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In Memoriam Erik G.F. Thomas (1939–2011)

"A good definition is half the work"

Erik G.F. Thomas, professor of mathematics at the University of Groningen, passed away on 13 September 2011 at age 72. His colleagues and former colleagues Boele Braaksma, Tom Koornwinder (coordinator), Jan Stegeman, Jacques Faraut, Gerrit van Dijk, Leo van Hemmen and Tony Dorlas look back on his life and work.

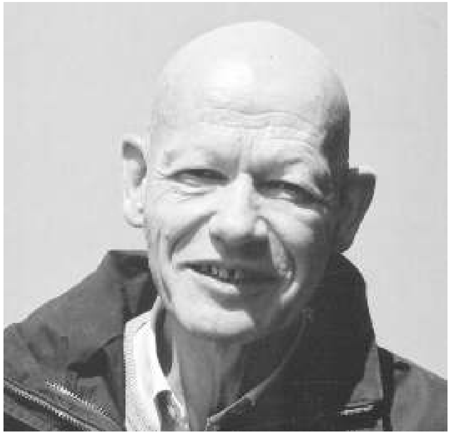
Erik Thomas was born in The Hague on 19 February 1939. He died in Groningen on 13 September 2011. He is survived by his wife Gerda and his daughters Karin and Christine.

Life and career (by Boele Braaksma)
Erik Thomas studied mathematics at the University of Paris, where in 1969 he obtained his PhD on the thesis *L'intégration par rapport à une mesure de Radon vectorielle* published in *Annales de l'Institut Fourier* [1]. His advisor was Laurent Schwartz, a Fields medalist, whose best known achievement is the foundation of the theory of distributions. After obtaining his PhD he stayed for a year as maître de conférence in Orsay before taking up the post of assistant professor of mathematics at Yale University. In 1973 he was appointed professor of mathematics at the University of Groningen.

Because of his background he brought a rich mathematical culture with him to Groningen. Erik was a passionate mathematician who conveyed his knowledge and enthusiasm to his many students and colleagues, and who often influenced them a great deal. His lectures and seminars were lucid and inspiring and showed many facets of the beauty of mathematics. He could explain very complicated pieces of mathematics in a transparent manner. His students were enthusiastic about his lectures and his inspiring personality. His door was always open to students and colleagues and he answered their questions with much care and without regard of his time.

Many problems posed to him came from other disciplines, in particular from Theoretical Physics and Applied Mathematics. Until recently he had a close collaboration with his colleague Joop Sparenberg from Technical Mechanics. Consequently he was advisor for several theses from these areas. Erik supervised his PhD students very closely and he had much influence on them. Their theses were valuable contributions to mathematics.

Erik had extremely high standards in his research. Although he had several unpublished works lining his shelves and despite the 'publish or perish' atmosphere during the


Erik Thomas, circa 2000

later part of his career, he submitted only his very best manuscripts for publication.

He also took his share in the administrative duties. From 1992 until 1994 he was chairman of the (nowadays Koninklijk) Wiskundig Genootschap. He remains in our memory as a man of the highest integrity and as both an inspired and inspiring mathematician of high calibre with a great desire to share his love and knowledge of mathematics with others.

Research (by Tom Koornwinder)

Erik Thomas has published 48 research papers. All of them are reviewed in *Zentralblatt MATH* (but only 39 are found there with the author identification `thomas.erik-g-f`, of which two are wrong), while MathSciNet misses two of these (but gives the other 46 with the author identification `Thomas, Erik G. F.`). Curiously, MathSciNet misses his 101 page AMS Memoir from 1974. His papers can be divided in six categories:

