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THE ET INTERVIEW: CHRISTIAN GOURIÉROUX AND ALAIN MONFORT

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Christian Gouriéroux



Alain Monfort

On a pleasant evening late in June 2010 the two Erics met with Alain and Christian in a Parisian restaurant. It appeared to us that French cuisine was the natural setting to talk about the French School of Econometrics. In fact, the menu read:

Apéritif offert par les Elèves des Grandes Ecoles
Terrine de pâté mesurable à la façon de Lebesgue
Petite surprise du Chef pseudo-vraisemblable
Steak sauce béarnaise aux attentes rationnelles et pommes frites belges
Petit colis de légumes avec inference indirecte

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Sélection de fromages création CREST
Crêpes bretonnes aux variables latentes
Expresso avec erreurs de specifications

It was a memorable dinner that started off the ET interview. The work of Alain and Christian spans both a long time period and a vast and diverse set of research subjects. While the French School of Econometrics stands for a large body of work to which many scholars contributed, it is their work that has shaped many of the core ideas and research themes. We wanted to have informal talks with Alain and Christian about what they thought would be the appropriate themes to cover. We had reserved a table in a quiet corner—and talked the entire evening—with only occasional interruptions by the waiter serving the food and wine. As the above menu suggests, the evening is best described as a journey through the history of econometric research over the past four decades. During the dinner, only mental notes were taken. The conversations had their natural flow, which we tried to keep in the published version.

In September 2010, the two Erics, back in the United States, sat down and started to write a long list of questions that we had extracted from the dinner conservations. Over the subsequent months we had a constant dialogue back and forth with questions and answers.

Peter Phillips approached us with the idea to conduct this interview. We are grateful to him for giving us the opportunity to interview Alain and Christian, whom we admire as scholars, as colleagues, as coauthors, and above all as longtime mentors and friends. Peter also provided very insightful feedback.

French high schools tend to have rigorous math education, and France has a long history of advanced mathematical training. Please tell us about your experiences and intellectual debt to the French educational system.

AM: When I was in high school, it was possible to choose a variety of disciplines, including Greek and Latin, and to postpone the specialization until the last year, i.e., the year of the "baccalaureat," which is the examination marking the end of secondary school study in France. I seized this opportunity of postponing the choice of a specialization and, therefore, I cannot say that I had advanced mathematical training at the high school level. However, after high school, I chose a scientific orientation and, as is frequent in France in the scientific domain, I decided not to enter the university system but one of these "classes preparatoires," which prepare students for the admission test to a "Grande Ecole." In these "classes preparatoires" the students have training in mathematics and physics, which lasts two or three years, and it is true that this training is very intensive and very efficient. After this training I was admitted to an old "Grande Ecole," Ecole Polytechnique (created just after the French revolution), and then to a young "Grande Ecole," ENSAE, which specialized in statistics and economics and was created in 1960. So, I was greatly influenced by the French educational system

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in the sense that I never studied at a university. Moreover, this partitioning of the French system also had a lasting impact on my teaching activity since, although I taught in several French "Grandes Ecoles," in particular ENSAE and Polytechnique, and several foreign universities, I never taught in a French university.

CG: I followed the same type of curriculum as Alain: I took the "classes preparatoires" and continued with courses at ENSAE, the School of Economics and Statistics. But, contrary to Alain, I entered the university system early, and studied mathematics in the Masters and Ph.D. programs at the university in parallel to the program at ENSAE. The specificity of the French educational system is that it selects students who are good in mathematics. This is both an advantage and a drawback. The advantage is that good students are not afraid to tackle quantitative problems, and have the analytical tools for independent research. The drawback is that a significant proportion of students continue to think that math is all that matters. This largely explains the strength of French research in mathematical economics and mathematical finance and its weakness in more applied fields.

With your rigorous math backgrounds, during your education, what brought you to choose econometrics and statistics.

AM: When I was a student at Ecole Polytechnique, i.e., between 1963 and 1966, statistics was not taught at all, as was also the case, a fortiori, for econometrics. There was no specific course in probability and, in any case, I had not been very interested in the probability training I received in the "classes preparatoires," because it was essentially made of combinatorial exercises, which I found sometimes funny but not very deep. In fact, a decisive turning point in my education was the fact that the professor of mathematics at Ecole Polytechnique was the great mathematician and outstanding pedagogue Laurent Schwartz. In his vast and wonderful course there were some sessions on probability theory that I found fascinating, and I realized only then that modeling hazard was not a hopeless enterprise. This course was a shock to me, but also to all the students I think, because it gave us the opportunity to see how a high-level mathematician was able to propose syntheses, bridges between different fields, and unifying frameworks. This unifying approach was in the spirit of the well-known "Bourbaki group," of which Laurent Schwartz was one of the main members. This very abstract and unifying approach of mathematics may have been a bit frightening for applied mathematicians, but it probably had a conscious or unconscious impact on our future research work. Many times Christian and I were tempted to choose a title for one of our papers beginning, "A general framework for. . . ." Coming back to your question on the origin of my choice of statistics and econometrics, it might indirectly be this discovery of a rigorous treatment of randomness that led me to choose the specialization in probability, statistics, and economics that was offered by ENSAE. However, since I simultaneously chose to become a civil servant at INSEE (the National Institute of Statistics) after my studies at ENSAE, I should

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have had a career as a civil servant. But a "zero probability event" occurred. At the end of my studies at ENSAE I was trying to select a position at INSEE from a list of vacancies. It was in May 1968, which was a very agitated period in France. After a strike the ENSAE students, who thought that the number of professors at ENSAE was much too small, obtained from the Directorate of INSEE the inclusion in the list of two positions of "assistant professor at ENSAE." I was fortunate enough to get one of them. So I had the opportunity to teach various topics like Fortran language, probability theory, linear programming, and statistics. In fact I learned statistics at the same time from classical textbooks by Lehman, Rao, Scheffe, and Wilks. I also read econometric textbooks by Malinvaud, Theil, and others, but I did not teach econometrics at this time. After four years I had to choose a more standard civil servant track at the Ministry of Finance, since the teaching positions were considered as transitory, and, normally, this should have been the end of my teaching period.

