be added to the Osterwalder-Schrader reconstruction theorem. This was a somewhat stressful period for Konrad, and I recall that he said more than once that he would like to escape to a pasture in the Swiss Alps, or that he withdrew to his study to play the violin.

I spent the first four weeks of our life in the US at the hospitable house of Vreni and Konrad in Winchester, busy setting up our own apartment at Watertown. This is when we really got to know each other well. I could share many recollections about that period, in particular about common friends, such as – besides Arthur – Sidney Coleman, Shelly Glashow, the Leutwilers, the Liebs, Azim Yildiz and others, as well as various distinguished senior professors of mathematics, such as George Mackey, Raoul Bott, and their spouses.

In the spring of 1974, Konrad started to collaborate with Joel Feldman on the cluster expansion for the $\lambda\phi^4$- theory in three space-time dimensions, leading to a construction of the thermodynamic limit and a proof of a positive mass gap for the weakly coupled theory. This work kept them busy until the fall of that year when Jim and Arthur animated a program on constructive quantum field theory at Aspen that we attended. It coincided with Nixon's resignation as president of the United States. At Aspen, one could spot at a distance celebrities like Dick Feynman, Steve Weinberg and Steve Frautschi, or make an appointment for a hike or a cup of coffee with Hans Frauenfelder. Our colleague and friend, the late Peter Minkowski with his family, was there, too; an occasion to become friends with him.

I drove my small family in the Osterwalder car, a spacious Ford station wagon, from Boston to Aspen, and Konrad drove his family back in their car from Aspen to Boston. Most unfortunately, the engine broke down somewhere near Northampton, and Arthur kindly went to fetch the Osterwalders there. Shortly after they had arrived at their house in Winchester, late in the evening, there was a shootout nearby, and everybody still awake lay down flat on the floor.

The next day, my wife and I drove from Winchester, where we had spent our last night in Massachusetts, to Princeton, our destination for the next three and a half years. Luckily my contacts with Arthur and Konrad remained lively.

In 1975, Arthur invited our dear friend Erhard Seiler to spend a year at Harvard. Konrad and Erhard collaborated on lattice gauge theory, which resulted in another highly-cited paper. I might mention that, some years later, Erhard, Konrad and I wrote a pure-math paper that dealt with what we dubbed "virtual representations" of symmetric spaces and has applications in QFT on space-times more general than Minkwoski space. Osterwalder-Schrader positivity was a crucial ingredient of our analysis. (Erhard and I completed this work at the IHES, and we had a tremendous amount of fun collaborating to late night hours.)

In 1977, the Osterwalders returned to Switzerland, and Konrad became a professor at the Math Department of ETH Zurich. He developed a reputation as a superb teacher. He became the advisor of a certain number of PhD students, among whom I should mention Emil J. Straube, Andrzej (Andrew) Lesnewski, and Felix Finster.

Konrad's best known work from his years at ETH is his work with Arthur and Andrew on a certain cocycle in Connes' cyclic cohomology, the celebrated *JLO-cocycle*. It serves to study topological properties – invariants – of noncommutative spaces.

In 1982, my family and I also returned to Switzerland, and I started to teach at the Physics Department of ETH, which, alas, is many miles away from the Math Department, so that interactions between Konrad and myself were rather weak.

In 1995, Konrad became the rector of ETH, and, in 2007, he became its interim president, after a former president had resigned. Konrad was one of the main promoters of the Bologna process, replacing ETH's diploma studies by Bachelor-Master studies. He also helped setting up the celebrated Monte-Verità Bauhaus hotel above Ascona as a conference center – a very wonderful one. I felt honored that he asked me to organize the very first conference there, featuring lectures by Arthur, Is Singer, Raoul Bott, and many others.

After retiring from ETH in 2007, Konrad became the United Nations Undersecretary General in Japan, where he built up the United Nations University in Tokyo and visited many destinations in Asia. He returned to Switzerland in 2013. Some time later, first signs of an eerie illness started to appear that recently killed him. With much generosity and empathy, Klaus Hepp continuously accompanied Konrad on the last stretch of his life.

Jürg Fröhlich
Prof. em.
ETH Zurich

2027 IUPAP Early Career Scientist Prize in Mathematical Physics

Call for Nominations

IUPAP Commission C18 (Mathematical Physics) calls for nominations for the IUPAP Early Career Scientist Prize, formerly known as Young Scientist Prize, in Mathematical Physics.