- *Integration with respect to vector-valued Radon measure* (10 papers, 1968–1997). This is the subject of his thesis [1] in Orsay. See the contributions by J. Faraut and J. Stegeman. The already mentioned AMS Memoir [2] also belongs to this subject.
- *General functional analysis and integration theory* (10 papers, 1965–1995). A paper of 60 pages with Alain Belanger in *Canad. J. Math.* [5] falls under this subject, but it also relates to Analysis on Lie groups.
- *Integral representation in convex cones* (8 papers, 1978–2004; 1 unpublished, 2011). See the contribution by J. Faraut.
- *Analysis on Lie groups* (9 papers, 1984–2005). See the contributions by J. Faraut and G. van Dijk. His work on this subject was strongly influenced by his expertise on the previous subject.
- *Applied mathematics* (4 papers, 1981–2002; 2 reports, 1997). Under this heading fall collaborations with his colleagues J.A. Sparenberg and J.C. Willems (see also the contribution by B. Braaksma), and with his former PhD student J.L. van Hemmen, as described in the contribution by Van Hemmen.
- *Path integrals* (7 papers, 1996–2008). This was the subject in which he was much interested during the last part of his career. See the contribution by T.C. Dorlas, with whom he wrote his last published paper.

Of course, there is much interrelation between these categories. In particular they are all fed by his impressive knowledge and mastership of functional analysis and integration theory. But he was the opposite of a narrow specialist who only publishes ever increasing technicalities in his own field. He enjoyed inspiration, interaction and collaboration with people from other fields, both pure and applied mathematics and also physics. But he avoided long series of papers with the same co-authors. He had 12 different co-authors with whom he wrote 11 papers.

A similar pattern can be seen from the subjects of the PhD theses under his guidance. According to the Mathematics Genealogy Project Thomas has had ten PhD students. For three of them (Klamer, Pestman and Capelle) he was the only advisor. All three wrote a thesis in Analysis on Lie groups. The other seven theses are with co-advisors, often on applied topics, and sometimes defended at another Dutch university.

Personally I got a closer acquaintance with Erik when he started to come regularly to the sessions of the Analysis on Lie groups seminar during the eighties (see also the contribution by G. van Dijk). His own lectures there were marvellous. But it gave also a great added value to a session if Erik was in the audience. By his frequent questions he really wanted to understand what was said by the speaker, and thus helped the speaker as well to understand his own stuff better.

Erik brought joy and enthusiasm to the annual sessions on Lie theory that Gerard Helminck organized at Twente University for a few days before Christmas. Even, in later years, as Erik battled his disease, he kept attending. His curiosity remained, and he could inspire us as he always had done.

Orsay (door Jan Stegeman)

Graag wil ik iets vertellen over de bijzondere relatie die ik meer dan veertig jaar met Erik Thomas heb gehad. Onder de Nederlandse wiskundigen neemt Erik een bijzondere plaats in, omdat hij niet in ons land heeft gestudeerd. De middelbare school bezocht hij in Engeland. Aanvankelijk was het de bedoeling dat hij in Cambridge zou gaan studeren, maar uiteindelijk kwam hij in Orsay terecht, 25 kilometer ten zuiden van Parijs (tegenwoordig onderdeel van de Université Paris-Sud). In zijn eerste studiejaar raakte hij gefascineerd door een analyse-college van Jacques Deny. Dit zou bepalend zijn voor Eriks latere carrière. Later kwam hij in contact met Laurent Schwartz, de vader van de distributietheorie. Deze gaf hem enkele problemen die Erik wist op te lossen, hetgeen uiteindelijk tot zijn promotie op 12 juni 1969 heeft geleid.

Doordat ik in 1969 een aanstelling in Orsay had om aan mijn promotieonderzoek te werken, kwam ik met Erik in contact. Als landgenoten trokken wij regelmatig met elkaar op. Erik maakte mij wegwijs in de Franse wereld, en anderzijds was hij geïnteresseerd in het wiskundige leven in Nederland, want daar wist hij heel weinig van. Ik heb toen voorgesteld om samen het paascongres van het Wiskundig Genootschap bij te wonen, dat dat jaar in Wageningen gehouden werd. Daar had ik de gelegenheid hem bij diverse Nederlandse wiskundigen te introduceren. Ik vlei mij soms met het idee dat mede daardoor Erik enkele jaren later er toe kwam om vanuit Amerika te solliciteren naar een lectoraat in Groningen.