CG: When you follow the French system, you take exams to enter a Grande Ecole two years after the baccalaureat. Then, you wait for your result and enroll in the best ranked Ecole you are given access to. Personally, I've never liked chemistry. This considerably reduced my choices, and this is why I ended up at ENSAE. The courses at ENSAE were essentially in the fields of probability, statistics, and economics. As I became interested in teaching and pedagogical issues rather early, I passed the aggregation in mathematics to become a professor. An advantage of the French system is that you can sometimes be hired into a permanent position without having a deadline for your thesis defense. So I took my time. I worked for two years on continuous time Markov processes, I spent another two years in the Ivory Coast, and then I did research on survey sampling. I turned to econometrics when I was given the chance to join the team of Laffont and Monfort. I was about 29 years old.

What role did Edmond Malinvaud play and what intellectual influence did he have during your career?

AM: Edmond Malinvaud played a crucial role in my career. First, I have been very much influenced by his textbook *Méthodes Statistiques de l'Econométrie*, which was probably the first textbook to propose a rigorous bridge between statistics and econometrics. But, even more importantly, he is at the origin of a second "zero probability event." It was in 1974, I was a civil servant at the Ministry of Finance, doing some applied econometrics and dreaming of a sabbatical year in order to seriously learn econometrics. It was a dream because even the notion of a sabbatical year was unknown at the Ministry of Finance. But I decided to try to obtain the permission of the head manager of the Directorate of the Ministry where I was. This manager was Edmond Malinvaud, and he said yes. This "yes" obviously changed my career. Moreover, Edmond Malinvaud advised me to spend a sabbatical year at the London School of Economics (LSE), and this was an excellent piece of advice since this gave me the opportunity to meet outstanding

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econometricians like Denis Sargan, James Durbin, Ted Anderson (who was on leave at the LSE), and excellent younger econometricians like David Hendry, Ken Wallis, Grayham Mizon, Hashem Pesaran, and Pravin Trivedi.

CG: Edmond Malinvaud was my professor in macroeconomics at ENSAE. But clearly, I was too young and not sufficiently mature, and more inclined to study probability theory and related topics. I can remember Malinvaud's lecture on Marx as a classical economist. He just kept reading a chapter from Marx for one hour, without making any comments. This was likely to avoid political questions from students, as it was right after the events of 1968!! But, from a pedagogical point of view, it was a very interesting experiment. After that class, I read the entire book to see if he had chosen the chapter in an endogenous way, a kind of selectivity bias. But, seriously, Edmond Malinvaud played a primary role in French education, as he created a new program in econometrics at ENSAE, which later became known as the School of Statistics. The curriculum for that new program was a perfect mixture of economics, mathematics, and statistics. It seemed to be the exact implementation of the definition of econometrics given by R. Frisch in his editorial of the first issue of *Econometrica*. Malinvaud in fact wrote several textbooks in micro, macro, and econometrics for the school. That new program did not exist when Alain was a student at ENSAE, as it was introduced later as a complete novelty. Malinvaud's curriculum is still offered by ENSAE, and kept regularly updated, which includes the publication of new advanced textbooks. It has also been introduced in some French universities, especially at Paris Dauphine and Toulouse. It became a perfect complement to the French math-based type of education.

Alain, some of your very first papers (even before your joint work with Christian) were with Jean-Jacques Laffont. Moreover, both you and Christian wrote many important papers with Laffont on disequilibrium econometrics. Can you tell us how this collaboration started and what you think was Laffont's legacy and influence on the French school of econometrics?

AM: I first had some contacts with Jean-Jacques when he was a student at ENSAE, between 1970 and 1972, but I started working with him after his stay in the United States and Canada. When he came back from Montreal, in 1975, I was just coming back from the LSE and I had been appointed as Professor of Statistics at ENSAE. Jean-Jacques had obtained a position as researcher at CNRS and he became a member of the Laboratoire d'Econometrie de l'Ecole Polytechnique, headed by Claude Henry. Jean-Jacques had just defended his Ph.D. thesis under Kenneth Arrow's supervision, but he was also interested in econometrics, and he had just finished a paper with Dale Jorgenson on the estimation of nonlinear simultaneous equations. He was also more and more interested in fixed-price equilibrium, or disequilibrium. In fact, many economists in France were working on this subject either at INSEE or at CEPREMAP, but the econometric side was

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not developed at all. Jean-Jacques, who has always been interested in both the economic and the statistical aspects of the problems he met, thought that it was important to try to fill this gap. And we started working on this theme. Before this joint work with Jean-Jacques, I was mainly interested in rather abstract statistical problems, like coordinate-free approaches to linear models or identifiability, and Jean-Jacques' economic culture clearly played a decisive role in the orientation of my research themes. Another decisive event was the appointment of Christian Gouriéroux as Professor of Statistics at ENSAE in 1976. Christian was looking for new research themes. He very quickly became interested in the fields proposed by Jean-Jacques, and he brought important new ideas. I think that Christian will agree if I say that the extraordinarily large spectrum of Jean-Jacques' knowledge, as well as his stimulating enthusiasm, were the starting point of our joint work.

CG: I was the youngest one in the group, but we all came from the same generation. Back then, the publications on disequilibrium, and also on rational expectations, motivated us to learn about those new developments in macroeconomics. I can say that, in some sense, we studied together, at our own pace, especially the rational expectations models, on which there existed a huge but rather blurry literature. We spent more than one year on rational expectations without finding anything interesting to pursue, but we were obstinate, motivated, and doing research together was a real pleasure. But you would be mistaken to think that our group was isolated. In fact, we had a lot of discussions with institutions such as INSEE and CEPREMAP, with several other young people like Jean Pascal Benassy or Guy Laroque on disequilibrium, and Roger Guesnerie on rational expectations. In fact, our job was to adapt economic theory to make it applicable to economic practice, that is, to write the econometric models for applications, discuss the identification issues, and explore the possible multiple equilibria and also to propose the appropriate estimation methods—hence, a coordinated program.

As you noted, both of you worked on econometrics of Rational Expectations with Laffont, and Christian, you continued to work on this topic with Fourgeaud and Pradel and also with Broze and Szafarz. What inspired this research, and how did it relate to work on structural macroeconometrics done elsewhere, such as at the Cowles Foundation?