The prize recognizes exceptional achievements in mathematical physics by scientists at relatively early stages of their careers. It is awarded triennially to at most three young scientists satisfying the following criteria:

- The recipients of the awards in a given year should have a maximum of 8 years of research experience (excluding career interruptions) following their PhD on January 1 of that year (in this case that is 2027).
- The recipients should have performed original work of outstanding scientific quality in mathematical physics.
- Preference may be given to young mathematical physicists from underrepresented groups and geographical regions.

The awards will be presented at the ICMP in August 2027 in Da Nang (Vietnam). A nomination should include a brief description of the achievements of the candidate that support the nomination, a CV, and a list of publications (or current links to that information online).

Please submit nominations to:

- Daniel Ueltschi (secretary, ueltschid@gmail.com)
- Jan de Gier (vice-chair, jdgier@unimelb.edu.au)
- Simone Warzel (chair, warzel@ma.tum.de)

The deadline for nominations is October 15, 2026. For further information, including past recipients, see

https://iupap.org/commissions/c18-mathematical-physics/c18-awards/

Time’s Arrow

Scientific anniversaries

100 years ago, in 1926, Erwin Schrödinger published six papers “quantization as an eigenvalue problem” in which he laid ground to wave mechanics and formulated the Schrödinger equation. See the article by Norbert Straumann in this issue.

50 years ago, in 1976, Jürg Fröhlich, Barry Simon, and Tom Spencer, published their work on infrared bounds and continuous symmetry breaking, the first rigorous proof of spontaneous breaking of continuous symmetries in (non-mean-field) models in dimension at least 3.

[*Physical Review Letters* **36**, 804 (1976) and *Commun. Math. Phys.* **50**, 79–85 (1976)]

Personal Celebrations

Lászlò Erdős 60, April 14, 2026

Lost luminaries

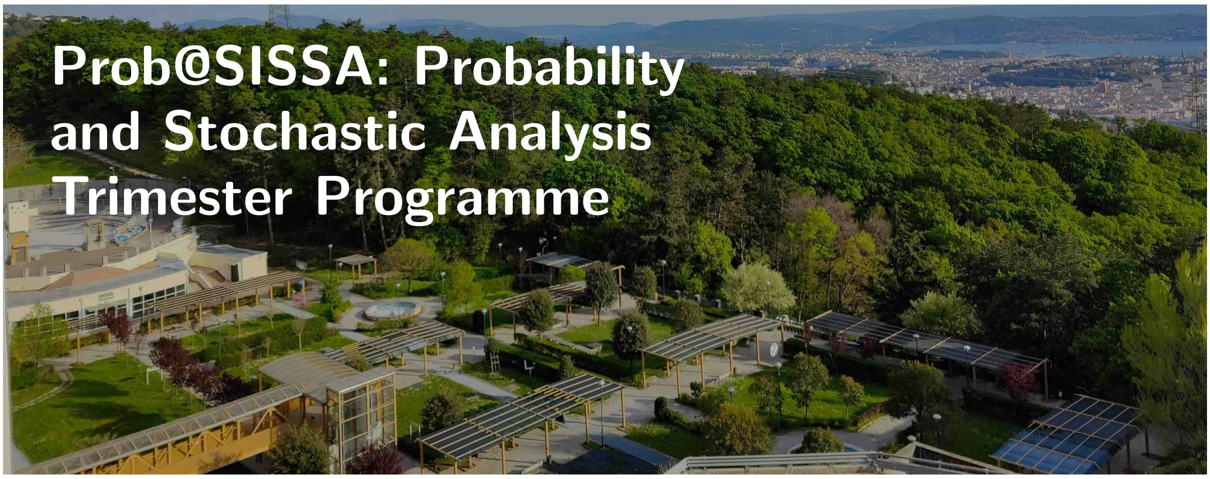
Thomas Hoffmann-Ostenhof (1945 – 2026)

Nicolaas Hugenholtz (1924 – 2026)

Konrad Osterwalder (1942 – 2025)

Armin Uhlmann (1930 – 2026)

Readers are encouraged to send items for “Time’s Arrow” to salmhofer@uni-heidelberg.de.