Erik en ik hebben altijd contact gehouden, zowel wiskundig als op het persoonlijke vlak. Bij mijn afscheid in Utrecht in 2000 was Erik bereid iets over mijn werk te vertellen, terwijl ik drie jaar later bij Eriks afscheid een verhaal over Erik heb mogen houden 'vanuit een historisch perspectief'. Een samenvatting van dit verhaal is opgenomen in het Groningse *Alumnieuws*, no. 12, maart 2004. Men kan dit nalezen op http://www.cs.rug.nl/jbi/History/Thomas.

Terugblikkend op mijn verblijf in Orsay besef ik wat een geweldig instituut dat was, waar Erik bijna tien jaar heeft kunnen studeren en werken. Ter illustratie wil ik enkele verdere namen noemen van wiskundigen (meest analytici) die ik daar in 1969 heb mogen ontmoeten: Jean-Pierre Kahane, Pierre Eymard, Nicholas Varopoulos, Michel Demazure, Henri Cartan, Pierre Cartier, Paul Malliavin, Antoni Zygmund, Lennart Carleson, Carl Herz, Adrien Douady, Jacques Faraut, Yves Meyer. Men kan zich voorstellen hoe Eriks talenten, waarvan het Groningse mathematisch instituut zoveel profijt heeft gehad, zich in zo'n omgeving hebben kunnen ontwikkelen.

Paris years and after (by Jacques Faraut)

I met Erik Thomas in Paris at the beginning of the 60's, when we were both students. Since then we kept in touch, and we had still recently exchanges by mail and phone.

I recall with pleasure the time we were both assistants at the University of Orsay, Paris-Sud. Erik was showing much enthusiasm for mathematics, communicating his enthusiasm on every occasion. For instance, I remember one evening in a restaurant in Paris when Erik was explaining quantum mechanics to me. Suddenly, all people at neighbouring tables stopped talking and listened to Erik's explanations. At the University we organized together a workshop for the students, something rather unusual for assistants in these days.

At that time Erik Thomas was writing his thesis under the prestigious supervision of Laurent Schwartz. The defense was a real event, which was attended by a large number of mathematicians.

After his doctorate, Erik Thomas worked for some years at American universities. While I was visiting him at Yale University, New Haven, Erik introduced me to the New York life.

Let me say a few words about some of his main mathematical achievements.

The thesis of Erik Thomas, defended in 1969, is devoted to the integration with respect to a vector-valued Radon measure. For a locally compact topological space T and a real Banach space E, a Radon measure μ on T with values in E is a continuous map $\mathcal{K}(T) \to E$, where $\mathcal{K}(T)$ denotes the space of real-valued continuous functions with compact support. (More generally one considers a locally convex topological vector space E which is quasi-complete.) One defines first the *semi-variation* of a function $f \geq 0$ by $\mu^*(f) = \sup_{|\varphi| \leq f,\ \varphi \in \mathcal{K}} \|\mu(\varphi)\|$, and then the space $\mathcal{L}^1(\mu)$ of integrable functions f with the property: $\forall \varepsilon > 0 \quad \exists \varphi \in \mathcal{K} \quad \mu^*(|f - \varphi|) \leq \varepsilon$. Then the map μ extends continuously to $\mathcal{L}^1(\mu)$ and, for $f \in \mathcal{L}^1(\mu)$, this defines $\int f\, d\mu$ as an element of E.

The thesis originated from two questions posed by L. Schwartz:

- Is there a dominated convergence theorem?
- Let the function f be *scalar-integrable* with respect to μ, i.e., integrable for all the scalar measures $\mu_{x'} = x' \circ \mu$ ($x' \in E'$, the dual of E). The *weak integral* of f is the linear form on E' given by $\langle \text{w-}\int f\, d\mu, x' \rangle = \int f\, d\mu_{x'}$. The question is: Does w-$\int f\, d\mu$ belong to E?

