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Fabrizio Durante, Giovanni Puccetti*, Matthias Scherer, and Steven Vanduffel

The Vine Philosopher

An interview with Roger Cooke

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Roger Cooke received his PhD (1974) from Yale University in Mathematics and Philosophy. From 1975-2005 he worked in the Netherlands, first as assistant professor in Logic and Philosophy of Science at the University of Amsterdam, and later as professor of Applied Decision Theory in the Department of Mathematics at the Delft University of Technology. In 2005 he moved back to the USA as senior fellow at Resources for the Future. In 2006-2008 he supervised the development of non-parametric continuous-discrete Bayesian Belief Nets for the Dutch Ministry of Transport. Subsequent development was under contract with Shell, AIRBUS, and the National Institute for Aerospace. In 2008 he was elected fellow of the Society for Risk Analysis. In 2010 he was named lead author in the fifth assessment of the Intergovernmental Panel on Climate Change for the chapter on Risk and Uncertainty. In 2011 he received the Lifetime Distinguished Achievement Award from the Society for Risk Analysis. He currently works on uncertainty quantification in conceptual design for AIRBUS and on value of information of Earth Observation Missions for NASA Langley.

The sixth interview of this series features a conversation with Roger Cooke. Roger is widely known as one of the developers of the *vine copula* model, but his path to mathematics has been a long and winding road crossing philosophy and jazz. Cooke's early work focused on the foundations of quantum mechanics and probability. Since then he studied methodological issues of risk analysis, uncertainty analysis, and expert judgment, with forays into competing risk, design of reliability data bases, and stochastic processes. His 1991 book *Experts in Uncertainty* [8] is a standard in this field and his short course on *Expert Judgment* has been given several times at NASA Langley and NASA Headquarters. His 2001 book *Probabilistic Risk Analysis* ([2], with Tim Bedford) has been translated into Chinese and Japanese. The 2006 book *Uncertainty Analysis with High Dimensional Dependence Modeling* ([18], with Dorota Kurowicka) bundles much of that work. In the following, our questions to Roger Cooke are typeset in bold-face.

1 Career

You hold a PhD in Philosophy and Mathematics from Yale University (1974). Have you ever thought to become a professor of Philosophy?

This is a serpentine story. I started as a major in "Physics and Philosophy" at Yale University. However, that program turned out to be a failing one-man-show so I switched to Philosophy, while completing the elementary physics and math prerequisites. My BA is in Philosophy. Midway through my PhD in Philosophy I shifted my interest from German transcendental philosophy to logic, philosophy of science, foundations of mathematics, and took courses in the math department. The most notable influence was Shizuo Kakutani,

Fabrizio Durante: Dipartimento di Scienze dell'Economia, Università del Salento, Lecce, Italy.
***Corresponding Author: Giovanni Puccetti:** Dipartimento di Economia, Management e Metodi Quantitativi, Università di Milano, Italy. E-mail: giovanni.puccetti@unimi.it.
Matthias Scherer: Lehrstuhl für Finanzmathematik, Technische Universität München, Germany.
Steven Vanduffel: Faculteit Economische en Sociale Wetenschappen, Vrije Universiteit Brussel, Belgium.

who taught measure theory and functional analysis. I was drawn to him because he was such a kind and humble person, in addition to being a great mathematician. My PhD thesis was on the Mechanics of Heinrich Hertz – largely unsupervised. The great thing about philosophy is that it gives a warrant to enter every field – "Hello, I'm doing the philosophy of molecular biology…" – the downside is that no one takes you seriously. For my PhD I had to tool up my math and took a year off to study differential geometry, during which time I supported myself playing jazz in the Netherlands with Chris Hinze. I immediately felt at home in the Netherlands and met my future wife. The Vietnam War was raging and I was trying not to get conscripted.

