

Interview Article

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When copulas and smoothing met: An interview with Irène Gijbels

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Irène Gijbels is a Professor of Statistics at the University of Leuven (KU Leuven), Belgium. She was awarded a PhD in mathematical sciences (with concentration in statistics) by the Belgian Central Examination Commission in 1990. Thanks to a Fulbright–Hays scholarship, she was then a Visiting Professor at the University of North Carolina at Chapel Hill in 1990–91. Upon returning to Belgium, she was a Senior Research Assistant at the National Science Foundation before taking up a position at the Université catholique de Louvain, in Louvain-la-Neuve. Affiliated to KU Leuven since 2004, she is the author or coauthor of more than 150 research papers, and wrote with Jianqing Fan the influential book *Local Polynomial Fitting and Its Applications*, published by Chapman and Hall in 1996. She also served as the Editor-in-Chief of the *Journal of Nonparametric Statistics* from 2013 to 2015. Irène Gijbels is an elected member of the International Statistical Institute and a fellow of the American Statistical Association since 2002. She became a fellow of the Institute of Mathematical Statistics in 2006 and was inducted into the Royal Flemish Academy for Science and the Arts (Koninklijke Vlaamse Academie van België voor Wetenschappen en Kunsten, Klasse Natuurwetenschappen) on December 12, 2015.

Since the early 1990s, Irène Gijbels has gained an international reputation for her deep and extensive contributions to the theory and applications of semi- and nonparametric statistical methods. She had briefly explored the use of smoothing techniques for copulas at the very beginning of her career. After a hiatus of nearly 20 years, she revisited copula modeling and quickly became one of the most prolific and influential researchers in the field. She has shown, among others, that smoothing is a natural paradigm on which to rely for conditional inference in this context, much like rank-based methods are in an unconditional setting. The following conversation, held virtually during the COVID-19 pandemic, gives an overview of her scientific journey. In the following, our questions to Irène are typeset in bold-face.

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1 Irène's path to academia

Thanks for granting us this virtual interview, Irène. First off, everyone is curious to hear about your personal background and your academic path. Can you fill us in, please?

I was born and raised in the village of Stokrooie, later incorporated to Hasselt, the largest city and capital of the province of Limburg, in the Flemish Region of Belgium. My parents had a very strong impact on my performance in life, and indirectly on my academic life. My father was a miner and my mother a housewife. Both came from large families of farmers and had to quit school at an early age to help on the farm. My parents were extremely hard working, and in particular my father could be called a self-educated man. He was very knowledgeable, and we had a lot of lively discussions at home, especially about politics but also around economics, sports, or societal matters. My parents did not intervene with my choices at school, but it was understood that I should do my utmost best in everything I undertook. “Giving up” was never, and still isn’t, an option. My father had a very serious accident in the mine when I was 11 years old. His intensive rehabilitation took more than a year. The company then offered him a clerical job but he turned it down. He went back underground to lead his team, but unfortunately had to stop working due to serious health problems a few years later. This kind of “stubbornness” is something I inherited from him. The values with which my parents raised me are simply at the root of everything I have achieved in life.

My background surely did not place me at the forefront regarding access to education. Indeed, education was considered a luxury; to secure sufficient means of subsistence was the priority. However, at crucial stages in my youth, I was lucky to encounter a few teachers who saw my potential. It started with the principal in primary school. He encouraged me (and convinced my parents) to take after-school French language lessons, and so I did from age 10 onwards. In secondary school I followed the “Science and Technology” program, and there the influence of my science teachers was inspiring and even crucial. I had very dedicated teachers in chemistry, physics, and mathematics. In particular, one of my math teachers, Mr. Johan Bortels, recognized my potential. Although I was “just” a child at a “technical school,” he encouraged my learning far beyond the required curriculum. I was solving all the math problems he could provide. He would discuss them with me and challenge me week after week.

At the end of high school, I was awarded a prize for “maintaining a positive attitude throughout the past six years.” This was highly unusual, in particular given the type of school. The prize as such was not that important to me, but the award came with two math books in English from the Schaum series. The idea definitely came from my math teacher, and I guess he never suspected what impact that prize had on me. I worked through these books during summer, just for fun. It comforted me in my idea to go to university. However, I could not get sufficient financial support from my parents. They simply could not afford it, and nobody in the extended family had ever had a university education. Nevertheless my parents respected my wish: I could go if I got a scholarship, and I could continue if I managed to renew the scholarship every year. That is how I became a first-year university student, and the rest is history.

How did you end up in statistics?