For the first question Thomas introduced the notion of extendable measure (*mesure prolongeable*). For such a measure every bounded Borel function with compact support is integrable. Thomas established a dominated convergence theorem for these measures.

For the second question Thomas introduced the notion of Σ-completeness. A Banach space E is Σ*-complete* if, for every sequence (x_n) with the property that $\sum_{n=1}^{\infty} |\langle x_n, x' \rangle| < \infty$ for all $x' \in E'$, there exists $x \in E$ such that $\sum_{n=1}^{\infty} \langle x_n, x' \rangle = \langle x, x' \rangle$ for all $x' \in E'$. Then Thomas proved: If the Banach space E is Σ-complete then a scalar-integrable function f is integrable, and the weak integral of f agrees with the integral of f. The thesis, which contains a large number of other results, has been published in extenso in the *Annales de l'Institut Fourier* [1].


Lecturing at Lorentz Center, Leiden, 2004

In a next period Thomas' interest focused on the Choquet theory of integral representation in convex cones. For a convex cone Γ in a locally convex topological vector space E consider a parametrization $t \mapsto e_t : T \to \mathrm{ext}(\Gamma)$ of the extremal rays. The problem is, for $f \in \Gamma$, to establish the existence and uniqueness of a measure m on T such that $f = \int_T e_t\, dm(t)$. By Choquet's Theorem such a measure exists if the cone Γ is well-capped, and it is unique if the cone Γ is a lattice with respect to its proper order. In the paper 'Integral representations in conuclear cones' [6], Thomas replaces the condition that Γ is well-capped by: E is a quasi-complete *conuclear space* (a notion too technical to be explained here) and the order intervals $\Gamma \cap (f - \Gamma)$ are bounded.

Thomas' condition is more general, and, in some instances, easier to be checked. Classical applications are Bernstein's and Bochner's theorems [13].

Thomas was very eager to find further applications. Generalizations of the Bochner–Schwartz theorem can be obtained as applications of Thomas' results. For a Lie group G, the space $\mathcal{D}'(G)$ of distributions is conuclear. The set Γ of distributions of positive type is a convex cone with bounded intervals. In general, for non-commutative G, the cone Γ is not a lattice. However the subcone of central distributions of positive type is a lattice. This leads to a generalization of the Bochner–Schwartz theorem for unimodular Lie groups.

For a compact subgroup $K \subset G$, the cone Γ_K of K-bi-invariant distributions on G of positive type is a lattice if and only if (G, K) is a Gelfand pair. This leads to the notion of generalized Gelfand pair. For a closed subgroup $H \subset G$, not assumed to be compact, both G and H unimodular, the following three properties are equivalent:

- For each irreducible unitary representation on a Hilbert space $\mathcal{H}$ the space of H-invariant distribution vectors has dimension at most 1.
- For a unitary representation π realized on a G-invariant Hilbert subspace $\mathcal{H}$ of $\mathcal{D}'(G/H)$ the commutant of $\pi(G)$ in End$(\mathcal{H})$ is commutative.
- The cone Γ_H of H-bi-invariant distributions of positive type on G is a lattice.

If these equivalent properties hold, then the space G/H is said to be *multiplicity free*, or the pair (G, H) to be a *generalized Gelfand pair*, and for such a pair there is a Bochner–Schwartz–Godement theorem [3].

Consider the Heisenberg group $H_n = \mathbb{C}^n \times \mathbb{R}$ with the product $(z, t)(z', t') = (z + z', t + t' + \mathrm{Im}\,(z'|z))$. The unitary group $U(n)$ acts on H_n by automorphisms, and $(U(n) \ltimes H_n, U(n))$ is a Gelfand pair. The closed subgroups $K \subset U(n)$ such that $(K \ltimes H_n, K)$ is a Gelfand pair have been classified by Carcano. On the other hand, it has been proved by van Dijk and Mokni that $(U(p,q) \ltimes H_n, U(p,q))$ is a generalized Gelfand pair. In [10] Mokni and Thomas obtain an analogue of Carcano's result for non-compact groups H by determining for which closed subgroups H in $U(p,q)$ the pair $(H \ltimes H_n, H)$ is a generalized Gelfand pair.