After returning to the US and finishing my PhD, I searched for a job in Philosophy. Those were hard times, I had a few unattractive nibbles, and decided to go back to Holland in 1975 and play jazz with Chris Hinze. A career path is always a negotiation with ultimate goals, proximal goals, strategy, and tactics. I was serious about music; it was a goal, not a strategy. Learning to master an instrument and become a conduit for the music were very important for me. Most people work, musicians play. The second foray into music imploded after a few months. With a wife, one and a half kids, no money, no income, living in with the parents-in-law, the world seemed replete with better options. Surrendering any illusion of control, I applied to every job vacancy I could find while the free-lance music gradually kicked in. I got a part-time job teaching Philosophy of Science at the University of Amsterdam for eighteen months. Initially that choice was tactical but I eventually realized that I belonged somewhere in academia. Music became a tactic and academia the goal. During that time I took courses in the math department. It was there I learned probability from Guus Balkema and Johannes Runnenberg. My appointment was extended. I studied foundations of probability, especially L.J. Savage's *Foundations of Statistics* [22]. In 1979, I landed a permanent full time position at the Delft University of Technology, Department of Philosophy and General Science. The philosophy section consisted of three people. I was attracted to Risk Analysis where foundational questions lay close to the surface: What is science? What is uncertainty? What is probability? These questions suddenly gained enormous practical importance and I scarcely gave infinite cardinals a second thought. In 1987, I switched to the Department of Mathematics and in 1993 I became professor of Applications in Decision Theory in the group of Probability, Statistics, and Operations Research. With compulsory retirement pending, in 2005 I took a position at Resources for the Future in Washington DC.

Which main problems inspired your research path?

My research interests were largely shaped by the problems we encountered in the risk/uncertainty analyses for the nuclear power plants in the 1990's:

(i) Can expert judgment be rendered scientific?
(ii) How can we represent dependence in high dimensional distributions?
(iii) How can we quantify models with expert judgment?

The response to (i) was to treat experts quantifying their subjective uncertainty as statistical hypotheses, with the added twist that not only statistical accuracy but also informativeness are important. To measure these we would ask experts about variables from their field to which true values were or became known within the time frame of the study. The trick is to convert these measures into a strictly proper scoring rule for making weighted combinations of expert judgments, and validating performance[1]. Vines came out of (ii). Dealing with several hundred input variables with univariate distributions and selected rank correlations from expert judgment, what to do, write a 300 dimensional multivariate density? The idea is that you can define a distribution by specifying a sampling routine. The dependence information you capture should be algebraically independent so that experts' input can always be combined consistently, and partial specification should be easily extendible with maximum entropy to a complete specification[2]. Probabilistic inversion came from

1 see https://en.wikipedia.org/wiki/Structured_expert_judgment:_the_classical_model
2 see https://en.wikipedia.org/wiki/Vine_copula

(iii). Experts could not quantify uncertainty on abstract modeling parameters, so complex models had to be "inverted" at distributions on observables supplied by experts.

Did a specific scholar influence your choice of starting an academic career?

Career choice: Socrates. The most profound influence was Jan Hilgevoord, a Professor of Theoretical Physics at the University of Amsterdam with deep interest in foundations, especially foundations of quantum mechanics. We started a study group on the foundations of quantum mechanics which went on for many years. My most significant result from that period (together with Mike Keane and Bill Moran) was an elementary proof of Gleason's theorem establishing the non-embedability of quantum mechanics in a classical hidden variable theory; see [6]. Mike Keane (Professor of Probability at TU Delft) and Joop Doorman (Professor of Philosophy at TU Delft) were also invaluable mentors.

You seem to like being provocative, both in talks and written material. How does the scientific community react to this style – any anecdotes?

In philosophy you learn to analyze arguments and challenge presuppositions. Philosophical dialogue is thesis $\Rightarrow$ antithesis $\Rightarrow$ synthesis = new thesis[3]; *criticism is the lifeblood of inquiry*. However, parochialism sometimes derails dialogue: Socrates was convicted of impiety and corrupting the youth and had to drink the Hemlock. The remorseful Athenians later banished his accusers and honored Socrates with a statue in the Hall of Processions. Anecdote: I submitted our first paper on expert judgment to a psychology journal, as psychologists saw this is "their problem." They were interested in covariates of what was called "calibration" (statistical accuracy, coverage). Their measures were maladroit, as they didn't know the sampling distribution of their statistics and couldn't do proper hypothesis testing. To overcome this, they would use very large numbers of subjects (students), but ask them the same few questions. Example: Suppose you want to know whether men are statistically more accurate than women. You ask (say) 400 subjects to assign each of (say) 10 uncertain events, or their denials, to one of the "probability bins" 50%, 60%,…,90%. Sum the weighted square difference between target (50%–90%) and the proportion of events in each bin that occurred, and compare these scores for men and women. By ignoring replicates (same event assigned to same bin multiple times) the significance of the observed difference is unknown. Following the Socratic script, I thought I should point out what's wrong with the existing practice in order to motivate something new. The referee report said that I should not criticize others, but should "try catching flies with honey instead of vinegar." I wrote back that flies eat honey but they also eat shit and withdrew the paper, which was later published in *Automatica*; see [7]. There are no flies in the Hall of Processions.

A provocative talk in this sense is one of your most recent one on "epicycles of regression." Could you express your point about this issue here?