My major in the mathematics program was in astronomy which, traditionally in Belgium, was part of the math curriculum. As with many things in life, it is somehow by chance that I ended up in statistics. After graduation, I wanted to do doctoral research in astrophysics; unfortunately, there was no position available at that time. I thus started working in secondary schools, teaching mathematics, physics, and even informatics. While I enjoyed teaching *per se*, I disliked most of the things around it (board meetings, administration, etc.). After two years, I started working over the summer holidays as a volunteer in a biomedical research institute at the Limburgs Universitair Centrum (LUC) in Diepenbeek. This greatly rekindled my interest in research, and I applied for a funded doctoral position within the statistics group there. To my surprise, I received an offer so I enrolled in the PhD program (Figure 1).

Your supervisor at LUC was Noël Veraverbeke. What was your thesis topic?

In my doctoral dissertation, I studied asymptotic representations under random censoring. More specifically, I examined the behavior of the Kaplan–Meier estimator, which is an analog of the empirical distribution function for randomly right censored data. To get a sense of that work, readers could take a look at my first paper in *The Annals of Statistics*, written jointly with Noël [40].



Figure 1: Irène (first row right) as a PhD student at the Limburgs Universitair Centrum (LUC). Also on the picture are Paul Embrechts and David Cox (top row, second and third from the left), Robert Serfling (first row, left), Noël Veraverbeke and Louise Ryan (middle, first row), and Ronald Randles (first row, second from the right).

In a recent interview [21], Paul Embrechts, who was then a professor at LUC, described the atmosphere there in those days. How was it like from your student perspective?

In those days, the number of PhD students in statistics in Belgium was small (say around 20), whereas nowadays, it has probably grown tenfold. As the community was small, we knew each other quite well, forming a close-knit community of young researchers. It must be realized, too, that most PhD positions at that time were for 6 years, with a considerable portion of time devoted to teaching-related tasks. Moreover, doctoral education was also not organized via doctoral schools, as it is now. I, for one, barely took any statistics courses. My training in the field mainly came from attending weekly reading/seminar work groups, scientific discussions with my advisor and visitors, as well as by working through books, papers, and the like on my own. I used to spend at least one full day per semester in the library, browsing through issues of journals, seeing what was published and who was working on what.

After defending your thesis in 1990, you received a Fulbright–Hays scholarship for postdoctoral studies in the United States. How and why did you opt for Chapel Hill?

After completing my PhD, I had some time left on my contract as an assistant, and I wanted to use it to go abroad through the Fulbright–Hays exchange program between Belgium, Luxembourg, and the USA. I searched on the Internet (which was rudimentary at the time) for American statistics departments that seemed a good match for me. During my PhD studies, several international researchers had visited LUC. One of them was Steve Marron, and hence, I included the Statistics Department at Chapel Hill on my short list. The quick reply from the Chair at Chapel Hill, the late Stamatis Cambanis (1943–1995), won me over. I am still grateful to him for giving me – a complete stranger – the chance to be a Visiting Professor at the University of North Carolina. He even went as far as to pick me up late at night at the Raleigh-Durham airport and, with his wife, kindly helped me to get settled in my apartment (Figure 2).

Whom did you meet in North Carolina; who influenced your career?

First, I want to stress that it was the dynamic environment within the whole Statistics Department at the University of Chapel Hill that made a success of my visit. I only knew Steve Marron a little, and I had only corresponded with Stamatis Cambanis. Before my visit, I had looked at what the various department

members were working on, notably Richard Smith and Ross Leadbetter. However, in hindsight, the greatest influence on my career came from people I had not yet met or heard about: a young researcher visiting the department, Tien-Chung Hu (now at National Tsing Hua University, Taiwan), and two assistant professors: Young Truong (Biostatistics) and Jianqing Fan (now at Princeton). We were all equally eager to contribute to science and had a wonderful time filled with scientific discussions. Very soon Jianqing and I started collaborating; we have written many papers since then. We were both filled with energy and would not rest before we had a full understanding of the problem at hand. With Tien-Chung Hu and Li-Shan Huang (who was then Fan's PhD student; now also at National Tsing Hua University), we worked on bandwidth selection for local polynomial regression [16].

Were you tempted to stay in the US?

Oh yes I was, and I hesitated for quite a while. At the time, my husband often travelled to South America, and we both enjoyed exploring new horizons. We very seriously considered staying in the US, and I even got some concrete job offers. But in the end, we felt this would likely only be temporary, perhaps for a decade or so, and that we would eventually prefer to return to Europe. So we went back, but then we struggled for several years with our decision to forego all the opportunities that were offered to us. Although we never regretted our choice, we realize that our lives could have been quite different.

Can you identify role models that motivated you to go into academia?