I had the chance to write with Thomas a paper [11] about the decomposition of unitary representations which are realized on Hilbert spaces of holomorphic functions, We gave a geometric criterion for multiplicity-free decomposition. (Our result has been reformulated in a much wider setting by T. Kobayashi, who introduced the concept of *visible action*.)

Thomas has been a very active mathematician until the last months of his life. An unpublished paper [15] written in June 2011 deals with multivariate completely monotonic functions.

A true analyst (by Gerrit van Dijk)
With Erik Thomas I shared a continuing interest in Gelfand pairs. It all started in 1979 with the doctoral dissertation of Erik's student F.J.M. Klamer, entitled *Group representations in Hilbert subspaces of a locally convex space*, for which I was asked to serve in the examination committee.

The theory developed by Klamer appeared to have immediate implications for my own work on Gelfand pairs: pairs of groups (G,K) with K compact, with the property that every irreducible unitary representation of G, when restricted to K, contains the trivial representation of K at most once. Klamer's dissertation gave rise to an extension to pairs (G,H) with H a closed, not necessarily compact subgroup of G. This was a breakthrough which excited Erik and me. A lot of new questions arose. Would it be possible to generalize in some form the well-known criterion of Gelfand for showing that (G,K) is a Gelfand pair, to pairs (G,H)? On a beautiful day in July 1980 I received at my home address a letter from South Africa. Upon opening, the letter appeared to contain the solution. I was very thrilled by the elegance of the result, but I also wondered why this letter was posted in South Africa. Later this became clear to me: Erik was visiting South Africa to make his acquaintance with the family of his future wife Gerda.

After returning to the Netherlands, Erik reported extensively on his new mathematical results in the seminar 'Analysis on Lie Groups', chaired by Tom Koornwinder and myself. He proved, in passing, also an important result [4] for classical Gelfand pairs: if G is connected, then the pair (G,K) is a Gelfand pair if the algebra of G-invariant differential operators on G/K is commutative. A little later it dawned upon us that Helgason had proven the same result, almost simultaneously.

Erik always impressed us with an excellent presentation of his lectures. His enthusiasm infected us, his independent thinking roused admiration. The importance of a good presentation he also successfully emphasized to his students. I have been a witness of this several times because some of his master students later wrote their doctoral dissertation under my guidance. With the passing of Erik we have lost a pure analyst and a true colleague.

Clarity of exposition (by Leo van Hemmen)
Erik Thomas was striving for mathematical clarity all his life, both while teaching and while discussing open problems. The way in which he practiced this clarity was fascinating and at the same time totally convincing. I was effectively Erik's first graduate student. In fact, I had two doctoral thesis advisors, Nico Hugenholtz in theoretical physics and Erik Thomas in mathematics. My topic was 'ergodic theory' for a dynamical system with *a priori* infinitely many particles; in my case, the infinite harmonic crystal in thermodynamic equilibrium, a problem that I knew from my solid-state physics days.

What I learned from Erik while working on my doctoral dissertation *Dynamics and ergodicity of the infinite harmonic crystal* (University of Groningen, 1976; *Phys. Rep.* 65, 1980, 43–149) was focusing on total mathematical clarity. To quote him: "A good definition is half the work." How true, but easily forgotten. As for buying books: "Only the very best is good enough." This wise advice has saved me from the nuisance of having seductively cheap but in reality boring books looking down upon me. We always kept contact and two decades later we embarked on another project, that suited him even better.