Regression is an enormous industry with much inertia. To compute regression functions, adepts have large toolkits for dealing with issues like including/excluding covariates, interactions, higher order terms, multi collinearity (as it is called, meaning "dependence among independent covariates"), model fit, transformations, heteroscedasticity, bias, convergence, efficiency, etc. These I call the *epicycles of regression*. Suppose we actually knew the multivariate density or mass function – in that case we wouldn't be plying the epicycles, we would just compute regression functions directly. Now suppose we have a rich set of multivariate densities with arbitrary margins. Why not take a best fitting member of this set, check the quality of fit, and just compute regression functions? And while you're at it, generate proxy data from this density and see how the epicycles perform? Why isn't this blindingly obvious? For continuous variables, regular vines give us such a set of densities with arbitrary continuous margins. This is a veritable orchard of low hanging fruit, but beware of parochialism: *You may have to drink the Hemlock before your statue appears in the Hall of Processions*. A discussion paper [5] is on the RFF website. Harry Joe and I are currently converting the paper to become more suitable for a journal.

In retrospect, what were the most rewarding moments of your academic career and were there also moments when you considered leaving academia?

3 This formulation seems to have emerged with the German transcendental philosopher Johan Fichte, but I trace the idea back to Socrates.

Figure 1: Left: The cover of the CD *Tortured Genius* by the Hein Van der Gaag Trio (Salad Dressing label 1993, Roger Cooke is in the middle); Right: Maria and Roger Cooke at the Oration, Delft 1995.

When it looked like Philosophy at Delft was *cul de sac*, I started playing professionally again. Then I got a position at the Department of Mathematics and dialed back the music, eventually stopping when I returned to the US in 2005. From 1990 to 2005 I played every Monday night in Amsterdam's jazz café *Alto* with Hein van der Gaag; see Figure 1, left. Rewarding moments – well I suppose becoming professor at Delft opened many possibilities, including working with Tim Bedford (see Figure 2, right). I worked very hard on my *oration* (see Figure 1, right), a formal public speech accepting a proffered professorship in which the aspirant professor addresses humanity on topics of mutual interest. I think it's the best thing I have ever written, though it drew a formal written protest to the math department for impiety from a pious colleague. Think of it as an ode to Socrates. It will be published in a Festschrift book being prepared by Simon French, Tina Singuran, Anca Hanea, and Tim Bedford and is available on my website at http://rogermcooke.net/rogermcooke_files/number%20of%20things.pdf.

You have a double citizenship: US/Netherlands. You are American by birth. Why did you come to the Netherlands? What are the pros and cons of living and doing research in the US and in Europe?

In the US it's all about money. While I was on sabbatical at Berkeley it struck me that colleagues mostly talked about money, and rarely about mathematics. In Holland we discussed mathematics a lot, money never. Playing music, I learned that if you can survive doing what you love, you're on the top of the foodchain, you've already won. In Holland one could easily cover the material necessities and devote oneself to important things. I hate money, it turns people into assholes. Holland unlike the US, is a great place to raise children because wealth is less polarized.

Many university programs contain courses on Statistics. Should these courses always be taught by professors of Statistics? How "dangerous" is it when they are provided by professors without a thorough academic background in Statistics?

In Delft many mathematics professors had their degrees in other fields, mostly engineering. A growing field always attracts people from neighbouring fields. An isolated field is a dying field, though it can still publish zombie journals. Statistics has lots of low end applications, but could do more at the high ends, in my opinion. Climate Change, Finance, Biology to name a few exciting areas outside the traditional comfort zones with i.i.d. errors. Recruiting professors with backgrounds in these fields could invigorate Statistics.

In a 2016 talk on climate change you stated that "At our current pace, it seems unlikely that we will understand climate change even after another 35 years" and also that "It's time to invest in an advanced climate observing system." Do you believe on scientific grounds that there is a climate trend (maybe better to say here "greenhouse effect") caused by human activities? What are your predictions for the future?

