Statistical Science
2011, Vol. 26, No. 4, 647–672
DOI: 10.1214/11-STS362
© Institute of Mathematical Statistics, 2011

A Conversation with Eugenio Regazzini

Antonio Lijoi[1] and Igor Prünster[1]

Abstract. Eugenio Regazzini was born on August 12, 1946 in Cremona (Italy), and took his degree in 1969 at the University "L. Bocconi" of Milano. He has held positions at the universities of Torino, Bologna and Milano, and at the University "L. Bocconi" as assistant professor and lecturer from 1974 to 1980, and then professor since 1980. He is currently professor in probability and mathematical statistics at the University of Pavia. In the periods 1989–2001 and 2006–2009 he was head of the Institute for Applications of Mathematics and Computer Science of the Italian National Research Council (C.N.R.) in Milano and head of the Department of Mathematics at the University of Pavia, respectively. For twelve years between 1989 and 2006, he served as a member of the Scientific Board of the Italian Mathematical Union (U.M.I.). In 2007, he was elected Fellow of the IMS and, in 2001, Fellow of the "Istituto Lombardo—Accademia di Scienze e Lettere." His research activity in probability and statistics has covered a wide spectrum of topics, including finitely additive probabilities, foundations of the Bayesian paradigm, exchangeability and partial exchangeability, distribution of functionals of random probability measures, stochastic integration, history of probability and statistics. Overall, he has been one of the most authoritative developers of de Finetti's legacy. In the last five years, he has extended his scientific interests to probabilistic methods in mathematical physics; in particular, he has studied the asymptotic behavior of the solutions of equations, which are of interest for the kinetic theory of gases. The present interview was taken in occasion of his 65th birthday.

Key words and phrases: Bayesian inference, Dirichlet process, exchangeability, de Finetti, finitely additive probabilities, History of Statistics and Probability in Italy, subjective probability.

1. PROBABILITY AND STATISTICS AT BOCCONI UNIVERSITY

Antonio: You received your degree in economics from "L. Bocconi" University in Milano. Why did you decide to study economics?

Eugenio: I enrolled in an economics degree essentially because it was the only choice I had. Having attended a Technical High School for Accountants, at the time the law did not allow students from this kind of secondary school to study mathematics at university, which would have been my favorite option. You needed to attend college preparatory schools to enroll in subjects like mathematics. My family could not afford my university studies and I was expected to get a job right after completing high school. The choice I made at the age of 14 was coherent with this scenario. By the time I got my diploma from high school, the situation had improved a little bit and I could afford going to university. This was also due to a financial aid program, adopted by the farseeing Italian center—left governments of the time, for students belonging to economically disadvan-

Antonio Lijoi is Associate Professor of Statistics at the Department of Economics and Quantitative Methods, University of Pavia, via San Felice 5, 27100 Pavia, Italy (e-mail: lijoi@unipv.it). Igor Prünster is Associate Professor of Statistics at the Department of Applied Mathematics and Statistics "Diego De Castro," University of Torino, c.so Unione Sovietica 218/bis, 10134 Torino, Italy (e-mail: igor@econ.unito.it).

[1]Also affiliated to Collegio Carlo Alberto, Moncalieri, Italy.

taged families. The money from this program (it was around 200 euros, per year, in 1965) and the earnings deriving from private lessons I used to teach to other students allowed me to obtain a degree at Bocconi University.

Igor: Why did you choose Bocconi University and not another university?

E: In the 1960s Bocconi was considered a prestigious university: a degree from it represented the key for obtaining a good and rewarding job on the market. These elements exerted an influence on me and my family. However, there was also a practical reason: Milano is just one hour by train from my hometown, Cremona, and I could go back home every weekend. Yet another reason is the influence of my friend Lorenzo Peccati, a mathematical economist from Cremona, who was a student at Bocconi while I was still at high school. He was well aware of my bent for mathematics and suggested a few advanced textbooks where I started reading the mathematical tools used in economic modeling. In particular, I was excited at reading the Italian translation of the monograph Allen (1956) on mathematical analysis for economists and this convinced me that Bocconi would still have allowed me to study Maths.

I: This is a funny coincidence since I was convinced by Lorenzo Peccati's son, Giovanni, by now a well-known probabilist, to enroll at Bocconi and for precisely the same reasons. Where did your passion for mathematics come from?

E: I was very lucky at high school because I had a brilliant maths teacher, Sidomo Vailati. He had a variety of scientific and cultural interests and also did consulting for a few private companies, thanks to his unusual, at least in that period, knowledge of statistics, probability and operations research. He certainly was a self-taught man in the area of Stochastics. At that time, probability and statistics, unlike analysis, geometry and algebra, were not perceived as relevant topics within mathematics degrees: they were only present in a few optional courses. To my knowledge, the only exception was the University of Roma due to the presence of Bruno de Finetti. In fact, this unfortunate situation lasted until the 1970s when the first full professors in probability, apart from de Finetti, were recruited after national competitions. Turning back to Vailati, it is worth recalling that, among some courses for high school teachers organized by the Italian Ministry of Education, he also took a course in probability, which was delivered by de Finetti. As a consequence, at the age of 16 I was introduced to the realm of subjectivism and learned the first elements of probability and its applications. These first years of exposure to de Finetti's approach have stimulated an intellectual and scientific interest that has certainly influenced my later research.

A: How was the environment at Bocconi University in the years you have been there?

E: Bocconi had very few professors among its own faculty and heavily relied upon adjunct faculty holding positions in other universities. These few professors were all influential personalities of the time, playing significant roles in the Italian social, political and economic life of the 1960s. For instance, Giovanni Demaria was a Paretian economist who acted as economic consultant for the constituent assembly that created the Constitution that lies at the foundation of the modern Italian Republic after World War II. There was also a special feeling between Bocconi and Milano, a city that had been able to overcome the disasters of World War II and was experiencing dramatic economic growth led by the manufacturing sector. Bocconi looked to me, and many others, as a vital part of Milano and contributed to consolidate this process. Then, during the last couple of years, the student protests of 1968 started. Despite being a private university, Bocconi experienced serious clashes and some of its students played an active role in the movement.

I: Did you like studying economics?

E: I was very fond of economics. The professors I was interacting with were quite enthusiastic about my inclination toward developing mathematical tools useful for economic modeling. There is an episode that occurred during my third year that I like to recall. I was attending a course in Political economy which included a series of seminars and one of them concerned the relationship between the Italian Central Bank and the Department of Treasury, which at that time was the subject of a lively debate. For an economic interpretation of the relationship between the two institutions, we were suggested to refer to an article by Giorgio La Malfa and Franco Modigliani; the latter was later awarded the Nobel prize in economics in 1985. The main contribution of the paper was the proposal of a static model. Playing a bit around with that model, I was able to derive a dynamic version of it, which seemed in line with the real situation in Italy. This was appreciated by the other students and the teaching assistants. Also in connection with the course on Public Finance, I devised a model describing the evolution of certain taxing decisions. Overall, I think I had quite a good economic intuition.

A: What did lead you to study probability and statistics?

E: I was both impressed and fascinated by the first year course in mathematics that was taught by Giovanni Ricci. It was more advanced than a traditional calculus course. The second year maths course, delivered by Giuseppe Avondo-Bodino, included also a part devoted to probability, which actually covered essentially the same material nowadays taught in first probability courses in maths degrees. During the second year of my degree I also attended a course on statistical Inference held by Francesco Brambilla, which was important for my education. Finally, my third maths course by Eugenio Levi contained some probabilistic applications. This experience revived my curiosity, dating back to high school, for foundational aspects of probability. Moreover, I perceived probability as a tough subject and therefore more challenging and stimulating than others I was studying.

I: What was the topic of your degree thesis?

E: I asked Avondo-Bodino to be my thesis supervisor. He was a passionate Fisherian and hostile toward the Bayesian paradigm: it might, thus, seem curious that the title of my thesis was "The Bayesian approach to hypothesis testing." Indeed, he chose that topic with the aim of proving the fallacy of the Bayesian paradigm: this is revealed by the fact that the potential of the Bayesian approach was going to be assessed with respect to hypothesis testing problems that had already received well-established answers within the frequentist framework. To be honest, he did not even like the Neyman–Pearson approach: according to him, it introduced subjective elements since it relied on decision theory. My task was essentially to: (i) collect as much material as possible on hypothesis testing, (ii) evaluate the possible impact of the Bayesian approach and (iii) establish whether it could be a sensibile alternative to the frequentist approach. And my supervisor obviously expected a negative answer to the last question.

A: And how did it work out? What were your first impressions on the Bayesian approach?