The trimester programme **"Probability and Stochastic Analysis"** will take place at **SISSA** (International School for Advanced Studies), **Trieste**, from **14 September to 11 December 2026**. Its scientific focus is probability theory and stochastic analysis, with emphasis on stochastic PDEs, random matrices, statistical mechanics, and probabilistic methods in QFT.

The programme features **two workshops** (**28 September - 2 October 2026** and **7 - 11 December 2026**) and **four courses** delivered by Paul Bourgade, Giuseppe Cannizzaro, Thomas Leblé, and Hao Shen.

Participation is free and open, but spaces are limited; travel and accommodation support may be available upon request. Financial-support deadlines: **1 July 2026** (Workshop 1) and **1 October 2026** (Workshop 2).

For further information and registration:

https://sites.google.com/view/probsissa

Organisers: Ilya Chevyrev, Gaultier Lambert, Hao Shen.

News from the IAMP Executive Committee

Call for Nominations for the 2027 Henri Poincaré Prize

The IAMP Executive Committee wishes to remind all members that the call for nominations for the 2027 Henri Poincaré Prize is currently open. The deadline is **September 30, 2026**. A nomination should include the following:

- Description of the scientific work of the nominee emphasizing their key contributions
- A recent CV of the nominee
- A proposed citation should the nominee be selected for an award

Nominations should be sent to the President (president@iamp.org) or to the Secretary (secretary@iamp.org) of the IAMP. Please also spread the word among your colleagues who are not IAMP members. Note that members of the Executive Committee of the Association are not eligible for nomination for the prize (see http://www.iamp.org/page.php?page=page_about). For past winners see http://www.iamp.org/page.php?page=page_prize_poincare.

New individual members

IAMP welcomes the following new members

1. Prof. Dohyun Kim, Sungkyunkwan University, Korea
2. Miraj Pothak, Softwarica College of IT and Coventry University, Nepal and United Kingdom
3. Sacha Amiel, Université Claude Bernard Lyon 1, France
4. Hai Chau Nguyen, Université Claude Bernard Lyon 1, France
5. Dr. Alessandra Frabetti, Université Claude Bernard Lyon 1, France
6. Dr. Cheng Yuan, Buffalo University, USA

Recent conference announcements

Operator Theory and Mathematical Physics

May 24 - 29, 2026; Prague (Czech Republic).

Recent developments in mathematical physics and related fields

May 27 - 29, 2026; Hagen (Germany.)

ICM 2026 Satellite Conference: Partial Differential Equations and Spectral Theory

June 8 - 10, 2026; Baylor University, Waco, TX (USA).

Lake Como School "Dynamics of Quantum Systems and Nonlinear Waves

June 15 - 19, 2026; Como (Italy).

Relativistic Thermodynamics: From Mathematical and Conceptual Foundations to Applications

June 15-19, 2026; Newcastle (UK).

Arbeitsgemeinschaft: From Trees to Bands: Discrete Localization, Unique Continuation, and Propagation

October 4 - 9, 2026; Oberwolfach (Germany).

More forthcoming meetings.

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`

Chiara Saffirio (IAMP Secretary)

NORBERT STRAUMANN
Physik-Institut
Universität Zürich
Winterthurerstrasse 190
CH-8057 Zürich, Switzerland
sekretariat@physik.uzh.ch

ARTHUR JAFFE
Departments of Mathematics and Physics
Harvard University
Cambridge, MA, USA
Arthur_Jaffe@harvard.edu

JÜRG FRÖHLICH
Theoretische Physik
ETH Zürich
CH-8092 Zürich, Switzerland
juerg@phys.ethz.ch

CHIARA SAFFIRIO
Departement Mathematik und Informatik
Universität Basel
Spiegelgasse 1
CH-4051 Basel, Switzerland
secretary@iamp.org

MANFRED SALMHOFER
Institut für Theoretische Physik
Universität Heidelberg
Philosophenweg 19
69210 Heidelberg, Germany
salmhofer@uni-heidelberg.de

RAFAEL BENGURIA
Instituto de Física, Facultad de Física
P. Universidad Católica de Chile
Av. Vicuña Mckenna 4860
Macul, 7820436, Santiago, Chile
bulletin@iamp.org