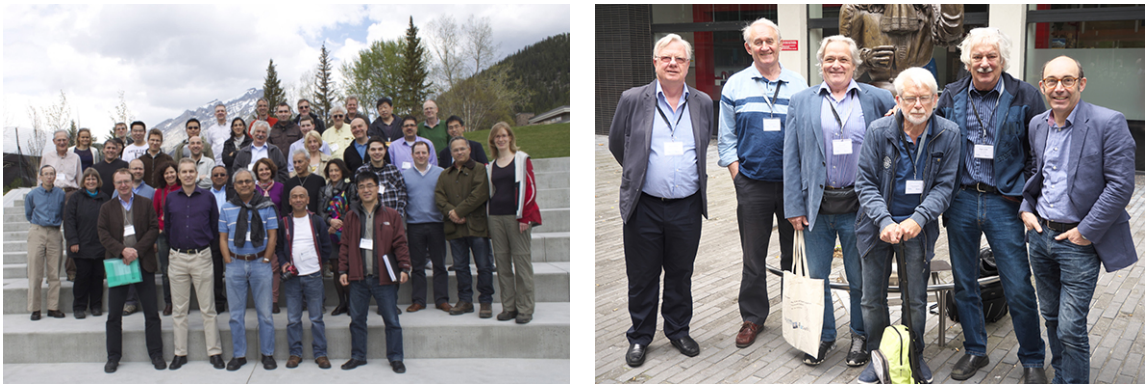

Figure 2: Left: Participants to the Conference *Non-Gaussian Multivariate Statistical Models and their Applications* held at Banff, Alberta on 19–24 May, 2013. Roger Cooke is standing directly in the center. Right: Roger Cooke with (from left to right) Simon French, Willy Aspinall, Jim Smith, Louis Goossens, and Tim Bedford at the Festschrift for Roger Cooke, Delft, July 3-5, 2017.

The syntax "do you believe" is not optimal. It's all about uncertainty. I think the scientific evidence from multiple lines (paleo, observational, modeling) overwhelmingly supports the claim that GHG emissions are raising global temperatures and effecting other changes. There is uncertainty regarding the pace of this change, in particular whether slow positive feedbacks will become operative in the next 200 years. We cannot explain the appearance and disappearance of 100 ppm CO_2 in the atmosphere between glacial and interglacial periods. Nor can we find some 10%–20% of the carbon we emit annually; we don't know where it goes and we can't predict what these "missing sinks" will do as temperatures rise. The public dialogue is hampered by the inability to reason with, communicate about, and decide under uncertainty. If we fail to cut emissions drastically and soon, and if some unpleasant positive feedbacks emerge with arctic carbon, weakening the ocean uptake, reducing albedo from clouds, etc., then large portions of the Earth could be unfit for permanent human habitation in 300 years. Even if we meet the Paris targets, there is a disturbingly high probability that regions with rain based agriculture in Iran, Afganistan, India, and elsewhere, will be unable to feed their populations in the next century. Climate becomes an additional stressor to already stressed systems. Trump is in the White House.

In many papers, one first proposes a theoretical model and next one validates the model with empirical results. A possible criticism of this practice is that some scholars may choose a theoretical model in such a way that it performs well for a given set of data, or that data might be selected to ensure validation of the model. Should we only read papers that do either theory or empirics?

There are good papers in both categories. Model validation is a great topic where statisticians leaving their comfort zone could make contributions. By the way, does the fact that the ubiquitous Cox proportional hazard model lacks a proper residual indicate a disregard for validation? Why don't we clean that stable first?

Is a non-linear research path still possible today, or does academia nowadays only accept "streamlined" candidates?

I think my path would be impossible today because jazz has crashed, but pathfinders always find new paths, by definition, if not by choice. Academics should jealously guard their freedom to accommodate outliers, and not abdicate to index-wielding bureaucrats.

2 Research

Would you explain your current position and work at the nonprofit organization *Resources for the Future* (RFF)?

RFF is a resource economics research institute. The mission is to correct market failures and allow the market forces to work for rather than against the environment. For example, since no one makes money selling air, air has no market value. The market fails to reflect the value of air in its market price. My appointment was funded by an endowment from Chauncey Starr, who created a chair in Risk Analysis with the stipulation that it should be occupied by a non-economist. Most of my work is currently under contract with NASA, calculating the value of satellite systems designed to measure climate processes. Most of our remote sensing information comes from weather satellites. Weather forecasting is a two week initial value problem. These systems are not designed to generate long accurate time series. The new proposed CLARREO Pathfinder system inter-calibrates existing systems and will allow us to measure climate sensitivity with requisite confidence many years before we would obtain comparable confidence with the existing systems. My role is to compute the real option value of the new system using the social cost of carbon methodology; see [4]. This work is ongoing. I'm now involved doing expert judgment uncertainty quantification on ice sheets and on the carbon cycle.

At the time of writing you have more than 10,000 citations in Google Scholar, which is impressive for a math professor, and your h-index computed on Web-of-Science is 22. What value do you attach to these figures and what value should a researcher assign to them?