E: While working on the thesis, I developed some skepticism about the automatic implementation of Bayes' theorem, which was a legacy from Laplace and his followers. However, my viewpoint was limited. In fact, writing the thesis was not an easy job, especially because I could not rely on many systematic and exhaustive treatments. There were, of course, de Finetti's papers, but, given the unorthodox way they were written, I was not able to understand the connection between his theory and the Anglo-American neo-Bayesian approach typically adopted in papers appearing in statistics journals at that time. De Finetti's work did not follow the standard Bayes–Laplace paradigm: in contrast, he re-constructed it and recast it in a way to be coherent with his approach to prediction. The books I referred to were Lindley (1965), Raiffa and Schlaifer (1968) and, mostly, Ferguson (1967), which contained a beautiful part on the Bayesian approach from the viewpoint of Wald's decision theory. The 1959 lecture notes of de Finetti's course at a Summer School in Varenna [later translated in de Finetti (1972)] and Savage (1954) were also helpful. I obtained a few minor results in terms of interpretation and comparison and also derived a "rule" for the choice of type-I error probability α. As soon as I completed the thesis, the monograph DeGroot (1970) appeared: I found it very interesting and it proved to be very useful for my statistical education.

A: One of your best friends and main coauthors is certainly Donato Michele Cifarelli. Did you meet him while studying at Bocconi?

E: Yes, he is actually 10 years older than me and was my teaching assistant while I was attending the statistics course. He delivered insightful lectures where it was apparent that he had remarkable mathematical skills and also a deep knowledge of the book by E. Lehmann. Therefore, it was quite natural to seek his help when I started working on the thesis, which required me to study also the frequentist approaches. His advice was very important, although at the time he looked, at least to me, not interested into the frequentist versus Bayes debate. I was impressed by his vast knowledge of frequentist methods, both parametric and nonparametric, as well as of probability theory and stochastic processes. I very much liked the fact that he preferred fundamental, though maybe difficult, at least for us, books to much more immediate cookbooks. For instance, he knew in great detail the celebrated monograph by J. L. Doob on stochastic processes. We have been bound by a deep friendship and reciprocal esteem ever since.

I: Who were other important scholars you met at the time and who influenced your early approach to mathematics?

E: Overall Bocconi was an intriguing place, at least in Italy, for probability and statistics, given these subjects were, as I said, almost absent from most maths degrees. At the time Italian statistics, and academia in general, did not have systematic contacts with the international community and the head of the Institute of Statistics, Brambilla, had the merit of introducing and spreading the great developments in statistics and operations research, which took place in the UK and the US. He had contacts with foreign scholars: for example, he is also cited by Leonard J. Savage in his 1954 book. At the heart of these scientific activities was the Centre for Operations Research, which, among its sponsors, could count on Adriano Olivetti, an enlightened and revolutionary entrepreneur of the time, who ran the company producing the celebrated typewriter "Lettera 22" now displayed at the Museum of Modern Arts in New York City. His company is also well-known for its pioneering contributions to the development of personal computers. Among the various cultural and scientific activities created and supported by Adriano Olivetti, it is worth mentioning the journal "Tecnica e Organizzazione": Brambilla was a coeditor of the journal when de Finetti published an important paper on the essentials of computational techniques based on Monte Carlo methods, "Macchine che pensano e che fanno pensare" ("Machines that think and that make you think"). Brambilla was a remarkable figure: he had been the assistant of Ferruccio Parri who, besides being one of the first Italian Prime Ministers after the war, was also imprisoned by the Germans during World War II since he had been one of the antifascist opposition leaders.

A: What happened after you graduated?

E: I really enjoyed working on my thesis and I was eager to continue, at least for some time, with research. Ph.D. programs did not exist in Italy since they were only introduced in the mid-1980s. So I was doomed to the military service which was compulsory and would have lasted for 15 months. I tried to postpone my entry for as long as possible, since I wanted to compete for a scholarship from the Italian Ministry of University. Had I obtained it, I could have freezed it until the end of the military service. Thankfully my strategy was successful and, when I was discharged in January 1972, I was able to go back to university. After graduation and before starting the military service, I shared the office at Bocconi with Cifarelli, and together we attended various maths courses at the State University in Milano. I then sat the exams during my military service, but in the end I did not complete a maths degree, since I was already involved in developing my own research and I was willing to publish! Nonetheless, those studies turned out to be very useful for me.

2. FROM TORINO TO BOLOGNA, MILANO AND PAVIA

I: Unlike many Italian academics, and more in line with what happens abroad, you have been working in many different universities. Was this important for your professional development?

E: Definitely. In addition to working in various universities, I also experienced very different environments. It has been very helpful from both a scientific and personal point of view. I met many statisticians and mathematicians with very different backgrounds. My first experience outside Bocconi was in Torino: it was a small Department, most colleagues were of my age and so it was pretty easy to settle in. Afterward I moved to Bologna in a much larger Department more in line with the Italian statistics tradition. Then I got back to Milano: first at the Mathematics Department of the State University and, then, to Bocconi. Finally Pavia, which is one of the oldest universities founded in 1361 and in a very prestigious Mathematics Department of which I am a proud member.

A: Tell us a bit about your first steps in the Italian academia in Torino.

E: My supervisor, Avondo-Bodino, was full professor in Torino and a lecturer at his department resigned and decided to leave the academia. Since they needed a replacement, in December 1973 I moved there with the concrete opportunity of obtaining, later, a permanent position. Due to absurd bureaucratic reasons, I finally obtained an Assistant Professorship only in 1978.

I: You obtained a full professorship position in a national competition at a young age in 1980 and, therefore, moved to Bologna which hosted one of the few faculties of statistics in Italy. Then back to Milano.

E: That was a time Italy was investing in universities, unlike now. Therefore, the career perspectives were quite good also in the academia if you worked hard. Incidentally, the head of the selection committee was de Finetti, I must say a recurrent figure in my life. In 1980 the only autonomous statistics faculties were Roma and Padova, whereas Bologna and Palermo were offering statistics degrees but within economics faculties. Statistics became a faculty in Bologna only toward the end of the 1980s. In fact, and in contrast to what happens outside Italy, faculties are pivotal players in Italian academia, mainly because they manage the recruitment. Now it seems that things will change but, as we say in Italy, everything changes so that nothing changes. Anyhow, in Bologna I mostly taught probability courses, but my ties to Milano were still strong, especially because of my collaboration with Cifarelli. Therefore, I accepted the offer from the Mathematics Department of the University of Milano in 1984, where they did not have a faculty member doing research in probability and mathematical statistics until my arrival.

A key role for my transfer was played by an analyst, Marco Cugiani, whom I also replaced as director of a research institute of the C.N.R., nowadays the Milano branch of the Institute of Applied Mathematics and Information Technology.

A: In 1989 you moved back to your beloved Bocconi University. Did you find any substantial changes since the last time you had been there?

E: At Bocconi things had changed a lot, the most apparent being that it had turned from an elite to a larger and more open university with something like 10,000 students. Hence, I think that some changes were necessary. As for myself, I was in a somehow privileged position since I was mostly teaching advanced and not compulsory courses, which were much more challenging than most other courses. Therefore, starting from a yearly basin of more than 2,000 students of very good quality, by self-selection I had small numbers of students, who were highly motivated and of the highest quality. I guess I have to mention you two, will not I? But let me also mention Chiara Sabatti and Giovanni Peccati, among many others.

I: What convinced you to move to another university in 1998?

E: During the 1990s, while I was there, an even more radical reorganization was occurring: courses of the type I was teaching were perceived as too "aristocratic" and had too few students so that they were doomed to be shut down. And the same destiny was foreseen for the most challenging degrees. In fact, when I moved back in 1989, I did it with the aim of setting up a statistics degree: I was very disappointed when the project was officially turned down in 1997. Therefore, I decided that my experience at Bocconi was concluded and that I wanted to move back to a maths department. Nonetheless, I still have a special affective relation with Bocconi.

I: Then you moved to Pavia. And I followed you, given I had just graduated from Bocconi and started my Ph.D. in Pavia. It was a new challenge for you at a mature age, was it not?

E: The only science faculty members and future colleagues of mine in Pavia I knew in person, prior to moving, were three brilliant mathematicians: Maurizio Cornalba (we were both members of the scientific committee of the Italian Mathematical Union), Franco Brezzi (we got to know each other at the meetings of the Italian National Research Council) and Enrico Magenes. Enrico Magenes, who passed away last November, shaped the department in its current form and significantly contributed to its international standing. When I moved, I was one of only two probabilists and since then we have been able to hire two additional Assistant Professors. Once arrived, with great enthusiasm I immediately got involved in the Ph.D. program and the outcome has been rewarding, as also witnessed by the achievements of some former Ph.D. students.