Suppose we have a differential equation

$$\mathrm{d}x/\mathrm{d}t = f(x) + \varphi \tag{1}$$

in some Banach space E; for example, $\mathbb{R}^n$. The system $\mathrm{d}x/\mathrm{d}t = f(x)$ is autonomous. It is supposed to have an equilibrium state; without restriction we can take it to be $x = 0$, i.e., $f(0) = 0$. Quite often the full f is neither known nor accessible to experiment, except for some of its 'components' where also the time-dependent input φ lives, and which can be sampled experimentally.

What we are hunting for is the solution operator that generates the time evolution induced by (1). Since Volterra (*Theory of functionals and of integral and integro-differential equations*, Blackie, London, 1930; Dover, 1959) one has often represented the solution to (1) as a series, which one now calls the Volterra series, with respect to increasing powers of φ. A canonical representation of the Volterra series expansion for the scalar case $\mathbb{R}^n$ with $n = 1$ reads

$$\begin{aligned} x(t) = \kappa_0 + \sum_{n \geq 1} \int_{-\infty}^{t} \mathrm{d}s_1 \cdots \int_{-\infty}^{t} \mathrm{d}s_n \\ \cdot\, \kappa_n(t - s_1, \ldots, t - s_n) \\ \cdot\, \varphi(s_1) \ldots \varphi(s_n). \end{aligned} \tag{2}$$

At reception after G. van Dijk's farewell lecture, Leiden, 2004

With Gerda in Strasbourg, 2005, at a conference in honor of J. Faraut

Since $x = 0$ is our equilibrium point, we substitute $\varphi = 0$ and find $\kappa_0 = 0$. In real life $x(t)$, or part of it, is given experimentally and we would like to determine the kernels κ_n. In Banach space Erik Thomas, my former graduate student Werner Kistler and I could solve this problem fully [12].

A solution operator is said to be *nonanticipative*, or *causal*, if for each t the solution $\mathcal{A}(\varphi)(t)$ depends on the restriction of φ to $(-\infty, t]$ only, i.e., on the past of the input. What we have actually done is obtaining the unique nonanticipative solution operator $\varphi \mapsto \mathcal{A}(\varphi)$ for (1) so that $x = \mathcal{A}(\varphi)$ solves (1). The Volterra expansion (2) amounts to expanding the solution operator $\mathcal{A}(\varphi)$ into a Taylor series around $\varphi = 0$,

$$\begin{aligned} \mathcal{A}(\varphi) &= \sum_{n=1}^{\infty} \mathcal{A}_n(\varphi), \\ \mathcal{A}_n(\varphi) &= \frac{1}{n!}\, \mathrm{D}^n \mathcal{A}(0)(\varphi, \ldots, \varphi), \end{aligned} \tag{3}$$

where $\mathrm{D}^n \mathcal{A}(0)(\varphi_1, \ldots, \varphi_n)$ denotes the n-th order directional derivative at 0 in the directions $\varphi_1, \ldots, \varphi_n$, the dependence upon t being understood; a glance at (2) may be helpful. What, then, can be said about convergence of the series in (3) and, if so, in what sense? Here a fundamental manuscript of Erik Thomas [8, Theorem 6.1] comes in where he introduces the notion of quotient-analytic maps in locally convex spaces, or for short *Q*-analyticity. The manuscript was intended to precede our common paper [12] but the journal found its mathematics too 'pure'. There is also a nice companion note [9], which needs to be included as well.

A few more words on the convergence of the Volterra series (3) are in order. The solution operator $\mathcal{A}(\varphi)$ is not analytic but 'quotient-analytic' (*Q*-analytic) in φ, as set forth by Thomas [8]. Neither is the convergence in (3) uniform in t. The consequence of the novel notion of *Q*-analyticity is that a Volterra series such as (3) converges uniformly for t in a compact interval I and for inputs φ in a neighborhood of zero that *depends on* the compact interval I under consideration.