Newton's *Mathematical Principles of Natural Philosophy* has approximately 4,000 citations on Google Scholar. D.R. Cox's *Regression Models and Life-tables* [14] has around 46,000 and Lofti Zadeh's *Fuzzy Sets* [25] counts more than 70,000. Much of the audience for our jazz gigs were pop musicians, many of whom earned much more money than we did, and nobody wanted to trade places. Lofti Zadeh and I would not swap places. With around 2,000, my book *Experts in Uncertainty* [8] sits happily below the Newton zone; if it got 70,000, I would feel very out-of-place.

Is it easier to publish your research papers in good journals once you are "well-established?"

I am not sure that is true. I would say it's easier to publish in a specific journal after you have written several good referee reports for them and aligned yourself with the Journal's agenda. Whenever you change fields or shift interest you start over at the bottom – with the other philosophers.

In many disciplines, academic research is using concepts/results from statistics that are rooted in a Gaussian dependence assumption (e.g., linear regression, hypothesis testing, principal component analysis) and it seems that this assumption is not always validated. Do you have any striking example(s) of "famous" papers that can be criticized on this ground?

The Munich group finds many examples of tail dependence in financial time series. We fit a vine density and compute conditional means and variances. I have yet to see a real dataset in which conditional variance is constant. The RFF discussion paper [5] on IQ and Breast Feeding has a graph plotting the conditional standard deviation of IQ as a function of a six-dimensional vector X of covariates against the conditional mean; see Figure 3. The conditional standard deviation strongly covaries with the conditional mean. These are computed with the approximating vine density using a Gaussian copula which in this case fits quite well. We are currently grappling with expert elicitation for tail dependence. An application to ice sheet dynamics was presented at the workshop on *Non-Gaussian Multivariate Statistical Models and their Applications* in Banff, 2013; see Figure 2, left. A video of that talk is available at http://www.birs.ca/events/2013/5-day-workshops/13w5146/videos/watch/201305221037-Cooke.html.

What is Risk Uncertainty explained to an undergraduate student?

Any student can understand the mathematical definition as

$$risk \;=\; chance \times (undesirable)\; consequence.$$

The problem is applying valid probabilistic reasoning in daily life. Even the Intergovernmental Panel on Climate Change (IPCC) fills some 5,000 pages with "highly confident in this, highly confident in that, highly confident in the other" without ever asking how confident they should be in *this, that, and the other*. The fact that most mathematical readers have to read that sentence twice illustrates the *Confidence Trap*: thinking that the conjunction of "high confidences" is the same as high confidence in the conjunction. The natural language (mimicked by fuzzy sets) herds humanity into confidence traps in droves. Inflating confidence invites an opponent to attack your confidence without having to defend an alternative position (the antithesis).

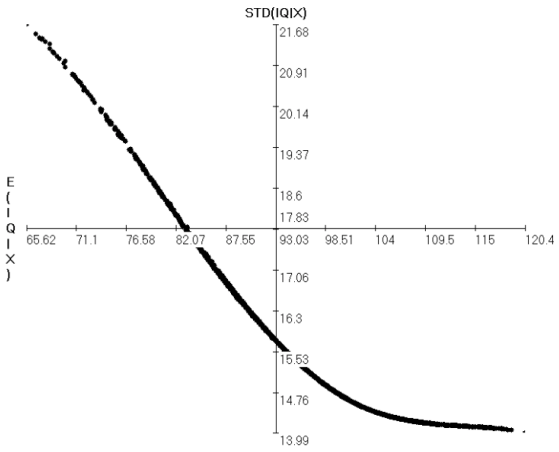

Figure 3: Scatter plot of conditional mean and conditional standard deviation of IQ as function of covariate values in NLSY data as described in [5].

In the climate debate, IPCC shoulders the entire proof burden. The deniers bear none and win by discrediting any one of the confidence claims … a catastrophic lack of philosophical training. Instead of inflating their confidence in climate futures, the IPCC should be challenging deniers to defend their own claim that doubling CO_2 concentrations will have little or no effect on climate.

You have been arguing for many years that one should invest more in a quantitative approach of making decisions under uncertainty. Are governments listening to you?

I have been trumped, Trump wants to cancel the CLARREO Pathfinder project.

Any anecdote/evidence where you felt that your work/advice made a change?

NASA has been happy with our results on the real option value of Earth observing systems and would like to see more of this sort of work, current anti-science politics notwithstanding. Most of the expert judgment stuff has seen direct application in policy preparation of one sort or another (see for example [24] and also a recent blog http://www.rff.org/blog/2017/respectability-looming-expert-judgment). I may be flattering myself, but I think my presence as a lead author on the IPCC's recent Fifth Assessment Report chapter on Risk and Uncertainty helped prevent the IPCC from becoming further ensnared in confidence traps (see [10]).