A: Throughout your career you have been working at faculties of science and economics, at public and private universities. What environment do you think better fits the needs of a researcher in probability and statistical science?

E: I think that the ideal environment, relatively to the Italian experience which is the only one I am aware of, is a maths department which is open toward the modern trends and research directions of mathematics and therefore not too much bound to traditional subjects like analysis, geometry and mathematical physics. And thankfully there are various departments that comply with these criteria. However, I must admit I am a bit pessimistic about the future: the Italian university system in general, and departments involved in basic research in particular, are struggling and suffering due to the indiscriminate financial cuts in recent years. These have been implemented somehow light-heartedly by the government since cuts in basic research funding are unlikely, at least in Italy, to cause immediate social upheaval. In fact, it would be important to abandon the habit of uniformly distributed cuts and aim at creating, or consolidating, niches of excellence. And I have seen many such niches during my career.

3. BAYESIAN INFERENCE

I: At the beginning of your academic career you started working on inferential problems according to the frequentist approach.

E: My interest in frequentist inference started soon after completing my degree thesis and heavily benefited from the collaboration with Cifarelli. And one of the first topics we started working on was hypothesis testing. Corrado Gini and other Italian statisticians had introduced a considerable number of summary statistics that were originally used only for exploratory data analysis to measure, for instance, concentration, variability, dependence and similarity between sets of data, and so on. Our idea was to use such summary measures for inferential purposes and, specifically, as test statistics for studying dependence in nonparametric problems. An early contribution in this direction was achieved by Cifarelli (1975) who studied the asymptotic distribution of a statistic arising in a test of homogeneity for two-sample problems. The paper contained a remarkable result on the distribution of the integral of the absolute value of the Brownian bridge. Our initial efforts led to a paper (Cifarelli and Regazzini, 1974) that we are very proud of: there we determine the limiting distribution of a measure of monotone dependence introduced by Gini. The program we set was very appealing and consisted in checking whether these statistics, when used for hypothesis testing, yielded tests that were more efficient than those commonly used at the time. For example, the index of monotone dependence I was mentioning was compared with Spearman's ρ and with Kendall's τ and in some cases it featured better performances.

A: Around the mid-1970s you turned back to Bayesianism. What about your skepticism?

E: Yes, and my experience at University of Torino was fundamental in this respect. The department library held the collection of all de Finetti's papers, well kept and easily accessible. I started looking at contributions cited by Savage as decisive for the foundations of the Bayesian paradigm. My curiosity was fueled by the fact that, as I said, de Finetti's work appeared to me completely disconnected from the Bayesianism I had studied on books and journal articles. It was a challenging task since de Finetti's writing style, which was actually one of the main aspects he was criticized for, was unorthodox and sometimes seemingly cryptic. Nonetheless, hard work and stubbornness finally allowed me to understand why de Finetti introduced exchangeability and the role such a form of symmetry plays in the reconstruction he gave of the Bayes–Laplace paradigm. This really opened my eyes on a new world providing a coherent and unified view of statistical inference, where subjective probabilities play an important role. In fact, suddenly the subjective interpretation of probability was the only one that made sense to me from both a philosophical and a mathematical point of view.

I: You were then able to convince Cifarelli to enter the realm of Bayesian statistics.

E: I have to say that Cifarelli shared my same doubts on the foundations of the Bayesian approach to statistical inference. However, after completing my study program in Torino I pointed him to the references where de Finetti was answering our questions and solving our doubts. Besides de Finetti's well-known papers, I had discovered many other "minor" contributions that were important for understanding the unified framework he had in mind. And, after struggling to understand, I started to love his style: entering his world had been very demanding, but once I succeeded the reward was incomparable. In his work one could find ideas, hints and concepts whose expressive force was much more powerful than a standard presentation of definitions, theorems and cool mathematical technicalities. Spurred by the enthusiasm, I had been able to convince Cifarelli and we started working together in this direction.

I: Is this when you started your research on Bayesian nonparametrics?

E: In some sense, yes. On the one hand, we were hoping to be able to tackle in a Bayesian setting the same issues we had addressed within classical nonparametric inference. On the other hand, we guessed that our starting point should have been de Finetti's representation theorem as stated in de Finetti (1937a) which we could consider as being nonparametric. In this fundamental paper, the law of an exchangeable sequence is described as a mixture on a space of probability measures and the prior is the almost sure limit, in a weak sense, of the empirical measure generated by the data. This motivated the investigation of random probability measures (rpm's) for statistical inference and might have led to extend the Bayes–Laplace paradigm. We planned to consider estimation of functionals of rpm's such as the mean, the variance or other characteristic parameters of the unknown distribution. The necessary preliminary step was to determine the posterior distribution of these functionals. A helpful reference was a short paper (de Finetti, 1935), where de Finetti provides a reformulation in Bayesian terms of methods used in exploratory data analysis for smoothing the empirical distribution. Moving from this, he basically addressed in a nonparametric framework both the issue of prediction and of evaluation of the posterior distribution on a set of probability measures. Unfortunately, we had no clue on how to define a probability distribution on a space of probability measures that would be analytically tractable. Of course, we were not aware of T. S. Ferguson's paper (Ferguson, 1973). We were stuck and all the attempts we made led us nowhere.

A: Was there any decisive event that helped you overcoming these difficulties?

E: In 1976 I met Andrew L. Rukhin who had left the Soviet Union and was in Italy just before migrating to the US. We discussed our research activities and I described to him the technical problems Cifarelli and I were dealing with. He suggested we go through Ferguson's paper in order to find an answer to our questions. And, indeed, that was the case: that paper allowed us to resume our project. So we started considering linear functionals of the Dirichlet process with the aim of determining their probability distributions analytically.

A and I: Let us also recall that the study of the Dirichlet process suited your passion for classical music very well!

E: Gustav Dirichlet is associated with the distribution because he evaluated the integral on the simplex. The musical connection is that he married Rebecka Henriette Mendelssohn, younger sister of Felix Mendelssohn, the famous German composer.

I: Were there other Bayesians in Italy at the time?

E: A few years after its re-flourishing at an international level, due to the work of Leonard J. Savage, the Bayesian approach was sort of rediscovered in Italy as well. This may sound surprising given de Finetti is Italian: however, one has to consider that de Finetti only entered academia in 1946, at the age of 40, when his research was already focused on different topics. Interestingly, he had obtained the position already in 1939, but could only start his job in 1946 after the fall of the fascist regime due to a law forbidding the appointment of unmarried professors, as was de Finetti's case. Anyhow, in those years there was a large group led by Giuseppe Pompilj in Roma and some scholars started to work on Bayesian statistics, like Ludovico Piccinato. In Roma there were also some of de Finetti's students like, for instance, Fabio Spizzichino. I should also mention a group based in Trieste and coordinated by Luciano Daboni, who started working under de Finetti's supervision soon after gaining his university degree. Besides actuarial mathematics, they focused mainly on exchangeable processes and foundational issues of Bayesian inference and, during the years, I had many fruitful interactions with them.

A: Even if more interested in the Bayesian paradigm, you did not avoid doing research based on a frequentist approach. It seems you did not, and still do not, see any ideological contraposition between Bayesianism and frequentism.

E: I have never seen this as an ideological contraposition. I think that ideological positions make sense only outside the realm of mathematics. Anyhow, even when I was working on statistical problems according to the frequentist approach, I always had the feeling that the Bayesian framework was far more complete and logically sound. I was not enthusiastic about the automatic use of priors on unobservable parameters: the subjective views I had on probability were in conflict with such a treatment of the Bayes–Laplace paradigm, as I believe that inference must concern quantities that can be empirically observed. But, on the other hand, the Fisherian attitude appeared to me as too drastic, because prior beliefs should play a role in statistical inference. Once able to fully understand the consequences of de Finetti's results, I became convinced that Bayesianism was the only acceptable way of inductive reasoning.

A: Current developments in Bayesian inference involve a heavy use of simulation algorithms. Do you still think there is a need for putting a strong effort in determining exact forms of Bayesian inferences (or, at least, error evaluation when approximations are used), even when these are difficult to use in practice?

E: Computational techniques have been decisive in making Bayesian models applicable to real world problems and some recent applications I saw are simply amazing. I definitely think that the advantages they yield largely surpass some drawbacks associated with their uses. That said, I would still like to make a point, which I think is important since it has to do with how statistical modeling is conceived. Indeed, models should be devised as simple as possible, while still preserving the capability of capturing the essential features of the phenomenon under study. Such a simplification could be achieved by first detecting inessential elements and, then, dropping them when it comes to the point of specifying the model. This attitude is natural when one aims at achieving exact estimates of the quantities of interest. However, if the need for pushing analytic results as far as possible disappears, it is likely that the models become more loose and unnecessarily complex. Both parsimony and extreme care in the formalization of models are still important guidelines for research: the only difference is that they now need to be spelled out clearly, while they were implicitly followed in the past. Another related and important point concerns approximation. When exact inferences are not possible, one should put some effort in providing an upper bound to the error of approximation yielded by the numerical techniques that are used. I have tried myself to work in this direction, for instance, in relation to approximating the probability distribution of the mean of a Dirichlet process. I know this is a challenging task, but it cannot be avoided.