AM: Rational expectations was another important topic that Jean-Jacques had in mind. This domain was less popular in France than disequilibrium, but Jean-Jacques was convinced that, in any case, it was necessary to build econometric tools to test this theory, and he had no difficulty in persuading Christian and me to work in this field. The main problem we first encountered was the multiplicity of the solutions when expectations of future endogenous variables are involved in the behavior of current variables. We found that the situation was

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even worse than expected since, even in cases of uniqueness of a stationary solution, there are an infinite number of asymptotically stationary solutions, which cannot be discarded by excluding explosive processes. In this respect Christian, with several coauthors, did a very good job in describing the whole set of solutions in a general setting. Rational expectations are now basic tools in the popular dynamic stochastic general equilibrium (DSGE) models. However, the selection of the unique particular solution that is exactly stationary still does not seem fully justified, since there exist other solutions that are equal to this stationary solution after a random date, such as trajectories with bubbles.

CG: As you mentioned in your question, the basic paper was written with Alain and Jean-Jacques, and the topic was next explored with other coauthors. This enlargement of the group was due to the fact that Jean-Jacques took a new position at Toulouse University and started creating the Institut d'Economie Industrielle (IDEI), which is now the Toulouse School of Economics. At the same time, I accepted a new position at Lille University, and we tried to introduce econometrics at other universities. My residence in Lille explains why my coauthors were either from Paris or Belgium. The initial paper considered the multiplicity of rational expectation equilibria. The next projects were meant to provide a better understanding of this multiplicity problem, like, for instance, to analyze the limits of learning procedures, or to see if the multiplicity disappears in nonlinear dynamic rational expectation models. Incidentally, the first time I studied financial markets was in the context of the transmission of information in market equilibrium under stochastic volatility. I guess that the market equilibria multiplicity problem is still not well understood. It is likely linked to the problem of incompleteness in financial markets, but the relation between these notions is unclear, at least to us.

One of the defining themes of the French school is the econometrics of misspecified models, and your work has been pathbreaking in this area. What brought you to the subject?

AM: In the early eighties the notion of pseudo-true value had been already introduced by Sawa, and the asymptotic behavior of the maximum likelihood estimator in misspecified models, in particular its convergence to the pseudo-true value of the parameter vector, had been studied by Hal White. However, there was no work on the conditions under which this pseudo-true value of the parameter is equal to its true value based only on conditional moments. So, we started working with Christian and Alain Trognon on this rather natural question, and we found that it was possible to find simple characterizations of the class of misspecified parametric models providing consistent and asymptotically normal "pseudo maximum likelihood" estimators of the true value of the parameter. In fact, Hood and Koopmans had already found that Gaussian misspecified models belong to this class and had called the estimators thus derived "quasi maximum likelihood" estimators. We preferred to use the term "pseudo" in order to characterize the

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whole class, which is much larger than the Gaussian class. With this work we realized that misspecified models may play a positive role, and it opened the way to other studies in the same spirit, in particular, to studies on encompassing, studies on indirect inference with Eric Renault, and very recent studies on fourth-order pseudo maximum likelihood methods with Alberto Holly and Michael Rockinger.

CG: As for the rational expectation models, they were among the first topics of our joint research. However, misspecification has always been to us the field of primary importance. It became a new major topic of research in econometrics at the time when we wrote the papers on the pseudo maximum likelihood method. Back then, Halbert White was working on the misspecification test, and Lars Hansen and Ken Singleton were developing the generalized method of moments, for instance. We got a clear vision of the idea of the problem after a talk on an application of Poisson models to patent data, given by Jerry Hausman at a meeting in Lausanne. We used that example of count data as a starting point, and wrote the core part of our papers on the train during our return trip from Lausanne to Paris. In those days, Swiss trains used to roll along rather slowly, long before the high speed trains were introduced. Therefore we had enough time for a discussion.

Alain, you presented a Cowles Foundation lecture on a reappraisal of misspecified models (1996, ET 12, 597–619). Tell us a bit about your lecture and, if you had to present the lecture today, what would you like to add or say differently?

AM: It was a very interesting and a bit of an intimidating experience, because I knew that it was only the fourth of this series of lectures in honor of Tjalling Koopmans, that I was the first econometrician to give this lecture, and that it would be attended by outstanding econometricians, including Peter Phillips, Don Andrews, and Chris Sims. In fact, it was when I worked on the preparation of this conference (in the first half of 1994) that I fully realized that many of our previous works with Christian, Alain, and Eric had some a posteriori coherence: namely, the positive role that might be played by misspecified models, and that is the reason I chose the title "A reappraisal of misspecified models." The lecture was organized around 10 examples connected with this general theme, and if I had to give this lecture today, I think that I would keep the same material, but I would also add some other stories. For instance, I would probably give examples about the choice of the auxiliary criterion in the indirect inference method, in particular about the possibility of using the criteria based on nonparametric estimators of conditional moments that we proposed with Monica Billio and also about the fourth-order pseudo maximum likelihood method I already mentioned.

You already mentioned your work on encompassing. Can you elaborate a bit more on how it fits into this paradigm and how it relates to work by David Hendry and others on this topic?

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AM: At the beginning of the 1990s we had worked on several aspects of misspecified models. In particular, we had studied nonnested hypotheses testing. More precisely we had examined the behavior of classical asymptotic tests when the two hypotheses are nonnested parametric families. In this work we assumed that the true distribution belonged to one of the two families. So the natural next question was: What happens if the true distribution does not belong to any of the families? This question was obviously connected with the work on encompassing by Hendry, Florens, Richard, and Mizon. We developed tools like, for example, the notions of binding functions, indirect identification, indirect information, and simulated binding functions, in order to propose a set of testing procedures of the notion of encompassing of one model by another, in the general context of dynamic models, and without assuming that the true distribution belongs to one of the models.

CG: While we continued to do research on misspecified models, our understanding of this problem has since evolved. Initially, we focused on estimation methods that were robust to a given type of misspecification. The pseudo-likelihood is an example of such a method. Later on, we realized that misspecified models could be a useful tool or instrument for providing information on other well- or misspecified models. The idea became so obvious to us in the context of encompassing models that we formalized it as a problem of nonnested hypotheses. It is even more clear in indirect inference, where the model of interest is difficult to estimate but easy to simulate, and then a misspecified model is used as an instrument to help estimate the model of interest.

Can you be more specific about how the notion of encompassing in Hendry's work compares to yours?