What should be the role of the "expert" in the model building procedures? How can we avoid arbitrariness and bias in an "expert's system?"

Hans Reichenbach distinguished the context of discovery and the context of justification. If models can be validated, it matters not how they were discovered; a narrative about pedigree and discovery cannot replace justification. Large models are not typically built by the top domain scientists. Validating these large models is so far from trivial that it receives far too little attention. If models predicting the far future cannot be validated with data, they should be constrained with expert judgment of top independent scientists using probabilistic inversion. The paper [21] in *Nature Climate Change* with Michael Oppenheimer and Chris Little addresses this.

How can a Bayesian approach help in model processes?

Strictly speaking, subjective probability is an interpretation of probability as partial belief of a rational individual, where "rational" is defined as satisfying Savage's preference axioms. Bayesianism is a theory of inference. My position is that subjectivism can be very useful in science in contexts where the relative frequency interpretation does not apply. The challenge is to render the use of expert subjective probabilities scientific. Bayesian updating does not constitute validation, unless the prior and likelihood assumptions have been validated, or unless the model can be validated out-of-sample. Whether a model's updated predictions agree with my personal beliefs is of little scientific value.

Figure 4: Roger Cooke at the conference *Dependence Modeling in Finance, Insurance and Environmental Science* held in Munich (Germany) on May 17–19, 2016. Slightly out of focus and sitting to his left, one can spot Harry Joe and Dorota Kurowicka.

3 Vine copulas

At a conference in Munich (see Figure 4) in 2016, Harry Joe and you "accused" each other of having invented vine copulas, both of you pointing out the merit of the respective other researcher. Could you please elaborate on the origin of vine copulas as you see it?

Vines Arise in the *Vine Copula Handbook* tells the stories; see [11]. Harry made the first vine, a D-Vine, in 1994 [16]. His motivation was to extend the bivariate extreme-value copula to higher dimensions and find finite-dimensional parametric subfamilies that would cover the whole family of min-stable multivariate survival functions. Independently, I published the formal definition and proved elementary properties, including the mutual information decomposition, in 1997, and gave the structure the name *vine*. The motivation as sketched above arose from applications. A referee at *Annals of Statistics* pointed us to Harry's work. Sampling theory was developed at the same time but didn't get published until the book [18].

How did the famous *vine paper* [3] with Tim Bedford originate?

When Tim joined my group, he got interested in vines and proved the important result that for any regular vine, any assignment of partial correlations from $(-1, 1)$ to the edges determines a unique correlation matrix, and every correlation matrix arises in this way. Correlation matrices are sparse in the set of symmetric matrices on $(-1, 1)^{N\times N}$ with 1's on the diagonal. Regular vines give an algebraically independent parametrization of the second order structure in terms of intuitive partials. Tim wanted to generalize this to "Cantor trees." I reminded him of Erdős' remark *I'd rather be generalized than generalize*. In any event, Tim found it more convenient to reverse the indices, so that the top tree with one node got index 1 and the bottom tree with $N-1$ edges got index $N-1$. Reversing all those indices was a hellish job. Then came the referee report which said we should reverse the indices back. I laughed uncontrollably. I was later very grateful that Tim insisted on submitting to *Annals of Statistics*, despite the five years it took to appear.

Could you try to explain the landslide of papers on vine copulas and did you anticipate this, say ten years ago?

That landslide was precipitated by Kjersti Aas et al. in the paper [1]. They developed algorithms for maximum likelihood estimation and simulation which opened the door to applications. I think the additive decomposition of the mutual information (MI) in terms of mutual informations on the edges of a regular vine in 1997 first persuaded me that regular vines might be an interesting structure. For a normal density, $\exp(-2MI) = D$, where D is the determinant of the correlation matrix. The special case of factorizing the determinant of the correlation matrix as $\Pi(1-\rho^2_{ij|D_{ij}})$, where $\rho_{ij|D_{ij}}$ is the partial correlation on edge $ij|D_{ij}$, of a regular vine, was used by Harry Joe in 2006 [17] and in 2009 [20] to derive the Lebesgue measure over the set of correlation