4. DE FINETTI AND THE INFLUENCE OF DE FINETTI'S WORK

I: There is no doubt your research has been deeply influenced by de Finetti's work. Which was the first paper of de Finetti you read through?

E: While I was completing my thesis at Bocconi I came across his joint paper with Savage (de Finetti and Savage, 1962). It contained a discussion on the choice of the prior distribution and was mainly illustrative with no deep mathematics involved but still evocative for a novice.

A: His most renowned piece of work certainly is the two-volume book on probability theory, de Finetti (1970). What else would you suggest to a student who is willing to study and understand de Finetti's stance in probability and statistics?

E: I would certainly suggest de Finetti (2006), two volumes containing selected papers by de Finetti, which have been published by the Italian Mathematical Union in 2006 in occasion of the centenary of his birth. The first volume is on probability and statistics, whereas the second is on applied maths and on the teaching of maths. As for his subjective views on probability, one should refer to de Finetti (1931). One should also read de Finetti (1937a). Another important piece of work is de Finetti (1972). Finally, de Finetti (1992) contains a selection of some of de Finetti's papers with English translation. Unfortunately, some significant contributions, at least to my knowledge, have been only published in Italian, such as those related to independent increments processes and some others on the subjectivistic definition and interpretation of probability.

I: As you just mentioned, the fact that he was not writing in English hindered the circulation of his ideas and results in the scientific community.

E: This is definitely true. For example, it is probably unknown to many that de Finetti introduced the celebrated τ index a few years before Kendall (de Finetti, 1937b). In 1939 he obtained some important results on optional stopping: indeed, de Finetti (1939) deals with the gambler's ruin problem, where one can also find an embryonic version of the Girsanov theorem. Another important contribution was the continuity theorem for characteristic functions: he proved it in the appendix of de Finetti (1930a). Besides these, it is worth listing a few other contributions for which a priority to de Finetti should be acknowledged: he completed what is now known as the Glivenko–Cantelli theorem before Francesco P. Cantelli in de Finetti (1933); in de Finetti (1940) he devised a model that anticipated the portfolio theory for which Markowitz was awarded the Nobel prize; he proved the theorem on almost everywhere nondifferentiability of the trajectories of the Brownian motion in de Finetti (1929).

A: With reference to the de Finetti (1929) paper, which is actually our favorite, should we not as Italians propose *Lévy processes* be called *de Finetti–Lévy processes* instead?

E: As I mentioned before, the answer is affirmative. Indeed, de Finetti started from a more general problem of providing the random counterparts of a Volterra classification for the ordinary laws of physics. In this context he identified processes with independent and homogeneous increments as those whose characteristic function satisfies the first of the equations in Volterra's classification, namely, $X' = f(\lambda)$. As a by-product, he also introduced implicitly the notion of infinite divisibility. In a subsequent paper, de Finetti (1930b), he further characterized the class of infinitely divisible laws as the class of distribution limits of compound Poisson processes, thus providing a representation theorem for infinitely divisible distributions. Lévy was not aware of de Finetti (1929) and resorted to a different approach to obtain more general and deep results. The contribution by Khintchine to the well-known Lévy–Khintchine representation originates from a paper published in 1937 (see Khintchine, 1937): Khintchine's paper builds upon Kolmogorov (1932), where Kolmogorov explicitly mentioned (even in the title of the article) that he was resorting to the approach set forth by de Finetti. So, yes, it should definitely be *de Finetti–Lévy processes*.

I: And what were his connections with the broader international scientific community?

E: His first international contacts, before graduating in mathematics at the University of Milano, are related to a paper, de Finetti (1926), he wrote on Mendelian inheritance, which had quite an impact in biology. It was his first paper and appeared on the Italian journal *Metron*. His results also attracted the attention of Alfred J. Lotka and Jacques S. Hadamard. The latter was so impressed by de Finetti's achievements that he suggested Georges Darmois to study the paper, as witnessed by one of the letters that de Finetti wrote to his mother in 1929 and that have recently been published by his daughter Fulvia. This research also originated the so-called de Finetti diagrams that are extensively used in population genetics.

A: An important event at which de Finetti drew attention on his research in probability was the International Congress of Mathematicians (ICM), which was held in Bologna in 1928.

E: That conference was definitely important for the development of de Finetti's interactions with foreign scholars. On that occasion he presented his first results on exchangeability and made contact with Maurice R. Fréchet, who later invited him to the Institut Henri Poincaré in 1935 and to the Colloque de Génève in 1937 where he then also met Jerzy Neyman and others. He had frequent interactions with Paul Lévy and Aleksandr Khintchine, respectively, on independent increment processes and on the proof of the representation theorem for exchangeable sequences. He was also in contact with Andrey N. Kolmogorov, as witnessed by the Kolmogorov (1932) paper on infinite divisibility whose title contains an explicit reference to "A problem of de Finetti." Both Kolmogorov and de Finetti also worked at the same time on the derivation of a representation theorem for associative means, now known as the de Finetti–Kolmogorov–Nagumo Theorem. He also got in contact with many eminent mathematicians via mail. In fact, he used to have a notebook in which he recorded to whom he had sent which of his papers: de Finetti's daughter, Fulvia, once showed it to me and the names are impressive. After World War II he had significant scientific collaborations with Leonard J. Savage and Lester Dubins and he interacted also with William Feller and Abraham Wald.

I: Were his views on the subjective approach to probability theory held in high regard?

E: In mathematics his work has been largely ignored, and not only because of the subjective interpretation. Indeed, the mathematical approach yielded by such interpretation does not require σ-additivity. In fact, finitely additive laws also become admissible and the traditional measure–theoretic approach to probability theory represents obviously a particular case. Countably additive probabilities are coherent in de Finetti's sense but are just a subclass of coherent laws. And de Finetti himself was well aware that many results could have been neater by assuming countable additivity. We may reasonably conjecture that his position in favor of including finitely additive probabilities somehow put him off from focusing on the particular countably additive setup. This could explain, for example, why he did not further investigate processes with independent increments. It is to be noted that the framework for his subjective approach had been settled by 1931 and, as evident from his published mail exchange with M. Fréchet, he fought for it for a while.

I: And what about the impact on statistical practice?

E: In Bayesian statistics references to subjectivism are quite frequent, but I actually see little of de Finetti behind them. First, in the subjective approach also finitely additive laws are allowed and, therefore, a proper subjectivist should try to analyze statistical problems in this setup. This point is very important in the case where "transcendent" conditions—such as convergence of sequences of random elements, forms of the corresponding limits, etc.—are involved: one should, then, establish the extent to which the conclusions depend on the specific σ-additive extension (usually unique) of the original finite-dimensional distributions. Second, from an interpretation point of view, subjectivism and objectivism are often mixed up and Bayes theorem is applied in an automatic way, whereas subjectivism would require probabilistic statements to be made on verifiable events. Subjectivism seems more a kind of catch-phrase than a real commitment. In my opinion, the papers of L. J. Savage, L. Dubins, J. Pitman, P. Diaconis and D. Freedman are the ones that adhere most closely to de Finetti's views.

A: Did your convinced support of subjective probability affect the way you teach probability courses?

E: This represented a sort of dilemma throughout my career. Focusing solely on de Finetti's mathematical theory of probability would have implied providing students with an unorthodox background in probability: it could have been an enrichment for some of them but also a drawback for some others, especially for those who needed to use probability as a mere tool in other disciplines. Therefore, most of the courses I taught were within the σ-additivity framework. Nonetheless, I have always tried to illustrate extensively some distinctive features of the subjective viewpoint in one of my first lectures. This was useful since it provided students with a more complete picture of the subject and allowed them to understand that the results I was going to state and prove were valid on a special class of probabilities sharing the property of countable additivity. Students were, then, aware that it was somehow like teaching them a course in analysis that was just about analytic functions! The connection with conditional properties was far more difficult to point out. As for the subjective interpretation, it is still possible to preserve it even when confining to σ-additive probabilities.

A: Can you provide some further insight on this last issue?