Role models played a major part in my development. As I mentioned, I could not rely on my roots for inspiration, and hence, my belief in what is possible in academia had to come from other sources. In particular, the percentage of female students in math was very low when I started university. For this reason, female role models were important to me. The first two were Marie Hušková and Jana Jurečková from Charles University, in Prague. I was a PhD student when in the second half of the eighties they were occasionally visiting LUC in Diepenbeek. These visits were by no means easy to organize; the Cold War and the Iron Curtain made it difficult for them to travel to a Western European country. But despite these difficulties, and although they had to leave their children behind for a while, they managed to cope with all this and had very fruitful and enjoyable scientific visits abroad. As having a family, and children, was also very important to me, Marie and Jana somehow showed me that a woman can have a successful academic career while being a mother. Of course I had many other (female and male) role models later on, and in my interaction with them, I was reassured that academic life was my “natural habitat” and that I could be successful in it.



Figure 2: Irène at her desk at the University of North Carolina at Chapel Hill in 1991.

2 Focus on research

Your research output is abundant and particularly diverse. What is your secret?

As a scientist, I am always eager to explore new horizons and get different points of view and insights from other fields. A good example of this is my work with Alexandre Lambert and Peihua Qiu on change-point detection and edge-preserving surface and image denoising, in which a great variety of tools came into play [31,32]. Another example is my work with Nancy Heckman on testing for an increasing hazard function, which combined techniques from order statistics, survival analysis, and change-point detection [25]. Some of my work also has to do with extreme-value theory, such as my paper with Liang Peng on estimation of a support curve via order statistics [37], and my recent work with Abdelaati Daouia and Gilles Stupfler on extremiles [13,14].

Important recurring themes are semi- and nonparametric inference, as well as asymptotic theory. How did your interest in these areas develop over time?

My interest in these research themes stems from my doctoral research in survival analysis, but the vast majority of the work that I did since then has been in the area of semi- and nonparametric inference in regression models. My interest in nonparametric regression was highly influenced by my postdoctoral studies in Chapel Hill, but it evolved a lot over time. I became familiar and worked with most of the existing nonparametric techniques, including kernels, local polynomials, splines, and wavelets. Over the years, I explored various aspects of inference, such as constrained and unconstrained estimation, testing, variable selection, and model selection. Of course, some of my papers have also dealt with purely parametric problems, such as my recent work with Anneleen Verhasselt and others on a family of two-piece asymmetric distributions for linear and circular data [3,29].

Applications are another hallmark of your work. Why do you view this as important?

That is because I am convinced that we need to pay attention to all aspects of a statistical problem: theory, computation, and implementation, but also the practical use of the methods. Although my strongest contributions were likely on the theoretical side, I value both theory and practice. For example, I consider theory and a well-designed simulation study as complementary. If a simulation study leads to surprising results, this might mean that some theoretical insights are missing; at the same time, theory provides a set of conditions under which a statistical method is expected to perform well.

Nowadays, software code is readily available on open-source platforms. In contrast, my first uses of a computer consisted of punching holes in cards, and submitting a deck of punched cards for a batch job at the computer center. I used Fortran 44 for the first numerical study included in one of my papers [30]. That said, not every article needs to contain theory, simulations, and real data examples. In my view, there is nothing wrong with a solid theoretical paper that provides a clear motivation for the problem at hand, yet does not include any simulations or data applications. All aspects should be addressed, but not necessarily by one and the same (group of) researcher(s), nor necessarily in a single paper.

One way to survey your research contributions is to proceed by main coauthors. How about starting with Jianqing Fan, with whom you wrote your well-known book?

My collaboration with Jianqing Fan started in the early nineties when we first met in Chapel Hill, where he was an Assistant Professor. Our most important joint contributions can be found in our book *Local Polynomial Fitting and Its Applications* [15]. While Jianqing established the foundations for local linear regression in earlier work, our work focused more generally on local polynomial fitting and applications in a variety of settings, including survival data, time series, and general likelihood settings, among others. My most cited paper, namely Carroll et al. [10], is on generalized partially linear single index models. Readers might not be aware of this, but beyond our work on local polynomial fitting, Jianqing and I co-authored papers on wavelet methods for unfolding sphere size distributions, and on minimax estimation of a bounded squared mean. We always had a lot of fun doing research together, driven by our curiosity. We share the same passion for research, are both quite energetic, and have working styles that are different but complementary.

Anestis Antoniadis is another energetic person with whom you collaborated early on.