CG: As mentioned by Alain, the notion of encompassing by David Hendry assumes that the true distribution is necessarily in one of the two competing models, whereas our extended analysis allows for the comparison of two misspecified models. The difference can be significant, as noted by the example of nested models; indeed, a model can nest another one without encompassing it, even in a linear framework. But more importantly, some deduced estimation methods explain how to collect and gather in an efficient way the information about the true distribution contained in several misspecified estimated models. Loosely speaking, it is similar to reconstituting a 10-dimensional figure from several 2-dimensional pictures. Thus, the methodology does not only provide model choice principles, but also estimation approaches.

AM: I would just add that, in our approach, we focus more on the notions of finite sample and asymptotic binding functions; we insist on the similarities and the differences with the theory of nonnested hypotheses testing, and we also make a connection with simulation-based econometric methods, since some test procedures proposed in our work necessitate Monte-Carlo approximations of the

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binding functions. This notion of simulated binding functions has since been extensively used in our work with Eric Renault on indirect inference.

You have greatly influenced the profession through the writing of many textbooks and research monographs. Many of your textbooks contain innovative material. Can you tell us something about how these textbooks were written and how they shaped your research?

AM: I would say that there are two kinds of textbooks. A first set of books are those based on courses given at ENSAE, in particular several books (in French) on probability, statistics, survey sampling, insurance models, credit risk, or GARCH models. Other books are syntheses of larger fields, like the two volumes on statistics and econometric models, the time series book, the simulation-based methods, the books (written by Christian and Joann Jasiak) on financial econometrics, or the econometrics of individual risk. It is true that the large scope and the unifying spirit of the second kind of books implied innovative developments in order to fill some gaps and, on some occasions, this initiated new research.

CG: Our aim was not to influence the profession, but to continue the kind of development initiated by Edmond Malinvaud in the School of Statistics. We first completed a series of advanced textbooks, which included two volumes of a comprehensive text in econometrics, and a more specialized text in time series. Later on, as the research programs kept changing in parallel with the new developments in econometric methodology, the new more specialized textbooks focused on new statistical methods, such as the book on simulation-based estimation methods, or on quickly evolving applied fields. With respect to the latter, I should note that, in particular, in the areas of insurance and finance, French and English books with rather different contents may coexist in the same field. Our idea was to publish first in French on all the topics of strategic importance to the French banks and the Paris Stock Exchange, such as advanced methodologies for credit scoring, or the implementation of Basel 2 regulation. In this case, the first objective was not to influence the academic profession, but to leave enough time for French institutions to implement the new methodologies in a very competitive environment. The books on other topics in finance were generally published directly in English.

One could in fact build an entire Ph.D. curriculum in econometrics based on your books. Looking at it today, if you had to write another book on econometric theory, what would it cover?

AM: In fact, we did not really write a book on econometric theory. As far as I am concerned, I think that it would be interesting to write a book that would present a unifying approach, like in our two volumes on statistics and econometric models, but including also dynamic models and simulation-based methods. The theory of statistical methods based on extremal estimators provides a convenient framework for such a general approach. Again, it would be in agreement with our

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attraction to general frameworks. However this would be a big work, and I do not know if we will undertake it. Another possibility would be to write a book on the econometrics of asset pricing and on its connections with macroeconometrics.

CG: I am currently writing a book on granularity theory with Patrick Gagliardini, which considers the questions of estimation and prediction in large panels with factors. In parallel the book discusses the measures of risk and the pricing of large portfolios. While these problems can also be solved by simulation methods, the simulations may be too time-consuming in practice. This is especially relevant in financial applications, where the computations need to be carried out almost in real time, and concern portfolios including several hundred thousands of contracts. The idea is to get closed-form approximate formulas by an appropriate use of the large amount of cross-sectional information.

If you don't mind, let's change the subject a bit. The early 1980s are marked by the work of Lars Hansen on GMM and your work (with Alain Trognon) on pseudo maximum likelihood (PML) and asymptotic least squares (ALS). Thirty years later, how do you look upon the relationship between these various contributions?

AM: The objectives of PML and GMM methods are similar, namely inference in semiparametric models defined by restrictions on conditional moments, but the spirit of the methods is different. As far as the ALS methods are concerned, they have been perceived as very close to the GMM methods although they are very different. Indeed, the ALS methods are based on mixed constraints on a parameter associated with a parametric or semiparametric model, and the goal is either to estimate the second parameter appearing in the mixed constraints or to test whether these mixed hypotheses are satisfied by the initial parameter. These testing procedures are particularly simple when the mixed hypotheses are linear with respect to the second parameter. These objectives are clearly completely different from those of the GMM, and we never really understood why they have been seen as very similar. Moreover the ALS can be seen as an extension of various methods like minimum distance method, minimum chi-square method, or Berkson's method, and not as an extension of moment methods.

CG: In fact, Lars's work has two components. The first one is the estimation method. From this point of view, it can be compared with pseudo maximum likelihood and asymptotic least squares, and also with maximum likelihood itself, as shown by Kitamura and others, who gave the empirical likelihood interpretation of GMM. The second component of Lars's work is an important modeling strategy, which is made very clear with the example of the CCAPM given by Hansen and Singleton. The semiparametric models often have to be specified by means of the conditional moment restrictions to get the right economic interpretation. To me, the main legacy of the GMM is that modeling strategy.

Well, talking again about modeling strategy—do you mind talking about generalized residuals? The notion of generalized residuals (your

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work with Eric Renault and Alain Trognon) focuses on specific directions of misspecifications, a lot of the literature has focused on general omnibus tests. What role do you think does or should specification testing play in econometrics?

AM: Specification tests obviously play, or should play, a crucial role in modeling strategy. Since a model can never be validated, its strength can only come from the fact that it has not been invalidated when embedded in larger models. This is precisely the role of specification tests. These tests are probably not used enough in practice, and the introduction of generalized residuals was aimed at making these tests more intuitive and, hopefully, more attractive for practitioners.

CG: Yes, our aim was to provide diagnostic tools that are both relevant and easy to use by the practitioners. In this respect, I always prefer residual plots to omnibus test statistics, and appropriate autocorrelograms such as, for instance, nonlinear autocorrelograms, to an ad hoc portmanteau statistic. I also prefer a battery of risk measures instead of a single VaR at 5% in financial applications.