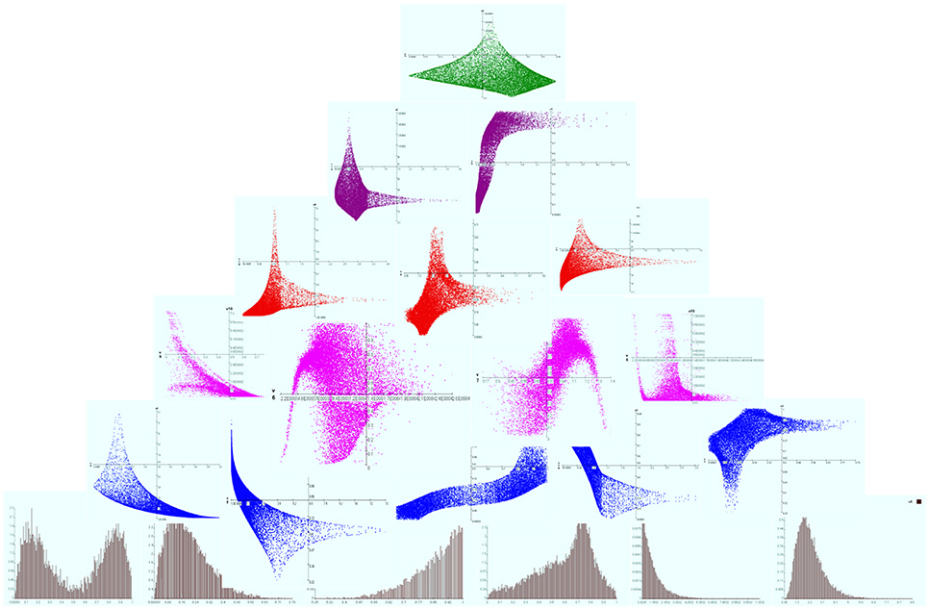

Figure 5: The picture on the cover of the *Vine Copula Handbook* [19].

matrices. Assigning appropriate beta distributions on $(-1, 1)$ to the partials on a regular vine, a method was found to sample random correlation matrices from a uniform density, such that each correlation matrix is "equally likely."

What are current challenges in research on vines?

The 2015 paper [12] with Dorota Kurowicka and Kevin Wilson gives some ideas: searching the space of vines efficiently, conditionalizing, and merging. There are very many regular vines on N variables, and with the simplifying assumption of constant conditional copula, they are not equivalent. Efficiently searching this space for a best regular vine with constant conditional copulae requires a low-proximity search algorithm, and that requires a good proximity measure. We took some halting steps in that direction, but the problem is far from solved. Low parameter relaxations of the simplifying assumption and discrete variables are interesting challenges. I'm interested in using the tools we now have to harvest the low hanging fruit, of which dispelling the epicycles of regression is perhaps the greatest.

What is the danger of being "addicted to vine?"

There is no danger until viniculturists become parochial and oppose further improvements (Cantor trees?). The ability to generate a rich class of handy multivariate densities casts problems like regression, density estimation, learning, fitting, smoothing, amongst others, in a new light. I think the anti-parochial phase will last a while.

If you had to choose a single image to communicate the relevance of vines in probability and statistics, which one would you choose?

The picture on the cover of the *Vine Copula Handbook* [19] was originally on my T-shirt. At a bowling alley an engineer from ESTEC approached me and asked what it was. We chatted for quite a while about vines. I guess that would be my choice; see Figure 5.

4 Concluding words

Chris Hinze, Hein van der Gaag, and jazz. What is their connection to Roger Cooke?

I'll add this to what was said above: the only way to learn jazz is by playing with people better than you. Those who have worked with me, Mike, Tim, Dorota, Anca, Tina, and Harry, will testify that I'm a fairly

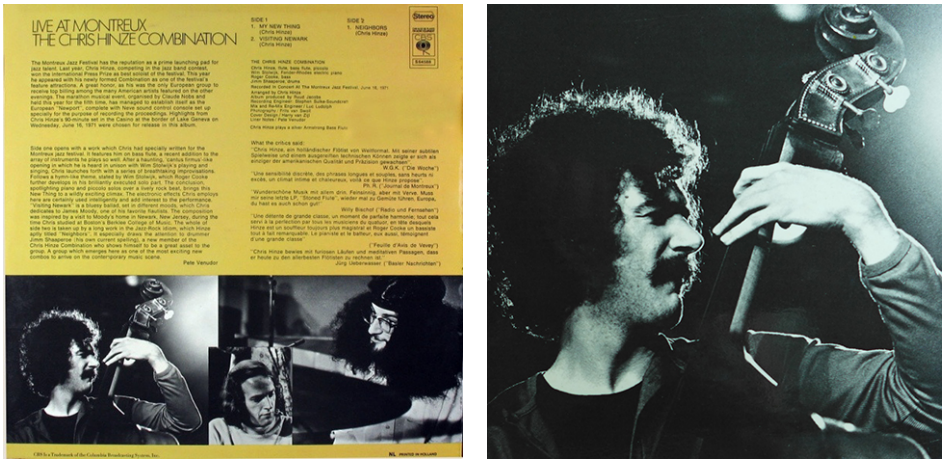

Figure 6: Roger Cooke (right) appears at the left bottom of the back cover (left) of the LP *The Chris Hinze Combination – Live At Montreux* (CBS label, 1971).