We have been collaborating for over 25 years now (Figure 3). One part of my PhD thesis had to do with the use of penalized likelihood techniques in density estimation under censoring. In that context, I had read



Figure 3: Irène on a visit to Cape Town, South Africa, to work with Anestis Antoniadis (right). Also on the picture (left) is Idris Eckley from Lancaster University in England.

a 1990 paper by Antoniadis and Grégoire [5], but we only met for the first time in person at a conference in Namur, Belgium, in 1993. Anestis invited me for a longer visit to his home institution in Grenoble, France, the following year and our first joint paper, on model selection using wavelet decomposition, coauthored by Gérard Grégoire, appeared not long thereafter [4].

Since then, Anestis and I have worked on a variety of topics, using wavelet decompositions and splines. The topics covered by our papers include model selection, change-point detection problems, inverse problems, monotone regression, smoothing nonequispaced heavy noisy data, penalized likelihood regression for generalized linear models, quantile regression and variable selection using P-splines for longitudinal data, joint estimation of mean and dispersion, and so on. We had quite a few coauthors, including Jianqing Fan, the late Brenda MacGibbon (1944–2022) – whom you knew Christian – various French researchers and, in recent years, Italia De Feis and Umberto Amato, from Naples, Italy.

Anestis Antoniadis is a very energetic researcher indeed, and we share many viewpoints on academic research. His knowledge is extremely broad, and he has a very nice personality. Ever since we met in the early nineties, we and our families have kept in touch, and we became very good friends.

Speaking of energy, of course, nothing beats the legendary Peter Hall, with whom you also had an opportunity to work. Was this the result of your first sabbatical in Australia?

Although I was familiar with Peter Hall's work for a long time, one of the first occasions we met in person was at the Mathematical Sciences Research Institute (MSRI) at Berkeley, California, where I was visiting for half a year in 1992. Peter was also visiting the MSRI at that time, and we had many scientific discussions. On a fine Saturday, Jianqing Fan (who was also visiting the MSRI around that time) and I even managed to drag Peter away from his desk for a joint visit to San Francisco (Figures 4 and 5).

In February 1997, I organized an international workshop on *The Art of Nonparametric Statistics: Methodologies and Applications* in Louvain-la-Neuve, Belgium, and the university seized this opportunity to award an honorary degree to Peter (Figure 6). I was working on change-point detection problems at the time, and we discussed that topic, also with Aloïs Kneip, who was then based in Louvain-la-Neuve. This resulted in a joint paper on estimation of jump points in nonsmooth curves [23].



Figure 4: Irène with Jianqing Fan (left) and Peter Hall (center) in San Francisco in 1992.



Figure 5: In front of Berkeley's Mathematical Sciences Research Institute in 1992, with Jianqing Fan (left) and the late Miguel Angel Arcones (1962–2009) on the right.

Then in 1998–1999, Peter Hall invited me to spend my first sabbatical leave at the Australian National University in Canberra. My husband and our 13-month-old daughter came with me. Canberra is an ideal place for a family and Peter, who was extremely kind, took great care of us. I had been working on testing monotonicity with Adrian Bowman and Chris Jones [9] while they were visiting me in Louvain-la-Neuve. This led to discussions with Peter and to our first paper on monotone regression, coauthored by Chris Jones and Inge Koch, who was also in Canberra during that period [22]. With Li-Shan Huang and James Gifford, we then further explored nonparametric estimation of a hazard function under constraints [42]. My fourth and last joint paper with Peter was co-authored with Aloïs Kneip; it was a follow-up to [23], but the focus was on constructing confidence bands for unsmooth curves [24].

Any anecdote you could share with us about Peter Hall (1951–2016)?

Everyone who knew him enjoyed his great personality. Most remarkable was that you could discuss any scientific problem with him. He would think for a short while and then give his opinion, often coming up



Figure 6: On the occasion of awarding an Honorary Degree to Peter Hall (center) in 1997, with Aloïs Kneip (right).

with key ideas of how he would approach it. While I was visiting Canberra, he would drive on Saturday to Sydney (a 4-hour drive, each way) to visit a sick family member. I recall him coming back on a Saturday afternoon (I was working in the office) saying that he had sketched the proof of a theorem while driving... I also remember that while carefully checking an early draft of a joint paper, I became convinced that we could not assume independence at some point in a proof. With considerable hesitation, I raised the issue with Peter, who concurred but could immediately solve the problem with a new argument. It was a challenge to keep up with Peter's pace and constant high level of concentration. Most remarkable to me though was his extreme kindness, in particular towards young researchers, and his great modesty (despite his clear scientific superiority). That made him unique.

Where else did you go on sabbatical during your career?