Furthermore, on the basis of (3) we can now conclude [12] that the kernels κ_n in (2) exist. But how to determine them? This is a classical problem for instance in neuroscience, which Wiener had already tried to solve. Let us work on the real line and substitute $\varphi = \lambda\delta_{t_\circ}$ into (1) and hence (2); here $\delta_{t_\circ}$ is a Dirac measure (unit mass) at $t_\circ \in \mathbb{R}$. Substituting a Dirac measure ('delta function') into (1) is opposite to what Wiener proposed but we will see in a minute why it all fits. Equation (2) now reads

$$x_\lambda(t) = \sum_{n\geq 1} \lambda^n \kappa_n(t - t_\circ, \ldots, t - t_\circ)\,, \tag{4}$$

which, for t in a given compact interval, converges uniformly. Differentiating (4) once with respect to λ at $\lambda = 0$, which we may do because convergence is uniform, we find $\kappa_1(t - t_\circ)$. Similarly, by substituting $\varphi = \lambda_1\delta_{t_1} + \lambda_2\delta_{t_2}$ into the series (3) and hence (2), and differentiating with respect to λ_1 and λ_2 at 0 we obtain $\kappa_2(t - t_1, t - t_2)$. We have developed [12] an algorithm, *differential sampling*, to obtain the n-linear $\mathcal{A}_n$ for arbitrary n through recurrence relations in Banach space, showing that the $\mathcal{A}_n$ are actually represented by continuous kernels κ_n.

What Wiener (*Nonlinear problems in random theory*, MIT Press, 1958) did was substituting white noise (wn) for φ, averaging (arithmetically) over ℓ runs (while taking advantage of the strong law of large numbers), and exploiting a key property of white noise in that its mean gives a Dirac delta measure: $\langle \varphi_{\mathrm{wn}}(t)\varphi_{\mathrm{wn}}(t+s)\rangle = \delta_s$ with, as usual, $\langle \cdots \rangle$ denoting the stochastic mean. To see how this works, we take a simple example, viz., the linear case in (2) with $x(t) = \int \mathrm{d}s\, \kappa_1(t-s)\varphi(s)$, multiply this by $\varphi_{\mathrm{wn}}(t')$, average over finitely many runs and obtain as approximation for ℓ finite but large

$$\begin{aligned} &\langle x(t)\varphi_{\mathrm{wn}}(t')\rangle \\ &\quad = \int_0^\infty \mathrm{d}s\, \kappa_1(t-s)\langle \varphi_{\mathrm{wn}}(s)\varphi_{\mathrm{wn}}(t')\rangle \\ &\quad = \int_0^\infty \mathrm{d}s\, \kappa_1(t-s)\delta(s-t') = \kappa_1(t-t'). \end{aligned} \tag{5}$$

Generating white noise is nontrivial, so why not use a delta measure as input? White noise has been used extensively in e.g. auditory experiments but why not use a click? A click sounds like, and is, an approximate delta measure (see W. A. Yost, *Fundamentals of hearing*, Academic Press, 1994), but the approximation is easy to produce and quite good. However, for using Dirac delta measures as input φ the kernels κ_n must be continuous. Together with Erik Thomas [12, Section 3] we could prove that they are even real analytic, under the fairly general condition of f in (1) being an analytic function satisfying a Lipschitz condition, which suffices for most purposes.

Now one could complain that white noise may well be hard to generate but in experiment a delta function is not perfect either. True. That is why we have also proven a continuity theorem [12, Section 6] showing that for $E = \mathbb{R}^N$ the approximate kernels approach the exact ones as the sampling, approximating, click becomes an exact Dirac delta measure. In passing we note that, though quite a bit clumsier, white-noise averaging is completely justified too once the kernels κ_n such as those in (2) are continuous.

Looking backwards, I realize that working with Erik Thomas was a fascinating experience where we all greatly enjoyed his deep insight, his clear explanations, and his great enthusiasm for clarifying why mathematical structures give new insight. You 'only' need to see them, as Erik did.