E: The difficulty I am referring to arises due to the fact that Kolmogorov's definition cannot be seen as a special case of coherent conditional probabilities. In fact, the Kolmogorov approach lacks an appropriate axiomatization and interpretation of conditional probability: the definition is by means of a limiting procedure. The perspective is then completely different. For example, de Finetti's approach necessarily leads to conditional probabilities that are regular and proper, whereas it is well known that Kolmogorov's definition does not. In order to grasp these mathematical and conceptual differences on conditional expectations and probabilities, one can refer to the works by L. Dubins, David Blackwell, Czeslaw Ryll-Nardzewski, William D. Sudderth, Roger A. Purves, Pietro Rigo, Patrizia Berti and also myself.

I: Tell us about your meetings with de Finetti.

E: I first met him in 1969 at a summer course on mathematical economics in Urbino. Since I was working on my thesis, I took the opportunity to ask him a few questions about his paper with Savage (de Finetti and Savage, 1962) I had read. He was not very talkative and probably thought I was not understanding anything. He was right, but I still went away with the impression that it was not simple at all to interact with him. Afterward I met him at some conferences during the 1970s, but at that time he was not working on statistics and probability with the same intensity and creativity of the early days: he was more inclined to elaborate on general philosophical and foundational aspects. The only thing I can say about our meetings is that I had the impression he was interested in nontrivial and original approaches or attitudes that to some other people might have appeared as singularities. For example, in Bologna he once told me he had been fascinated by the mathematical physics lectures held at the Polytechnic in Milano by a lecturer, Bruno Finzi, whose assignments were notoriously challenging and contained exercises that Finzi himself could not solve. He recalled the solutions he had been able to give were very original and much appreciated by Finzi. He also told me he had appreciated lectures on economics of insurance companies delivered by Ulisse Gobbi at the Polytechnic in Milano because they had been the source of inspiration for the mathematical modeling of many aspects of economics he had later investigated. I am surprised by this, since in Gobbi's work I did not find any mathematical formalism.

A: He was also engaged in public life and gained some popularity because of his political experience.

E: His political experiences can be well understood if one refers to the environment where he grew up. De Finetti's family was wealthy and highly educated. They were part of the Italian community in territories of the Habsburg Empire, and his father was an engineer working for the Austro-Hungarian railway. During his childhood he had learned about the irredentist ideas of the Italian minority that was aiming at unification with Italy. Such aspirations quite naturally developed into strong nationalist feelings once the area became part of Italy. Moreover, having been part of a minority, he developed a strong sensitivity toward injustice in all respects and, therefore, also a strong criticism toward some social implications of capitalism of the time. This blend of ideas somehow naturally led him to support the rising fascist party: its initial political and social program included a series of reforms whose goal was the complete State control of the economy. As de Finetti himself wrote a few years before dying, the direction of the whole economy, once freed from the terrible tangle of individual and interest group selfishness, should lean toward the collective achievement of a Paretian "optimum" and should be further inspired by "fairness" criteria.

A: Hence, his support to fascism was mainly the result of ideal feelings that were fueled by strong social and economic views.

E: This is further witnessed by the fact that after the fall of fascism, he sympathized with left-wing movements without adhering to a large political party. Finally, during the 1970s he started being involved in important campaigns for civil rights and for social justice. The Italian party that better fitted his political thoughts of the time was the Radical party.

I: Can you tell us something about it? It seems that, while being involved in political activities set forth by the Radical Party, he spent one night in jail!

E: In fact, he did not end up in jail because the order to release him arrived before being imprisoned. To make a long story short, he was editor of a newspaper of the Radical Party, which was publishing letters of conscientious objectors who refused to perform the compulsory military service. This was illegal at the time. The day he learned he was going to be arrested, he asked the police whether it was possible to arrest him at the *Accademia dei Lincei*, the most prestigious Italian science academy, where he was going to have an official meeting the day after. He motivated such a seemingly bizarre request with the fact that the police could have saved some money by not picking him up by car at home: the *Accademia dei Lincei* building was, indeed, just a few steps away from the prison he was supposed to go to. However, the order to release him arrived as soon as he got to jail. This episode had a huge echo in the press.

A: We also recall a story you told us about Kolmogorov visiting Roma and wanting to meet de Finetti.

E: In 1962 Kolmogorov was awarded the Balzan prize for Mathematics, the other awardees being Pope Giovanni XXIII for Peace, Paul Hindemith for Arts, Samuel E. Morison for Humanities and Karl von Frisch for Biology. Two well-known mathematicians, Gaetano Fichera and Olga A. Oleinik, went to collect him at the Roma airport and asked him what they could do for him. And, as Fichera reported, his answer was, "If you know him, then you should organize a meeting with de Finetti."

A and I: De Finetti's papers are scattered with brilliant ideas, sometimes only sketched. What are the aspects of de Finetti's work which still need to be developed?

E: As for some specific topics, such as exchangeability and processes with independent increments, in my opinion most of his ideas have already been extensively developed and not much is left to investigate in the precise direction he had originally thought of. On the other hand, I believe that much is still left to investigate on the general foundations of probability theory that emerge from his work and that he strongly supported. These studies might have a relevant impact in statistics, in physics and in other research areas. The advances I am thinking of concern both the interpretation of probability and the enlargement—along with its mathematical implications—of the class of admissible probability laws to include also the finitely additive ones.

5. PROBABILITY AND STATISTICS IN ITALY

A: You investigated quite extensively the development of statistics and probability in Italy in the first half of the 20th century (e.g., Regazzini, 2005). Can you tell us about it?

E: In contrast to what happened in the Anglo–American world or in Russia, in Italy probability and statistics developed along almost separate paths. Probability started growing in mathematical environments. As far as I know, the first to deal with the topic in a comprehensive way was Guido Castelnuovo, a famous mathematician who was mainly doing research in algebra and geometry. His 1919 book on probability (Castelnuovo, 1919) was used as a textbook for quite some time in those few mathematics degrees where probability was taught. The interpretation of probability was frequentist, in line with a view that would have been later shared also by Fréchet, Lévy and Kolmogorov, and covered results of the Russian school up to Andrey Markov and Aleksandr M. Lyapunov. Already, back in 1915 he had the idea of setting up a school of statistics and actuarial sciences at the University of Roma, which was then created in 1927. It had considerable success with many enrolled foreign students and then became a proper faculty in 1936 with Gini. In the preparation of his book Castelnuovo was helped by Cantelli, who is considered, also at an international level, one of the first modern probabilists. He derived, among other contributions, versions of the laws of large numbers, the Borel–Cantelli lemma, a mathematical theory of risk that was named after him, and developed an autonomous abstract measure–theoretic theory of probability, which appeared before Kolmogorov's. It is interesting to recall that in this last development a crucial point was the proof of the existence of measurable maps defined on [0, 1], endowed with the uniform distribution, in such a way they have prescribed probability laws: such an approach also reflects the idea of adhering to the classical definition of probability due to Laplace. Anyhow, this problem led him and his students to anticipate at least part of what is nowadays known as the Skorokhod representation. A distinguished scholar who obtained important results along the lines of research undertaken by Cantelli was Giuseppe Ottaviani, who is also known for his inequalities that are related to Cantelli's theory of risk. Francesco G. Tricomi, eminent analyst and friend of Cantelli, also gave some contributions to probability as did Carlo E. Bonferroni, who is well known for his inequalities.

A: Given such a glorious tradition, it is quite surprising, as you said earlier, that the first full professors in probability were appointed by Italian universities only in the 1970s, with the notable exception of de Finetti.

E: Actually, at the beginning of the 1970s only two professors in probability were recruited, namely, Giorgio Dall'Aglio and Giorgio Letta. Dall'Aglio was at the Faculty of Statistics in Rome and was a member of the before mentioned group led by Pompilj. Letta is from Pisa and spent several research periods in Germany and France. The latter experience stimulated collaborations between Italian probabilists—some of whom were Letta's students—and French probabilists in Paris and Strasbourg, a fruitful trend which is still ongoing. Then a larger group of people were appointed at the end of the 1970s in various Italian universities.

I: And what about statistics?

E: In the last three decades of the 19th century, topics that are today ascribed to Mathematical Statistics were taught in geodesy or astronomy courses.