Two of my sabbatical leaves were spent in Australia. One was in Canberra, where Peter Hall was my host; the other was at the University of Western Australia, in Perth, where I worked with Jiti Gao. Other sabbatical stays were at Texas A&M in College Station, in the US, and at the University of Stellenbosch, in South Africa. During my latest sabbatical leave, in 2014–2015, I also spent some time in Cape Town and in Toulouse, France. Apart from shorter stays in Europe, I was always accompanied by my husband and my children (when they were young). These sabbaticals were important, as each of them allowed me to explore very different topics and led to new research directions.

Over the past 15 years or so, Noël Veraverbeke also became a key collaborator of yours.

As mentioned earlier, Noël was my thesis advisor. Early in my career, we published a few papers together, mostly in survival analysis. After our 1991 joint *Annals* paper [40], we went our separate ways, though we remained friends and met regularly at workshops. One such occasion was in Hejnice, Czechia, in September 2007. The theme was robust and nonparametric statistical inference. With Noël approaching retirement, I told him in jest that it would be time for us to start working together again.

That workshop actually marked the beginning of our second round of collaboration. Noël was talking about distribution function estimation, and one of the issues that came up in the discussion around this was related to the estimation of conditional copulas. As you know, semi- and nonparametric inference for conditional copulas turned out to be one of our major contributions to dependence modeling. Among my joint papers with him is ref. [1], which Noël first presented at a workshop you organized in Montréal, Christian. This article, which appeared in the Special Issue of the *Journal of Multivariate Analysis* that ensued [18], addresses semiparametric estimation of a conditional copula. It uses a local likelihood approach and a local polynomial fitting technique. We proved, among other things, an asymptotic

normality result for the proposed estimators. The proof of this result, which is by no means trivial, relies on the theory of U-statistics. You could view it as a return to my roots, given that one of my first papers with Noël and Paul Janssen was on asymptotic representations of trimmed U-statistics [28]. It was fun to dive again into this theory after a few decades.

Your collaboration with Marek Omelka also started about then, no?

Marek and I started collaborating when he joined my group as a postdoctoral researcher in 2007–2008. He came from Prague. This was in the wake of the lively (and by moments heated) discussion forum about copulas, some of which resulted in the discussed paper by Mikosch (Figure 7) on “Copulas: Tales and facts” [45]. I had worked on copulas in the late eighties and was following this conversation from a distance. Somehow, I felt stimulated and inspired to contribute again to copula modeling. I caught up by reading a number of papers/books on copulas and dependence measures, including [8,19,20].

Marek Omelka’s visit offered an opportunity to start a new project on this theme. Given my expertise on smoothing techniques, it was natural to try and use these tools in dependence modeling. Most of my work with Marek was done in collaboration with Noël Veraverbeke, some of which I mentioned earlier. Marek and I also developed tests for tail monotonicity and multivariate measures of association – including inference for multivariate tail coefficients – but we worked on other topics too, such as homogeneity tests for multivariate dispersion and the notion of depth for functional data.

My first paper with Marek [46] was in line with a 1990 article of mine with Mielniczuk [33] on kernel estimation of copulas. In the process, we also examined the regularity conditions traditionally imposed on copulas, e.g., for the convergence of the empirical copula process, which we found to be too strong as they ruled out many of the standard copula families. We proposed weaker conditions on the second-order partial derivatives of copulas (in particular at the edges and corners of the unit hypercube), and pointed out that these are often met. This subject was later picked up and elaborated upon by others; see, e.g., ref. [49]. My association with Marek has been, and continues to be, very successful.



Figure 7: Irène and Thomas Mikosch, plenary speakers at the 19th European Young Statisticians Meeting in 2015 in Prague.

3 Research on dependence modeling

Very few people know that you published your first copula paper back in 1990!

Indeed, my first paper on copulas appeared in 1990, based on work done in the late eighties. I started working with Jan Mielniczuk when he visited our university in 1989. A large part of the work on our copula paper [33] was done while I was visiting the Polish Academy of Sciences and Warsaw University of Technology for a couple of weeks. As I had been working on smooth estimation of a distribution and quantile function using kernel techniques, and given Jan's interest in nonparametric techniques, looking into copula density estimation came naturally. We were inspired in part by an earlier paper of Jan's on kernel estimation of a so-called grade density [12], which is a ratio of two densities.

Estimation of a copula density c is related to estimation of a specific grade density, because for any random pair (X, Y) with joint density h , marginal distributions F_X and F_Y with corresponding densities f_X and f_Y , one has

$$h(x, y) = c\{F_X(x), F_Y(y)\}f_X(x)f_Y(y) \Leftrightarrow c\{F_X(x), F_Y(y)\} = \frac{h(x, y)}{f_X(x)f_Y(y)}.$$

Jan and I worked very intensively to finish this article, fearing that we might not get another occasion to meet. At the time, Poland was behind the Iron Curtain.