Beyond your work on generalized residuals, you have often resorted to latent variables for model specification (your joint work on factor models; Alain—your work on regime switching models with Monica Billio, Christian Robert, and Fulvio Pegoraro; Christian—your work with Eric Ghysels and Joann Jasiak on stochastic volatility durations). What do you think are the advantages of using latent factor models?

CG: Let me answer for the case of financial applications. Latent factors have an advantage not only with regard to prediction. They are the only technique that is currently available to account for common risks in models used for fixing the required capital, managing a portfolio, or pricing basket derivatives. Examples of such latent factors are the cycles, the so-called, but not well-defined, systemic risk, or the longevity risk due to an uncertain overall increase of human lifetime. In this respect, it was a good decision of the regulators to require the presence of latent factors in the credit risk models of Basel 2, even though adopting a static Gaussian specification of those factors in the basic Basel 2 approach was a less bright idea. The introduction of factors can also be important from a more conceptual point of view. A typical example is the introduction of factors with nonlinear dynamic effects by means of stochastic time deformation. This questions the notion of liquidity, trading activity, and more generally leads to thinking about what is time.

AM: There are advantages and disadvantages in using latent variables. The main advantage is that the introduction of latent variables creates flexibility in the dynamics of the system; in particular, introducing switching regimes may be particularly useful in many fields, for instance in modeling interest rates. However, continuing with the example of interest rates, working uniquely with initial latent factors and then using the functions giving observed rates in terms of

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these factors can create severe problems. In particular, in such an approach the dynamics of the latent factors are estimated in a very indirect way and, therefore, it is difficult to implement specification tests regarding their dynamics and to treat in a satisfactory way very important problems like persistence. Moreover, the economic interpretation of latent factors is often not obvious. So, I am not sure that the extreme approach starting from latent factors only and then deriving measurement equations for observable variables is necessarily the best one. In many instances a mixture of observable and latent variables is preferable.

Your interest in latent factor models brings us to the subject of simulation-based methods in econometrics. How do you look upon the tension between the prevalence of misspecified models in econometrics and the notion of simulating a "true" data generating process?

CG: This is a difficult question, which can be addressed to all applied fields. It is clear that any model is an approximation of the real world, and therefore is misspecified. It is also clear that the complexity of a model depends on the domain, even for economic applications, and it is often a function of the future use of this model. Typically, for instance in financial applications, precision in derivative pricing matters, and this goal is expected to be reached by using more sophisticated models. "More sophisticated" means more sophisticated than the model of market competitors, although all these sophisticated models can be seriously misspecified. What I want to say is simply that the aim is not necessarily to find a well-specified model, or a true generating process. I would prefer instead to find a model, or an estimator, or a prediction formula that is sufficiently robust to some amount of misspecification and simple enough to be understood. If such a robust model were used in economic decision making, the outcome would be more reliable, or even more socially beneficial, than an outcome of a sophisticated analysis strategy. This is what I would prefer, but am unable to do.

AM: The simulation-based econometric methods have considerably widened the set of parametric models for which inference methods are available. However, these new possibilities raise the problem of choosing between sophisticated parametric models and simpler semi-parametric models, on the basis of their risk of misspecification. It is an interesting problem, which has not often been considered, with the notable exception of the work by Eric Renault and Ramdan Dridi, who proposed a generalization of indirect inference methods to a semi-parametric context.

Both of you worked tirelessly to build institutions promoting research. For example, could you please tell us how and why CREST was created?

CG: At some point it became clear to us that a school like ENSAE needed an associated research center to supervise Ph.D. students, and to provide the

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technological backup necessary for keeping the program up-to-date in the domains of economics, finance, and statistics, and to attract young, promising researchers. However, the creation of CREST had to overcome the rigidity of the administrative structure: the School of Statistics, ENSAE, belongs to the National Statistical Institute (INSEE), which back then had a (small) research department. Alain was the Director of that center. It took several steps to create CREST. First, following a suggestion from Jacques Mairesse, we chose a name that is easy to understand both in French and English, and is familiar to the Anglo-Saxon world, as in this case it is the name of a toothpaste brand, for instance. Then, an initial budget was set for the Ph.D. grants, essentially. During the first two to three years, CREST was home to some carefully selected Ph.D. students who already had supervisors in the graduate schools they came from, and to whom we offered a better research environment than the other French institutions in terms of grants, seminars, computers, data sets, as well as additional supervision and job market assistance. This first generation of our Ph.D. students included Jean-Michel Zakoian, Nizar Touzi, Christian Robert, and Jean-Marc Robin, to mention a few. The second step that lasted another two to three years was the preparation of a merger with the research department of INSEE: We rather quickly increased the size of CREST to 20 financed Ph.D. students, and we hired several researchers for graduate supervision, such as Jacqueline Pradel, Bruno Jullien, and Patrick Rey. Next, we benefited from the fact that a part of INSEE moved to a new building and physically reunited CREST and the research department of INSEE, even before the merger formally took place. The name CREST was maintained as the name of the new center that included four laboratories: the research department, a laboratory in statistics, a laboratory in industrial organization, and one in finance-insurance. Alain became the head of CREST, Patrick Rey became the head of Industrial Organization, and Christian Robert was the head of Statistics. My role was first to create and then to chair a new finance-insurance research laboratory. Since the beginning, CREST has played a very special role in INSEE, and implicitly in the Ministry of Finance, as CREST is the only research center that officially belongs to that Ministry. At CREST we ensure the links between the administration and the academic world, and also provide a connection with the private sector, that is banks and insurance companies. That connection exists through some of the academics who belong to CREST, while the INSEE administrators are not allowed to interact with the private sector. Currently, CREST has all the aforementioned laboratories plus macro-econometrics, microeconometrics, and quantitative sociology, and has about 120 researchers and 40 Ph.D. students.

AM: I just would like to add some comments about the merging, in 1993, between the first version of CREST, created a few years before within ENSAE, and the research department of INSEE. As Christian said, this research department was small, approximately 15 people, but it played an important role in putting forward a research culture within INSEE. I think that without this preexisting culture, INSEE would not have allocated the same funds to the creation of CREST.