mediocre mathematician. I get by using things I learned from philosophy and music, and a little help from my friends.

You have quite some experience performing live music. Does this experience influence the way you "perform in class?"

Absolutely, I practice.

What did you do on Wednesday, June 16, 1971 at the Casino at lake Geneva?

I played at the Montreux Jazz Festival with Chris Hinze, see Figure 6. Montreux was a nice gig and we got a good reception. Another concert is more storied, namely playing with a solo jazz trio in a special performance of the Chicago Symphony Orchestra (June 12, 1970). It was a veritable Hate Fest. The piece was written by pianist William Thomas McKinley at the University of Chicago. At the first rehearsal the concert master/first violist asked me to move a bit to the side because otherwise he couldn't see the conductor Erwin Hoffman, which wasn't a problem because he never looked at the conductor anyway, but the conductor needed to see HIM. It was open warfare through two rehearsals in which the conductor mocked us and mocked the composition. The musicians in the orchestra were suspicious of us, hated the composer, hated the conductor. After the rehearsal of my bass solo the musicians tapped their music stands – a form of approval. The conductor retaliated by doing a facetious little flamenco dance on his podium saying "Oh I see, it's like Segovia." We never got through the score in rehearsal. I was 24, unseasoned, and scared out of my wits. The concert went perfectly. The orchestra was incredible, they didn't need any rehearsals, they were just seizing the opportunity to vent grievances.

What books or articles were most important for you?

Without hesitation, Feller's *An Introduction to Probability Theory and Its Applications*, vol. I and II [15], and Savage's *The Foundations of Statistics* [22].

How do you choose a journal to publish your new article?

If the article fits in the agenda of a journal and I know the editor, I would choose that one. If, as often happens with interdisciplinary work, it doesn't fit well with any existing journal, I would go for a new journal seeking to enlarge its footprint. "Top" journals have editors and referees who are deeply invested in their problems. They get very many submissions and will thus select based heavily on proximity. Disruptive new ideas will have a tough time. Some journals now require authors to cite publications in their journal. Driving research by publication in high impact journals is a bit like driving your car by looking in the rear view mirror. New journals serve an important role in the ecology of science. Of course bureaucrats are always looking for ways to "steer" science without understanding it.

You are the most known for vines and expert judgment. Are there any subjects you worked on in your career that did not take off? Do you have any idea why this did not happen?

I worked on competing risk and dependent censoring, which is important for designing reliability data bases. Preventive maintenance is dependently censoring the failure process. Current practice treats this censoring as independent and can cause large errors. This work was published [9] and I shopped it around at conferences, but the uptake was limited. I also worked on the Cox proportional hazard model, proposing a new validation approach. The idea is this: if all covariate coefficients are zero then the baseline hazard function equals the population cumulative hazard function. Covariate loading causes these two to differ, but the difference will be small if most loading is on missing covariates. One could test and hopefully reject the hypothesis that these two hazard functions are the same. It was published in a good journal [13], but the interest was zero. Most practitioners want to do what is currently acceptable. Doing something new and better only causes them headaches in explaining to clients and referees.

What are your children doing? Are they mathematicians or philosophers? If they are not would you have liked them to be?

The oldest daughter danced with the Netherlands Dance Theatre for many years and now teaches ballet at the Royal Conservatory in The Hague. The youngest is principal bassoonist at the Hague Philharmonic Orchestra. They both went for the artistic side. Of course we wanted them to do whatever they wanted to do.

What are your final suggestions to an undergraduate student willing to start an academic career?

Don't get discouraged by the parochialism in science. Friends and personal contacts are more important than indices. Don't abandon good ideas just because others don't like them, and don't hold onto bad ideas just because they're yours.

How did you like doing this interview?

I really enjoyed the other interviews and seeing the diversity in our field. It's a sign of vitality. Too many people think that humanity consists of more or less failed attempts to be exactly like them. Oscar Wilde reputedly said "be yourself, everyone else is already taken."

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