The fact that a copula density lives on the unit square is a difficulty to be taken into account to ensure consistency of a kernel method. In a univariate setting, Hominal and Deheuvels [44], and later Schuster [48], had discussed a mirror image modification to deal with the bounded support issue. Such a one-dimensional reflection method consists of augmenting the dataset by reflecting all observed points with respect to the two boundary points. Jan and I extended this method to the bivariate setting, reflecting each data point with respect to each edge and corner of the unit square.

Jan and I were both quite proud of our results. As we focused on asymptotics, however, our paper did not deal with the choice of bandwidth. In my first joint paper with Omelka in 2009, we came back to this reflection method in kernel estimation of the copula density [46]. We started by briefly reviewing methods for copula estimating, then focused on how to avoid restrictive conditions on the underlying copula, and on how to improve the kernel estimation by taking a variable bandwidth.

How did your interest in conditional copulas come about?

Consider a response Y and a covariate X and denote the conditional cumulative distribution function of Y given $X = x$ by F_x . Then note that, for all $y \in \mathbb{R}$,

$$F_x(y) = \Pr(Y \leq y \mid X = x) = E\{\mathbf{1}(Y \leq y) \mid X = x\}.$$

Given that a conditional copula is nothing else than a (special) conditional distribution function, and estimation of a conditional distribution can be seen as a regression function where the response variable is of an indicator type, my interest in estimating conditional copulas is probably not a surprise. One of the first papers dealing with smooth estimation of a conditional distribution was joint work by Hall et al. [43]. Of course estimation of a conditional copula is more involved because in realistic settings, the (conditional) marginals are unknown and hence must be estimated. Although estimation of a bivariate conditional copula can be reduced to estimation of a conditional bivariate distribution function, combined with estimation of conditional univariate distribution functions, the complete problem is not an easy one, because there is possibly smoothing to be done at the two levels; the article by Fermanian et al. [17] is relevant here. Among the main issues is the amount of smoothing needed in each layer, and how to make a choice in practice. My long experience with smoothing techniques came in handy.

My work on conditional copula estimation was instructive in many ways. For one thing, there are several ways in which one can estimate conditional marginals, as we described in ref. [50]. Second, the way in which the pseudo-observations are obtained has an impact on the bias of the estimate. Third, when it is known that the dependence structure is not influenced by the covariate, this information should be taken into account in the estimation. These findings were reported in papers [36,41,51].

Apart from these, it was also quite a challenge to look into more general settings in which the covariate is multivariate or even of a functional form; see ref. [35]. As you may know, one of my other research interests lies in statistical methods for functional data.

You also looked into testing for specific dependence structures or covariate effects in the conditional setting. How would you summarize your work along these lines?

My work on testing for specific dependence structures was done mostly with my PhD student Dominik Sznajder, who is now an actuarial consultant for Milliman in Brussels. We developed tests for positive quadrant dependence and tail monotonicity [34,38,39]. As for the tests designed for specific covariate effects, they make it possible to investigate, among others, the special case of the absence of covariate effects, which is often referred to as the simplifying assumption [2]. This modeling assumption got a lot of attention in the literature, in particular because it is often made implicitly in applications.

My contributions to these two broad types of tests, i.e., pertaining to structures and to covariate effects, are important. They each come with their own challenges. In particular, my work on the inference under constraints was challenging, because I did quite a lot of research on constrained estimation in the late nineties and in the first decade of the 21st century. This made me especially curious to find out how to do constrained estimation in the context of copulas. As usual with copulas, special problems had to be dealt with, such as measurability issues given that a copula is a distribution function leading to a measure, and hence, the constrained estimate should lead to a measure. For the second group of problems, the motivation and challenges were quite different; they required new tools for deriving tests, as well as estimation under a restricted setting, different than a qualitative constraint as when you test for a specific structure. We studied a few semi- and nonparametric approaches.

What brought you to study sums of random variables with copula-induced dependence?

I have always enjoyed the company of colleagues working in finance and insurance, both at Louvain-la-Neuve and Leuven. In Louvain-la-Neuve, actuaries had been using extreme-value copulas to analyze bivariate tail dependence [11] and in Leuven, Wim Schoutens is an expert in financial engineering who organized many talks in this field, which I attended. My research on sums of random variables, and subsequently portfolio selection, was done with one of my former PhD students, Klaus Herrmann, who is now affiliated with the Université de Sherbrooke, in Québec (Canada). A starting point for the results in our paper [26] were the algorithms developed by Arbenz et al. [6,7]. My papers with Klaus [26,27] are at the crossroad of several disciplines, which I like. Although I have no expertise in financial mathematics, it was fun to investigate optimal portfolio selection and how it is influenced by the impact of the dependence structure between the assets.