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In fact, at the time of the creation of CREST in 1993, the research department had already been in existence for 25 years. It was created by Edmond Malinvaud in 1968, and in the seventies it was headed by Pascal Mazodier, who did a good job in opening this center to outstanding foreign researchers. Many of them were invited for periods of various lengths and sometimes jointly with the Laboratoire d'Econometrie de l'Ecole Polytechnique, of which Alberto Holly was a member. Among the invited researchers we can mention were Peter Phillips, Marc Nerlove, Ron Gallant, Rob Engle, Ken Wallis, Hal White, Richard Smith, Jerry Hausman, and Angus Deaton. However, in spite of this important opening to international research, the research department was mainly orientated toward the methodological improvement of economic studies at INSEE and had small links with ENSAE and with the university world. A major change through the creation of CREST has been to greatly increase the links with the French universities, in particular through research grants.

Did CREST evolve since its creation in the eighties according to your expectations? Exceed your expectations? What is the lasting impact of the existence of CREST? What do you think is next?

CG: As you know, it is rather easy to create a research center, but it is more difficult to keep it alive. The center has been in existence now for more than 20 years. It is necessary to continuously maintain the inflow of new people in order to preserve its international reputation. This is not always easily done in periods of increased competition with other research centers in France, such as the Toulouse School of Economics, or Delta now as a part of the Paris School of Economics, and also with increased competition at the international level. Our policy for solving this problem is threefold: first, try to keep the top rankings in some fields, such as labor economics, financial econometrics, insurance, and statistics of diffusion processes; second, continue to manage a rather large number of well-selected Ph.D. students, especially in industrial organization and finance. We know that, after their thesis, several of them will be attracted by the Toulouse School of Economics or by the graduate schools in the United States, but we need a sufficient number of them to stay close to CREST. Those students will get jobs in various institutions and will become parts of a network in which CREST is integrated. Third, we have the possibility of adjusting rather quickly our number of laboratories and topics to the new developments in research, and to adapt to the needs of the job market for students. What's next? Likely, in the medium term, mergers of some laboratories of CREST with the laboratories of other research centers in Paris in order to improve or strengthen some fields. But we are looking for cooperative mergers. We don't want either to be swallowed, or to fight the others. And such cooperative mergers take time.

AM: Yes, I would say that the evolution of CREST in the nineties has been what we were hoping for, and probably a bit more. As I said, the main uncertainty was the degree of involvement of INSEE in terms of funding, buildings, researcher and

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administrative positions, and student grants. After all, INSEE is not a research institution, but an official Institute of Statistics and Economics associated with the Ministry of Finance. In this respect, we have been very lucky to have, as heads of INSEE, Edmond Malinvaud, who supported the idea of a joint research center at an early stage, and then Jean Claude Milleron and Paul Champsaur, who also have been of great help in promoting research within INSEE. As far as the future of CREST is concerned, my feelings are mixed for several reasons. First, the French academic landscape is evolving fast: The universities are becoming more autonomous, some private funds are appearing, in particular through the creation of chairs or research laboratories; second, the specific situation of ENSAE will also change: A new status making it more independent of INSEE has been defined, its physical location will change, and the new location will no longer be in downtown Paris, but on a campus south of Paris. So it is clear that, in this new environment, CREST will have to find a new equilibrium.

Despite the enormous success of CREST, don't you feel that more of the best students in the French system tend to leave for North American Ph.D. programs than, say, 20 years ago and, even more importantly, stay abroad for their academic careers?

CG: Concerning the students, our aim was twofold: to increase the number of students defending a Ph.D. thesis and to provide an opening to other European, American, or Asian systems. Thus, this is a positive development if a larger number of our students are joining other academic systems or stay abroad for their career in the academic or private sectors, as long as we, first, keep strong links with these researchers abroad; second, we are able to keep enough French researchers in our own French research centers; and third, we are able to attract to these centers foreign students and researchers. We just have to manage appropriately our population of Ph.D. students and researchers.

AM: It is difficult to have a precise evaluation of this problem for the French system as a whole. As far as CREST is concerned, as Christian said, the center has always been open to other countries, and we greatly encouraged movements in both directions. When we created CREST, it was clear to us that, given the position of CREST within INSEE, such a center would not provide careers for young doctorates and that it was very important to create links with the academic world, in France and in other countries. This international option implied that the proportion of foreign Ph.D. students at CREST has always been high and, conversely, it is true that a significant proportion of French students choose the North American system, but I do not feel that the balance between the two kinds of movements has much changed recently. Moreover, like Christian, I think that as long as this balance remains reasonable, these exchanges between different countries are more a strength than a weakness for our center.

Alain, starting in the late 90s you were editor of *Econometrica*. What remains with you today about this that you would like to share with

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us? Do you think it gave you the possibility to have some lasting impact on the evolution of the journal? Also, do you have any thoughts on how the journal has evolved since those years?

AM: This was an interesting but frustrating experience. In my view the main problem of *Econometrica* is a problem of balance: balance between economic theory and econometrics and, within econometrics, balance between theoretical and applied econometrics. When I was appointed as Co-Editor in 1996, I had these ideas in mind, and I was hoping to be able to modify these balances and to promote more econometrics and especially more applied econometrics. However, I quickly realized that everything is driven by the structure of the submitted manuscripts, and that it is very difficult to modify this supply. There is a strong self censorship among the authors, and it is not easy to fight against that. For instance, the Frisch Medal is clearly not a sufficient incentive. I supported the idea of creating new journals associated with the Econometric Society, but I was very surprised to see that it was not envisaged to create an econometric journal and, consequently, I have not been surprised to hear that some people, in particular Peter Phillips and David Hendry, were considering creating another international society devoted to econometrics. The project of creating an Econometric Foundation, viewed as a complement of the Econometric Society, is a good idea.

In recent years you turned to research in financial econometrics. What motivated you to work in this area?

CG: My involvement in finance is not so recent, since it started 30 years ago at the beginning of the 80s. However, unlike other French probabilists who specialized in derivative pricing, I used my knowledge of microeconometrics and specialized in the scoring techniques for individual contracts, credits, and car insurances. This is hard to infer from my curriculum vitae, as the credit scoring technology was highly confidential to protect the interests of the banks. Nevertheless, the paper on pseudo maximum likelihood estimation of a Poisson model became a basic reference on premium updating in car insurance (the so-called bonus-malus). Later on, the confidentiality restrictions were relaxed and partly removed for two reasons. At the time of the first French securitizations, in which I was involved, it appeared necessary to provide some reliable information on the pool of securitized credits to potential investors. Providing such information to investors is the French way of doing business, which is clearly different from the U.S. way, as seen from the recent U.S. sub-prime crisis. The second reason was of course the Basel 2 regulation for credit risk. Due to the securitization, we were obliged to know the design of credit derivatives, their prices, and the capital required to hedge their risk. As we had the knowledge of individual contracts, we did not come up with a battery of continuous time models, like the Wall Street methodology, but instead we tried to reconcile the microscopic and macroscopic approaches to the problem. In parallel to Darell Duffie and Ken Singleton, we realized the importance of compound autoregressive models, which are called affine models in continuous time.