Lectures by a not well-known Italian mathematician, Paolo Pizzetti, were very interesting and contained some innovative ideas on significance tests. More conventional, at least according to the Italian framework, statistics courses were in law faculties: many academic statisticians actually had a degree in law. Most of them were involved in Official statistics and it was therefore natural that the interactions between statisticians and probabilists were rather limited. The first modern Italian statistician was Rodolfo Benini, who had a law degree from the University of Pavia and developed statistical methods for demographic, sociological and economic problems around the end of the 19th and the beginning of the 20th century. I recall once I came across historical documents presented in noteworthy conferences of the American and British Economic Societies where Benini is referred to as one of the founders of econometrics. I think this due to his analysis of income and wealth distributions and to the pioneering use of multiple regression methods to estimate, for example, demand curves. He also had the idea of studying contingency tables with fixed marginals. Among his successors, the main figure is certainly Gini, also a graduate in law. His methodological contributions to statistics were praiseworthy and were later studied not only in relation to mere data analysis. Gini dominated Italian statistics until his death in 1965 and created a school of faithful followers. A prominent group of scholars was led by Pompilj at the Faculty of Statistics in Roma. As I recalled earlier, Dall'Aglio was one of its members and he obtained noteworthy mathematical results that can be traced back to the Ginian analysis of statistical relationships. His results, however, have a remarkable independent interest: for example, he provided a relevant contribution to the definition and to the properties of what is today known as the Wasserstein distance. See Dall'Aglio (1956).

A: You mentioned Paolo Pizzetti who seems to be a neglected figure within the Italian statistics community, was he not? We have never heard of him in our statistics courses.

E: Yes, he was unfairly neglected. His contributions, which appeared in the 1880's, were very innovative and relied on an original approach that somehow anticipated a few distinguishing ideas lying at the foundations of statistics as set forth by Karl Pearson and by Ronald A. Fisher. As an example, he proposed procedures that were very similar to the significance tests Fisher would have later adopted as a distinctive feature of his methods. Pizzetti also had remarkable mathematical skills that allowed him to determine the exact distribution of certain statistics used for data analysis. And he was well aware of the results achieved, in this direction, by a German geodesist Friedrich R. Helmert. He reproved Helmert's results with the aim of extending them and relied on innovative methods and techniques that Fisher himself would have later proposed independently. This is very well documented in a recent historical monograph by Anders Hald. As you can easily guess, Pizzetti's ideas were totally different from those that Gini would have later expressed apropos of the Fisherian tests. Indeed, Gini was very critical about the use of significance tests and his criticisms were shared by de Finetti. This may partly explain why Pizzetti is not known by many statisticians. His work was somehow considered as heterodox for quite some time, as demonstrated by the 1960s reprint of Pizzetti's 1892 book (Pizzetti, 1963). In the preface, written by V. Castellano, P. Fortunati and G. Pompilj, it is claimed that parts of Pizzetti's work were "misleading and... contained errors that had been masterfully pointed out by Gini in Gini (1939)." And the "misleading parts" they were referring to are exactly those where Pizzetti uses his results for devising statistical tests.

A and I: The excerpt you read can partly explain the isolation of the Italian statistics community in those years.

E: It partially does. Indeed, I think that Gini's critical remarks make sense. The point is that they were not complemented by alternative proposals that could take his concerns into account. Hence, it was almost inevitable that Gini's position would have become marginal and isolated within the broader international community. It should be recalled that isolation fitted very well with the political climate favoring autarkic tendencies during the fascist regime and it unfortunately further consolidated over the years in the Italian statistics community, at least in academia. This obviously had a long-lasting negative impact, from which Italian statistics started recovering only in the 1970s.

A: Gini was appreciated both for his scientific achievements and for his praiseworthy services as a scientific expert within various important Italian institutions.

E: Definitely. He was founding President of the Italian Central Institute of Statistics in 1926 and set up first the School and then the faculty of statistics, demographic and actuarial sciences in Roma in 1936. He was in constant contact, also meeting him in person, with Mussolini, who used to pay attention to statistical analyses for taking decisions on policy issues. For example, he acted as a technical advisor within the programs of demographic and eugenics policies pursued by the fascist regime. Later he also founded the Italian Statistical Society, of which he has been President for 20 years. Besides the scientific and institutional authoritativeness he gained in Italy, it should be recalled that he obtained countless recognitions abroad as well. Among them I could mention that he became Honorary Fellow of the Royal Statistical Society, Vice President of the International Sociological Institute, and Honorary Member of the International Statistical Institute. In 1920 he was the founding Editor of the journal *Metron*, which published papers by many eminent statisticians of the time, such as R. A. Fisher, A. A. Chuprov, A. J. Lotka, S. S. Wilks, E. E. Slutsky, S. Kullback, H. Wold and A. L. Bowley.

I: We have also heard of some funny stories about Gini bearing ill-luck. Can you tell us something more?

E: Yes, this is somehow true, but it is to be considered within the typical Italian attitude of making fun of powerful people, as Gini certainly was. There are various minor anecdotes and a dramatic episode that would allow to conjecture a "correlation" of the type you are referring to. As for the latter, something incredible happened in 1927: he was on the steamboat "Princess Mafalda," which shipwrecked off the Brazilian coast between Salvador de Bahia and Rio de Janeiro, and he was among the few survivors, the "legend" says thanks to his rowing skills, a sport he had practiced in youth. A less dramatic and funnier story I have heard of concerns an episode where, chatting with a colleague of his, he paid a compliment to a young female student's legs whom they met on the stairs: after a few steps she fell down and broke her leg. I remember that Ottaviani did not mention his name, he referred to him as *the unnamed*, since mentioning his name could have led to something bad happening. All kidding aside, after the shipwreck in Brazil, he criticized the Italian authorities for the poor assistance from the Italian Navy and, more in general, from the Italian government. These complaints caused him a lot of troubles with the fascist regime in Italy. He had, indeed, a strong and straight attitude that helped him to protect scientific matters and appointments from political influence. Of course, this position attracted the aversion of many Fascist party officials who strove for Mussolini to remove him as president of the Italian Central Institute of Statistics. And his criticisms on the occasion of the shipwreck were added to the list of Gini's "offences" to the regime that led to his resignation in 1932. However, as I said before, he kept collaborating with the regime as a scientific expert in demography, statistics and eugenics.

A: Cantelli, de Finetti and Gini were the towering figures in probability and statistics before World War II in Italy. They were also completely different characters. How did they get along?

E: Gini published de Finetti's work on Mendelian inheritance in *Metron* and offered de Finetti a job at the Italian Central Office of Statistics before he graduated. While at the Italian Central Office, de Finetti was involved in a project for predicting the evolution of the Italian population and crucially designed all modeling aspects of the project. He then wanted this to be credited as his contribution, but Gini was reluctant to do so. This episode is well documented in one of the letters de Finetti wrote to his mother and contained in the collection published by his daughter that I have already mentioned. In any case, at the end of his four year contract in 1931, de Finetti moved back to Trieste and started to work for the insurance company *Generali*. The relationship between de Finetti and Cantelli was quite a difficult one, since they were in strong disagreement on the interpretation of probability. Cantelli did not want to hear anything about finite additivity and he also tried to prove that σ-additivity was a necessary property.

I: In addition to *Metron*, there was also the *Giornale dell'Istituto Italiano degli Attuari* (*GIIA*), which was a top journal in statistics and probability during the 1930s. Why have they both lost their international reputation since then?

E: The *GIIA* was established in 1930, the same year *The Annals of Mathematical Statistics* published their first issue. It was edited by Cantelli and the most distinguished scholars of the time, such as Cramér, Fréchet, Kolmogorov, Khintchine, Lévy, Neyman and von Mises, published fundamental contributions on it. World War II ruined everything, since its publication was suspended and the *GIIA* lost its elite status among the top probability and statistics journals which, during and soon after the war, included *The Annals of Mathematical Statistics*, along with *Biometrika* and the *Journal of the Royal Statistical Society*. The other Italian prestigious journal, *Metron*, which was established in 1920, paid a high price for the line of development of Italian methodological statistics and actually already declined before the war.

A: In 1978 you actually published a very interesting paper characterizing the Dirichlet process in terms of linear predictive distributions (Regazzini, 1978) on the *GIIA*. Why did you decide this was a suitable outlet for your paper?

E: After my discussions with Rukhin, the Dirichlet process became a main ingredient of my research agenda. In fact, I was dealing with risk premium models, for insurance companies, which were linear combinations of an empirical part and an expected value related to some prior guess—they identify the so-called credibility premium. I thought to revisit the problem coherently with the predictive distributions generated by an exchangeable sequence and asked myself what the underlying de Finetti measure was: it turned out to be the law of a Dirichlet process. I wrote this paper while I was working with Cifarelli on the distribution of linear functionals of the Dirichlet process. I then presented it at a conference, where Luciano Daboni, an editorial board member of *GIIA*, was present: he liked the paper a lot, invited me to give a seminar in Trieste and proposed for me to publish it in *GIIA*. Some years later the same result was independently obtained by Albert Y. Lo (Lo, 1991).

I: During the 1980s you were probably one of the few statisticians in Italy who published their papers in international journals. Do you have any idea why this happened at the time?