Your recent work on directed acyclic graphs and copulas is another example of breadth!

This was quite a challenge, believe me, because my knowledge of graphs was rather minimal when I started collaborating with Gerda Claeskens and our co-supervised PhD student Eugen Pircalabelu on this topic. The two of them were more familiar than I was with graph modeling, but the project benefited from joining our forces [47]. The key element of our approach is the use of C-vines for parent-to-child edges and D-vines for the parent-to-parent edges in graphs. This was inspired by the fact that for C-vines, each tree has a dominant node. Modern selection tools were then used to choose appropriate copulas in the various parts of the graphs.

Were dependence modeling conferences important for your work in this field?

I did not attend that many conferences over the years, for various reasons. I definitely prefer small- to moderate-size meetings. One event I really enjoyed was the 2015 Oberwolfach workshop on *Copulae: On the Crossroads of Mathematics and Economics*, organized by Xiaohong Chen, Wolfgang Härdle, Piotr Jaworski, and Johanna Nešlehová. The same goes for the various workshops and conferences I attended in Munich, Germany (organized by Claudia Czado and you, Matthias, among others) and Salzburg (Figure 8), where the main organizers were Fabrizio Durante and Wolfgang Trutschnig.

These are just a few examples of the many nice events organized by the dependence modeling community. Specialized events tailored to a specific area are particularly pleasant, I find, and they always lead to a wealth of scientific discussions. Generally speaking, the dependence and copulas sessions at the yearly CFE-CMStatistics conferences held mainly in London have also been quite instructive.



Figure 8: Group photograph for the Salzburg workshop on dependence models and copulas, Salzburg, Austria, September 2016. Irène is in the first row, between Ludger Rüschendorf (left) and Noël Veraverbeke (right).

4 Around collaboration and mentoring

You have graduated over 20 PhD students to date. What do you expect as an advisor?

With time I came to realize that I like guiding young researchers, although this is not an easy task. I take it very seriously, which is not so surprising given that I was raised with a strong sense of responsibility. An unwritten rule in my family was: “If you do something, you do it as well as you can.” Hence I set the bar very high for myself, and I guess also for those who choose to work with me or be around me... To be a successful PhD student, you need to work hard, and think and rethink issues until you gain the insight needed to understand a problem. As with many things in life, research has its ups and downs, so to meet with success, a PhD student or a researcher must be resilient. Personally, I will basically have no rest until I am nearly 100% satisfied with a piece of work. I expect the same of my students.

What differences do you see between current practice and your days as a PhD student?

As I already mentioned, there were relatively few students in my days. We had a considerable amount of teaching to do, but a lot of time for research, too. Nowadays, there are many more PhD students around. They have four years to fulfill the requirements of the doctoral school, and they are followed closely by a supervisory committee. They must also attend courses on topics such as scientific integrity and presentation skills, participate in doctoral seminars, etc. They have teaching assistantships and scholarships, as we did, but their responsibilities are a bit more limited.

Another important difference is that nowadays, research is done under more pressure. In my institution, you must have at least one paper accepted in an international peer-reviewed journal to be eligible to defend your thesis. When I was a PhD student, I was not thinking in terms of writing papers. Although I eventually wrote some, it came rather naturally. When I defended my thesis, I had five papers published, and in the end, eight papers resulted from research pursued during my doctoral studies. In the current competitive academic system, we are pushed to have more and more PhD students. When supervising these students, we must keep in mind that about 80% of young scientists awarded a PhD degree end up working outside academia.

I would also like to say that today, PhD students are much better trained than we were in terms of communication and writing skills. While this is commendable, one should always keep in mind that the scientific content and the quality of the research are, and should remain, the top priorities.

Do you have any specific advice for young women going into science?

In my days, gender mattered and, unfortunately, it still does. Fortunately, there is now greater awareness about this issue, but awareness alone is not enough. I am not a feminist nor in favor of positive discrimination as such, but I think people should get equal chances and treatment, independent of gender, race, etc. I think I was lucky to encounter some female role models such as Marie Hušková and Jana Jurečková early on. I am not sure I would have been confident enough to follow the same path as I did without having had at least some female, as well as male, role models.