Path integrals (by Tony Dorlas)
For me as a student at Groningen University, Erik Thomas was one of my favourite lecturers. I really intended to study theoretical physics but his inspiring lectures persuaded me to complete a degree in mathematics as well. He used to give his lectures entirely without notes and would often wear a round-necked jumper which he took off during the lectures. The lectures were always a model of clarity and organisation, showing a real mastery of the subject. Later, as a lecturer myself, it was always his lecturing style that I tried to imitate. Having just returned from Yale, and having studied in France, Erik sometimes used uncommon words during the lectures. Thus, I first learnt the word *digressie* (Dutch for *digression*) from him. Despite his encouragement, I nevertheless decided to do my PhD in theoretical physics, but kept in close contact with Erik through his weekly seminar, which touched on many interesting subjects, but especially harmonic analysis, which was his main interest at the time. It consisted of a small group of students, giving a number of lectures in turn, often studying a particular book or article. I have learnt a lot from those seminars, not just mathematics but also how to present a talk. Around Christmas time, Erik would often invite us home for dinner with his wife Gerda. Here his French habits were also apparent. We would have a cognac and a plate of lettuce before the main meal. The conversation was often quite philosophical in nature, in keeping with Erik's interests.

Although not trained in physics, Erik did have an interest in it and in the 90's he started work on the mathematical definition of the Feynman path integral. This is the *Lagrangian formulation* of quantum mechanics introduced by Feynman after a tentative suggestion by Dirac. Originally, Feynman formulated his path integral as an alternative way of expressing the solution of the Schrödinger equation in non-relativistic quantum mechanics, but then he generalised it to the relativistic case and quantum field theory. It proved to be a particularly useful tool in perturbation theory giving rise to his introduction of Feynman diagrams. This led to much shorter calculations of relevant quantities than the traditional Hamitonian approach.

However, the concept is still poorly understood in a mathematical sense. Many alternative formulations have already been suggested to give a mathematical meaning to this concept, but none is particularly satisfactory. The most fruitful to date is the *Euclidean approach*. Here one makes an 'analytic continuation' of the time variable to imaginary time. This turns the ill-defined oscillatory Feynman path integral into a well-defined Wiener integral. This was done by Kac, and is known as the *Feynman–Kac path integral*. It is such a powerful tool that many theoretical physicists today think in terms of Euclidean space rather than Minkowski space quite routinely. As Erik remarked, however, this does not really answer the question what mathematical entity corresponds to the Feynman path integral itself. It is known that it cannot be a (complex-valued) measure. Cecile De Witt-Morette suggested that it should be a distribution of some kind, but she did not give a more detailed construction.

A proper mathematical definition was given by Albeverio and Hoegh-Krohn after a suggestion by Ito, in terms of the Fourier transform of a bounded measure. Although this is indeed a proper mathematical formulation, Erik was not happy with it. He argued that the space of Fourier transforms of bounded measures is an unwieldy space. He initiated a new approach, exploring various simplified scenarios. One of those was to discretise space, another to discretise time.

At the time I was a lecturer at the University of Swansea, where professor Truman had also worked on the Feynman path integral. As I was interested myself, I invited Erik to Swansea, where we started a collaboration. At that time he had already worked out a discrete-time formulation [7], and we considered a possible continuous-time limit. Unfortunately, the result was negative. This discouraged me more than him, and I turned my attention to other projects. More than ten years later, on a visit to Groningen I discussed the matter again with him. It turned out that he had only published his work on a finite-space version but thought that it could be generalised to infinite discrete space. I offered to try and work this out, and sent him a draft version of a paper some time later. Although Erik was already unwell at that time, he nevertheless sent some comments and we communicated about its publication. We decided to send it to the *Journal of Mathematical Physics*, where it was accepted [14].

Just this year, I had a new post-doc (Matieu Beau) with whom I decided to work on the path integral again. So far, we have been able to extend Erik's work on the discrete-time integral, simplifying his approach somewhat and considering more general boundary conditions. Sadly, we will have to do without his comments and encouragement.

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