E: Well, first of all, most people, both in statistics and probability, did not even try to submit their work abroad. It was simply not necessary for the progress in academic careers. Even many mathematicians only published in Italian journals. Overall, the need for trying to spread one's own work at an international level was not felt yet. Actually, it was probably not even felt in the Anglo-American world: it just happened that their journals then became the "international" ones. By the way, papers that were published in Italian journals with a very limited spreading were not all necessarily of bad quality. On the contrary, some of them are very well known even abroad and contain innovative ideas. Anyhow, in recent years things have changed substantially and young researchers submit their work to the best international journals.

A: We, as students, have nice memories of summer schools organized by the Italian scientific community to support the spreading of probability and statistics. You have been an active part of this initiatives.

E: After attending some of them as a student, I have been involved several times in organizing and teaching at summer schools that took place in various beautiful locations in Italy, such as Cortona, Perugia, Livigno and Rhême-Notre Dame. In addition to being an opportunity to meet talented students, summer schools also allowed me to get in contact, and actually build up friendships, with some authoritative scholars such as

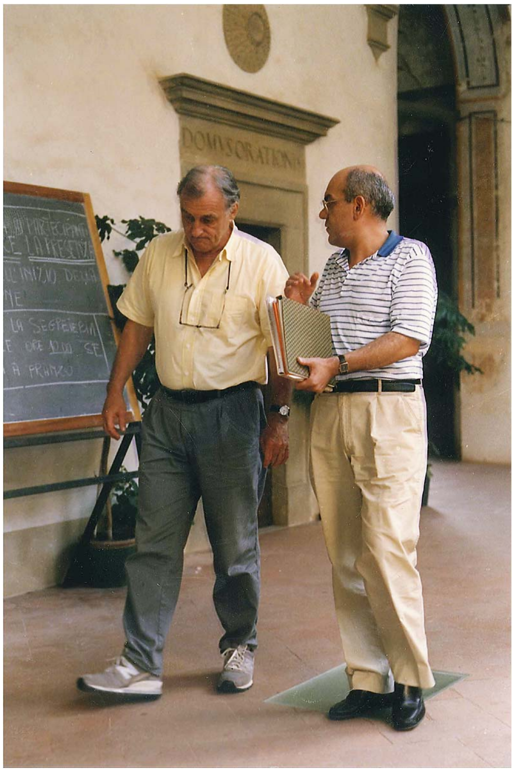

FIG. 1. *Patrick Billingsley with Eugenio in Cortona*, *Summer School*, 1989.

Alan Agresti, Patrick Billingsley, Albert Y. Lo, Slava Sazonov, Henry Teicher and Jon Wellner. Unfortunately, the cuts operated through the years by the Italian governments have made it more difficult to sustain the organization of such praiseworthy initiatives.

A: You then also started to collaborate with Sazonov. In fact, one of your articles we really enjoyed reading, for the wealth of results and techniques it offers, is Regazzini and Sazonov (2000). How did you convince him to do research on Bayesian statistics?

E: Slava was a wonderful person I really miss. He was a loyal friend to me and an extraordinary mathematician. I first met him at a conference at the beginning of the 1990s and, then invited him to deliver a course, jointly with Albert Lo, at a summer school organized by Bocconi University in 1992. He then taught also in the 1993 and 1994 editions. We started collaborating in 1996, while he was teaching a course on "Probability Theory in Hilbert spaces" at the Italian National Research Council in Milano. Our first joint work concerned central limit theorems for partially exchangeable arrays of random elements taking values in a Hilbert space. At the time I was also preparing my lectures for a Ph.D. course on Bayesian nonparametrics to be taught in Roma and I was dealing with

FIG. 2. *Patrick Billingsley, standing in the far right, and Eugenio with some students in Cortona, Summer School,* 1989.

the problem of estimating a statistical model by means of a mixture of Dirichlet processes. Such a problem was suggested by Diaconis and Ylvisaker (1985): with Slava we showed that it is possible to construct a mixture of laws of Dirichlet processes that approximates the distribution of any random probability measure, with respect to the topology of weak convergence. And we have been able to obtain, under suitable assumptions, the corresponding approximation bounds for the posterior measures. These results were presented at the 1st Workshop on Bayesian nonparametrics that took place in Belgirate (Italy) in 1997. While we were working on this paper, my mother became seriously ill and Slava has been very important in supporting me in such a difficult period.

I: You have always had good relationships with probabilists and statisticians from Russia. For example, Ildar Ibragimov is another good friend of yours who has been several times in Pavia contributing to the Ph.D. program. I had the pleasure to attend his lectures and really enjoyed them.

E: It was actually Slava who suggested I contact Ildar Ibragimov. In fact, I had asked Slava indications for possible instructors for Ph.D. courses. And Slava mentioned about Ildar and told me that in addition to being a great scientist he was an excellent teacher. Of course, I knew Ildar by fame and I feared he would have not accepted my invitation but he did. I got the chance to meet in person not only a brilliant mathematician but also a wonderful person. His courses in Pavia were greatly appreciated and I liked the fact that he, and also Slava, was trying to adapt his lectures to the students' background. Our Ph.D. classes are quite composite, with most students having either mathematics or economics degrees. The former typically have good backgrounds in pure maths but not in statistics and probability, whereas for the latter it is the opposite. I remember Ildar asking me, "How come I have students in my class who know about Radon–Nikodým derivatives of stochastic processes but struggle with Fourier transform coefficients?"

6. RESEARCH

6.1 Bayesian Nonparametrics

A: Your papers with Cifarelli on functionals of the Dirichlet process (Cifarelli and Regazzini, 1979, 1990) are probably your most well-known contributions to Bayesian nonparametrics. And it is amazing how many connections your results have with a variety of research areas such as combinatorics, mathematical physics, theory of stochastic processes, the moments problem, and so on. Were you aware of these?

E: As I said, our original problem was merely of a statistical nature. From an analytical point of view, the task we were facing was very challenging, but we were not aware of the connections with seemingly unrelated areas of mathematics. We learned about some of these relations thanks to the paper by Persi Diaconis and Johannes Kemperman that was presented at the Valencia

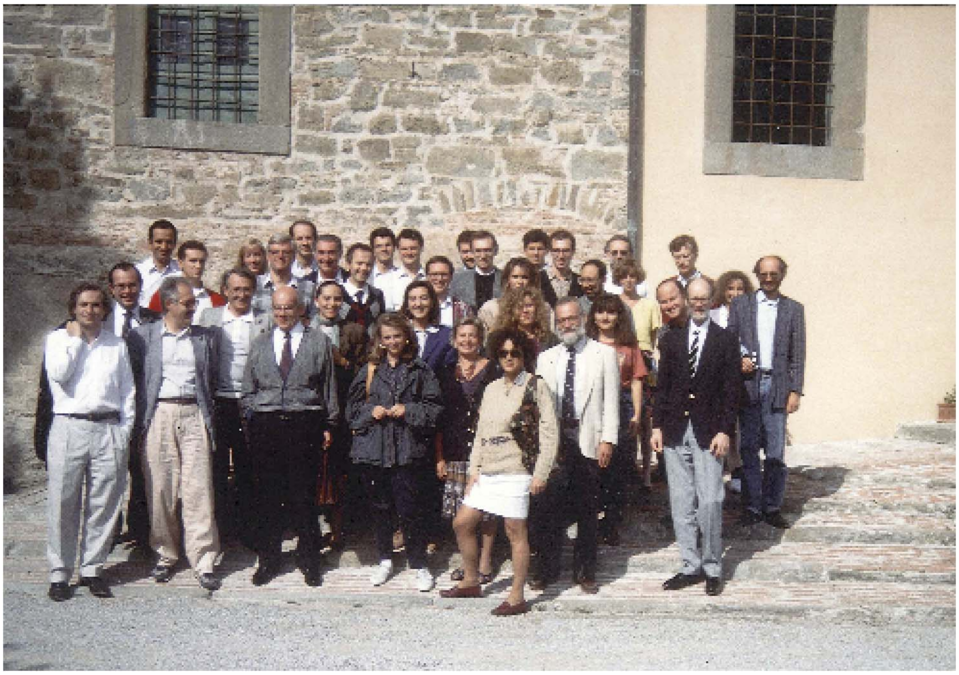

FIG. 3. *Workshop on "Recent developments in exchangeability," Cortona, October* 1991. *Among others, Luigi Accardi, Donato M. Cifarelli, Guido Consonni, Persi Diaconis, Joe Eaton, Colin Mallows, Jan von Plato, Maurizio Pratelli, Wolfgang Runggaldier, Marco Scarsini, Brian Skyrms, Fabio Spizzichino, Piero Veronese, Wolfgang Woess and Eugenio*.

meeting in 1994; see Diaconis and Kemperman (1996). In addition to embedding the whole problem in a wider mathematical context, it is also very well written and sketches a few open problems; I strongly recommend reading it. It is also thanks to this very same paper that my work with Cifarelli gained some popularity.