Looking at all sorts of statistics about gender, both in academia in general and in statistical science in particular, I think we still have a long way to go. Every year, I see a lot of bright students in my classes, both male and female. But the proportion of females gets smaller and smaller as one moves up the academic ladder. I think that role models, mentors, and advisors can at least assuage some concerns, but as a community, as a society, we also need to embrace diversity rather than being afraid of it.

Are there aspects of the Belgian university system that you particularly like or dislike?

As I mentioned before, I hesitated quite a while before choosing to return from the US. What I like less in a country/system like Belgium, is that it is inherently small. The country has a rather complex (political) structure with different regions and languages (Flemish, French, German). Opportunities for moving from one place to another end up being rather limited.

In Belgium, a sizeable proportion of people still build their career where they obtained their PhD, and if not, they will typically at least stay within the same linguistic region. This is quite different from the US, say, where there are far more opportunities to move around, and negotiate conditions when you're made offers. Also, sources of funding for scientific collaborations between Belgian researchers across different regions were slim or lacking for a long time. Major improvements have gradually been made, but I would like to see these artificial barriers gradually eliminated in Belgian academia.

And more globally, what is your perception of the way the academic world is moving?

Over the course of my career, I have seen major transformations in the scientific community. In particular, science has become way more competitive in all its facets, and the pressure to get more and more papers in top-ranked journals is now enormous. As a result, I find that opportunities for creative work and exploring new directions are becoming too rare.

When I attend a talk, a seminar, or a conference, I always try to learn something from it, and I often cherish afterwards what I learned from it. I rarely leave a seminar room with a feeling of having wasted my time. In their research, more and more people tend to be almost exclusively focused on the specific and specialized problems at hand. I notice this in particular with younger generations of scientists, facing the pressure to publish. I regret that they more rarely strive for and experience the open mindedness of the general culture of science. It seems to be all a bit lost in the race.

Another major change I witnessed is that science is now much more in the public's eye than it used to be. This is to be applauded but seeking public or media attention should not become a goal on its own. I do not like the celebrity-driven culture that has gradually been brought into science. Solid science needs time to be creative; we need careful thinking, to take the opinion of peers into account, etc. Scientists should seize every opportunity to practice humility. Of course, the greatest eventually get in the limelight because of their findings but for my part, I prefer to remain in the shadow. As a child I was very shy, and in many ways, I still am. I am also not a person that likes (or seeks) attention.

We are very conscious of that and are thankful to you for granting us this interview!

5 Concluding comments

If you could start all over again, would you still choose to be a statistician?

I would be a scientist for sure. I don't think I have the ability to be anything else. The fact that I am a statistician, and not a pure mathematician, an astrophysicist, or a biologist, say, is just happenstance. As a 17-year-old girl, I hesitated for quite a while between various science programs: math, physics, biology, chemistry, etc. I ended up choosing math, and I am happy with my choice and the path it led me to, but I think other paths, as a scientist, would have been possible.

What would you say was the greatest help along your path?

It was crucial for me to have a husband who understands this strange craving for research of mine. It is nearly always there, showing up at the weirdest moments. With his help and understanding, I could pursue research while also being a dedicated mother. Achieving work-life balance is not easy, in particular when your job is your passion, as is often the case in academia.

In particular, I am thinking back of the time around the birth of my two children, in 1997 and 1999. Being used to work all the time, I felt lost and disconnected when I had to wind down and concentrate on being a full-time mom during these periods. In both cases, I continued to the very last moment, while my husband stopped working well ahead of the due date, so that he could drive me to the hospital when the time would come. After giving birth I needed to recover, and be there for the newborn. But the abrupt change made me feel awkward. Ten days after giving birth, I ended up going to a conference! I realize that I may sound like a bit of a workaholic, but the fact is that I never learned to “lay back” and relax, simply for the purpose of relaxing. Of course, I know that after a major change in life, one needs time to find a new equilibrium, but patience is also not my strongest point, you see.

My husband and I still laugh on occasion about these weird situations. After the births, he sometimes joined me at a conference so that he could take care of the newborn when I was attending lectures. For the sabbaticals and longer visits abroad, he knew I could not leave the children behind, and hence he arranged a leave from his job to travel with me, and took care of the children. He even took up the role as a surrogate teacher to make the travels with the children possible. We both love our jobs a lot, and this mutual understanding and help has been crucial to me in striking a good life balance.

What does the future hold in stock for you?

For one thing, I don't see myself sitting at home not doing research. Although there are a lot of things I would like to do when I have more time, I would also want to get back to a piece of paper (and laptop) and do some math/stat now and then. At the same time, I am fully aware that this might become impossible at some point. We have little or no control over what will happen to our body and mind as time unfolds... Only the future will tell.

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