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We contributed to the literature on affine models by introducing the Wishart processes in finance, that are multivariate extensions of the standard Cox, Ingersoll, Ross model for stochastic volatility. More recently, we developed, with Patrick Gagliardini, granularity theory, a method of obtaining accurate closed-form formulas of prices, financial reserves, or of the VaR for large portfolios of individual contracts under systematic risk. You should not be surprised that we continue to work on this topic, with focus on systematic risk in the framework of Basel 3. We have also learned to perceive finance in our own way, due to the background knowledge of duration models and event analysis. In fact, several people from CREST were involved in the change of status of the Paris Bourse, now NYSE-Euronext, especially in the introduction and testing of the electronic trading system, the CAC system, and in the construction and management of market indices. Thus, it is not surprising that we tried to understand the problems of liquidity and their price impacts. For instance, we introduced some summary statistics for market activity, such as the activity measure or time-at-risk, in a joint work with Gaelle Lefol, Joann Jasiak, and Eric Ghysels. More recently, we looked at a way of taking into account the lack of activity on the option market in the recent paper on the extended method of moments, XMM, with Patrick Gagliardini and the other Eric.

AM: My involvement in financial econometrics is more recent than Christian's. In fact, I was driven to this area first because of Eric R. and Christian's involvement and also because I quickly realized that econometric methods were not much used in a field largely dominated, at least in France, by purely probabilistic models of pricing based on continuous time models. However I tried to go on working simultaneously on econometric theory, for instance on simulation-based methods with Monica Billio and Christian P. Robert, or on fourth-order pseudo maximum likelihood methods with Alberto Holly and Michael Rockinger. More recently, I moved on to macrofinance, which is a nice mixture of macroeconometrics and finance. For instance I studied with Fulvio Pegoraro and Caroline Jardet the problem of the measure of persistence in near-cointegrated models, which are frequently encountered in this field. We mainly considered this problem in the context of measuring long-term premia, and we found that a proper measure of persistence is a key issue in this domain, but we believe that it is also the case for other long-term problems like the long-term behavior of portfolios performances, the evaluation of long term VaRs, or longevity risk.

What are your thoughts on the future of the field of financial econometrics?

CG: I can provide a clear answer to another question that is directly related to yours: What will be the new research topics in financial econometrics at CREST? Of course, regulation and systematic risk analysis of products other than the standard products already regulated under that Basel 1 or 2 agreements. We are currently creating a new network of databases on small French hedge funds in order

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to improve the quality and the diffusion of information concerning their returns, portfolio allocations, and manager incentives. These databases are necessary not only to examine their risks and their characteristics, but also to provide information to small household investors, if the hedge fund market is to be open to them in the future. Another topic of interest is the longevity risk with the upcoming securitizations of life insurance derivatives and the issuing of longevity bonds. In addition, we expect the appearance of T-bonds with very long maturities of 30 or even 50 years. What is the interest for econometric modeling? The fact that the dynamic models that exist in the literature are poorly adapted to examining the long-term effects. For instance, all standard term structure models have a constant long-term interest rate. The first data on long-term risk-free bonds clearly show that the 30-year rates are much more volatile than the 10-year rates. An interesting question to explore is: How to make the existing models compatible with this stylized fact? Should we still be assuming no arbitrage opportunity? How to estimate the long-term parameters in the new models and how to use the new data sets that will arrive in three to four years?

AM: I think that one of the main problems financial econometrics will have to cope with in the future is the old problem of aggregation, I mean both time aggregation and cross-section aggregation, which are not independent. As far as the time unit is concerned, it is striking to see the huge difference between microstructure models working with a time unit that is very small (in particular, in high-frequency trading problems) and, for instance, the study of very long-term bounds. Does that mean that we will have a partitioning of financial econometrics according to the time unit retained, or is there some hope of finding some time aggregation methods allowing us to connect at least some elements of the partition? Answering this question will be of primary importance. The cross-section aggregation raises the problem of the link between macroeconomic aggregates like GDP, inflation, unemployment, and financial variables like interest rates, term premia, default spreads. In this respect it is surprising that, for a long time, financial models of interest rates have completely ignored the interaction with macrovariables and monetary policy of central banks. So, I think, or at least I hope, that in the future, financial econometrics will be less partitioned and that some bridges between these various subfields will be built.

What do you think will be the role of financial econometrics beyond regulation and macroeconomic policy you mentioned? In particular, where do you see the most important impact of the field on financial market practitioners in the future?

CG: I see the question as follows: How to diffuse the research in financial econometrics among professionals in an appropriate way? Currently, the main channel is clearly by means of the newly recruited professionals; we expect that they have followed regularly updated programs in their academic curriculum. However, there can exist a gap between theory and practice: They can join departments

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with a different culture, or institutions with missing or weakly reliable databases, and be subject to short-term deadlines not compatible with the necessary time to think. The main difficulty is to manage and standardize the technological transfers. In this respect, I think that the most important component of the regulations is not necessarily the derivation of formulas to compute the reserves, but the minimal databases needed for transparency, the standardization of some derivative products and, as a consequence, the standardization of the associated analyzing and pricing methodologies, which can then be better known and understood. Also important are the underlying back and forth discussions to compare the methods internal to the banks and the ones proposed by the specialized agencies. This is step-by-step diffusion, which seems difficult to control.

AM: Beyond the specific cases of regulation and macroeconomics, I hope that financial econometrics will play an important role in the promotion of statistical methods and statistical culture, because I am always surprised to see that the bases of the statistical approach are often ignored in the financial industry. For instance, even the notions of a statistical model or of hypothesis testing are often not well understood and, therefore, misused or not used at all. I am convinced that problems could have been avoided in many instances if hasty and short-lived calibrations had been replaced by genuine statistical modeling, including diagnostic tests, significance tests, and efficient estimation methods. If financial econometrics is able to improve this situation in the future, this will be a great success.

Alain, you have worked quite a bit on term structure models—exploring notably the role of regime switching models. You have given the topic a lot of thought. What prompted you to go in that direction?

AM: I have been led to this field because I am now working with several members of the Banque de France, in particular Caroline Jardet, Fulvio Pegoraro, and Jean-Paul Renne, who are interested in the links between sovereign interest rates and real activity, more specially in the computation and the role of term premia, and in the impact of financial crises in the joint dynamics of these kinds of variables. As I already said, a main problem in this field is the appropriate treatment of the persistence of the dynamics. One way to tackle this question is the averaging estimator method. Another is the introduction of regime-switching models, the idea being that the persistence is mainly due to the persistence of regimes. Moreover, switching regimes models are clearly appropriate for evaluating the impact of some crises. Finally, it turns out that, if the regimes are properly coded, the regime-switching feature can be incorporated in the CAR (or affine) or extended CAR methodology. In particular, we can have models with switching parameters that, at the same time, provide explicit or quasi explicit formulas for yields of any maturity, including corporate yields. We have recently developed, with Jean Paul Renne, a general econometric framework using this property that seems promising.

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Christian, there is a lot of discussion about the Basel 3 bank capital requirements. This is a topic that you have researched extensively. Where do you think we are heading on this topic, and how much academic research in the field has affected the new proposed regulation?

CG: The links between academic research and regulation have to be considered in the long run. Let us first consider Basel 1. The regulation was established after a lot of back and forth between the regulators and the financial institutions. Where were the academics in this process? First, a lot of them were scientific advisers to banks, and were indirectly involved in the process. Second, the implementation of the new regulation has to be carried out by people with an advanced level of technological expertise within the banks and among the regulators. Academics are training those new young professionals. For instance, the students who will become civil servants for French regulators take courses from ENSAE, and several of them end up hired by the private sector. What about Basel 3? The decision to move forward to Basel 3, while Basel 2 has not been yet properly implemented in a number of countries including the United States was a hastily taken decision during the crisis with a short and unrealistic deadline of 2011. The academics have not been much involved in the discussions concerning that decision because of the attacks of the mass media about the alleged role of mathematics in pricing securitized assets, and in the development of the recent financial crisis. The new revised deadline, at least for liquidity risk, is now 2019. This gives academic researchers enough time to get involved in the usual back and forth, as well as for training students who may work under Basel 3 in the future.

You just mentioned the usual "back and forth" between academic research and regulators. From the beginning, regulators have pushed ideas such as value-at-risk that from the start were highly criticized by scholars—based on economic theory (rational choice under uncertainty). Another example is RiskMetrics and exponential smoothing for estimating volatility—here too, academic research was highly critical of such an approach. Do you feel that sometimes, regulators pursue agendas that right from the start are known by the academic community to be fraught with problems?

CG: Regulators and specialized firms such as rating agencies, KMV, J.P. Morgan, etc., are continuously pushing ideas; some of them can be appropriate and are also useful for developing research; other ones can be misleading, even wrong, and the source of huge operational risks. Sometimes, the academic community could say at the beginning that some propositions are irrelevant; for instance, to propose a unique prediction formula for all stock return volatility, such as the exponential smoothing with fixed smoothing coefficient that you mentioned, to use the same abbreviation AAA for firm, country, and CDS ratings, whereas they have different meanings, to fix a level of risk, such as 5%, for computing the reserve

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independently of the environment and of the need of economic policy. In fact a lot of people, including often the regulators, confuse the notions of standardization and uniformity. Why not use as a first step exponential smoothing, but at least adjust the smoothing parameter according to the asset, to the period, etc. However, academic researchers also have to react with caution, otherwise they will fall into disrepute. The question of the VaR is a good example: This choice was done after several surveys and studies among practitioners showing that VaR was better understood than a variance-covariance matrix, for instance; it provides the complete information on the risk distribution if it is computed for each risk level; it makes professionals aware of extreme risks; the terminology is much more attractive than quantile, or percentile; it is known that in practice a VaR at risk level 5% corresponds to the value of an expected shortfall at another risk level. I know that VaR is not a coherent risk measure because it does not satisfy the axiom of subadditivity introduced in some theoretical academic papers. But before criticizing the VaR since some set of axioms are not satisfied, it would have been preferable to criticize the axioms themselves: The axiom of homotheticity, which assumes that the unitary price of a share does not depend on the sold volume, is not in line with what is observed in markets, the axiom of subadditivity, which implies an arbitrage opportunity and an interest in merging banks. Whereas the mathematical proofs are always politically neutral, this is not necessarily the case for mathematical assumptions. Clearly, the priority in this case is not to replace the VaR by the expected shortfall, but to teach people that an historical VaR differs from a conditional VaR, a realized volatility from a conditional volatility. They have to learn how to take into account the information, how to condition, how to change the measure along the cycles.

AM: It is a difficult problem. First, because the questions that regulators have to answer are difficult and controversial. For instance, there is no agreement in the academic world on important and basic topics like measure and aggregation of risks, allocation of global reserves to particular entities, distinction between systematic (or systemic) risk and unsystematic risk, etc. So it is not surprising that the state of the present relations between academics and regulators is not satisfactory. There is another difficult point, which could be more easily improved: the pedagogical problem. What I mean by that is simply that the documents produced by official regulatory agencies are often poorly presented, weakly justified, and extremely difficult to read. This obviously creates a barrier between the academic and regulatory worlds, which could have been avoided if these two worlds had worked together at an early stage, but I fear it is too late now.

Finally, Christian, you have recently taken a position at the University of Toronto. Can you tell us a bit about your experience teaching econometrics in Canada versus France? To what extent are the backgrounds, the expectations, and the studying behavior of the students different?

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CG: I am teaching at the Ph.D. level both in France and in Canada; the audiences are very different, but both interesting from a pedagogical point of view. In France, the audience is more homogenous, with a common high-level mathematical background; they are generally younger (22–24 years old) and often less autonomous. So we have to introduce formulas and math on our slides, to show that the professor has the appropriate mathematical level, where the aim is to teach them economic and financial applications. In Toronto, I try to advance progressively and at a more individualized speed their technical level according to the type of applied problem we are discussing. There is also more focus in Toronto on the statistical software and the practical implementation.

We thank you both for sharing many of your insights and thoughts with us.

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