I: The basic trick you resorted to was the inversion of a Cauchy–Stieltjes transform for the mean of the Dirichlet process. How did you arrive to this intuition?

FIG. 4. *From the left: Alan Agresti, Eugenio and Slava Sazonov in Livigno, Summer School, July* 1993.

E: The procedure actually relied on the determination of recursive relations for the moments of the linear functional. Such a strategy was inspired by the work of M. Kac who used it to obtain the well-known Feynman–Kac formula; see, for example, Kac (1949). This closeness is further revealed by the adoption, in our paper, Cifarelli and Regazzini (1979), of the same symbols used by Kac! Cifarelli had successfully used it to establish a closed form expression for the probability distribution of the integral of the absolute value of the Brownian bridge in Cifarelli (1975). These recursive relations we obtained allowed us to determine the Laplace transform whose iteration yields the Cauchy–Stieltjes transform. We then resorted to the inversion formulae of the Cauchy–Stieltjes transform to deduce an exact form for the probability distribution of a linear functional of the Dirichlet process. Most of these ideas were already contained in Cifarelli and Regazzini (1979). In Cifarelli and Regazzini (1990) we basically completed that paper and provided some further insight.

A: More recently you developed an alternative method based on an inversion formula for the characteristic function.

E: The approach you are referring to was inspired by the representation of the Dirichlet process as the normalization of a gamma process that was first pointed

FIG. 5. *Slava Sazonov and Patrick Billingsley* (*on the left*) *and Eugenio and Andrew Rukhin* (*on the right*) *with some students in Rhemes-Notre-Dame*, *Summer School*, *July* 1994.

out by Ferguson himself in his 1973 paper. This representation combined with a suitable inversion formula led to new forms for the probability distribution of the mean of a Dirichlet process, which are recorded in a paper with Alessandra Guglielmi and Giulia Di Nunno. I have then extended, with the two of you, the approach to deal with means of random probability measures induced by the normalization of a generic process with independent increments.

I: At the moment, Bayesian nonparametric regression is a hot topic. In this respect, a paper of Cifarelli and yourself has been recently "rediscovered" (Cifarelli and Regazzini, 1978). Can you talk to us about its origin and contents?

FIG. 6. *From the left*: *Giorgio Dall'Aglio*, *Henry Teicher and Eugenio in Perugia*, *Summer School*, *August* 1995.

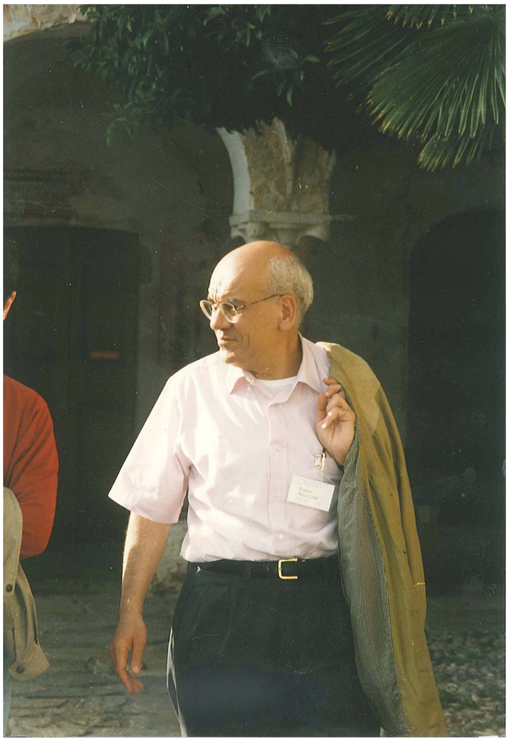

FIG. 7. *Eugenio in Belgirate*, 1*st Bayesian Nonparametrics Workshop*, *June* 1997.

FIG. 8. *From the left*: *Donato M. Cifarelli*, *Persi Diaconis and Eugenio at Stanford University*, *July* 2002.

E: The original goal of our research was to determine a probability distribution for partially exchangeable arrays of random elements. In particular, we were looking for a solution that could be treated analytically, while avoiding the independence assumption among rows. These were the two reasons which led us to the idea of resorting to the mixture of products of Dirichlet processes. We have been able to determine the associated system of predictive laws and the distribution of vectors of functionals. In a parametric setting, partial exchangeability had been incorporated in a paper by Lindley and Smith (Lindley and Smith, 1972). I then used our model to study credibility formulae with collateral data. Cifarelli had also developed the model for applications to ANOVA and linear models, the latter in collaboration with Marco Scarsini and Pietro Muliere. We did not even submit the paper to a journal, since, as I said, at the time a technical report or a journal publication counted the same for us. Nowadays, I am really pleased to see the recent explosion of proposals on dependent nonparametric models, somehow in the spirit of our 1978 paper, developed by S. MacEachern, P. Müller, D. Dunson and many others.

I: In our opinion, the work of two probabilists, John F. C. Kingman and Jim Pitman, has to be listed among the main and most far reaching contributions to Bayesian nonparametrics, even if not directly focused on it. Do you share this view?

E: I am strongly in favor of a Bayesian approach that solely relies on the specification of distributions for observable random elements. Therefore, in general, I like all those contributions and tools that aim at providing systems of predictive distributions related to modeling and applications. These do not resort to conditional distributions, given parameters (either finite or infinite-dimensional) that in some applications would be devoid of any empirical meaning. And, the works by Kingman and Pitman, although originated in different

FIG. 9. *Jon Wellner and Eugenio with some students in Cortona*, *Summer School*, *August* 2004.

FIG. 10. *Eugenio at a conference on "Non–linear PDEs: homogeneization and kinetic equations," Wien, June* 2006. *In the picture, among others: Peter Markowich, Pierre Degoud, Eric Carlen, Maria C. Carvalho, Ester Gabetta, Giuseppe Toscani, Cristian Ringhofer, Anton Arnold and George Zubelli.*

research areas, have an important impact on Bayesian statistics. Even though I read their papers only recently, I have appreciated them very much since they open up the possibility of implementing the Bayesian paradigm in the direction I lean toward.

6.2 Exchangeability

A: The contributions of Kingman and Pitman you just mentioned are closely related to exchangeability, a topic you extensively worked on both from a statistical and probabilistic point of view.

E: My interest in exchangeability was stimulated by reading de Finetti's papers. The first place where I came across the statement of de Finetti's representation theorem was the monograph by Loève. But I could not understand its statistical implications. I could appreciate its relevance for inductive reasoning only through a careful study of de Finetti (1930a, 1937a): in my opinion, these papers really stand out in terms of conceptual and mathematical rigor and effectiveness in highlighting the role of exchangeability for induction, and remain unbeaten to date. Of course, the modern uses of exchangeability and the key role it plays in modeling a variety of phenomena are probably beyond what de Finetti could have expected.

I: You have also been working on characterization theorems in this context.

E: You are probably referring to results I have obtained with Sandra Fortini and Lucia Ladelli and that characterize systems of predictive distributions associated with exchangeable sequences of random elements. I have also noted that these kinds of results have recently attracted more and more interest in Bayesian nonparametrics practice. Another interesting characterization was obtained in a paper I coauthored with Giovanni Petris where we dealt with exchangeability in the presence of finitely additive probabilities: we stated and proved a weak version of the representation theorem that reduces to the celebrated de Finetti theorem (strong version) if one specializes to the case of σ-additive probabilities. In this situation, we were also able to use the representation theorem to show existence of a random probability measure defined by means of a system of finite-dimensional distributions agreeing with Ferguson's framework.

FIG. 11. *IMS President Jim Pitman with Eugenio at the IMS Fellows Ceremony*, 70*th IMS Annual Meeting*, *Salt Lake City*, *July* 2007.

I: You have also provided nice contributions to the investigation of properties of partially exchangeability.

E: Indeed, I have been, and I still am, interested in forms of dependence more general than exchangeability, as witnessed by some contributions I have already mentioned before, such as the paper on mixtures of products of Dirichlet processes or the formulation of a central limit theorem for partially exchangeable arrays. Besides these, I wish to mention a nice characterization of partially exchangeable arrays that has been established in a paper I wrote with Fortini, Ladelli and Petris. Indeed, we proved a conjecture formulated in de Finetti (1959), according to which a suitable random matrix related to the transitions of a recurrent process is partially exchangeable if and only if the law of the process can be represented as a mixture of laws of Markov chains. Moreover, we have been able to show that de Finetti's definition of partial exchangeability is equivalent to the one provided by Diaconis and Freedman in a couple of papers they wrote in 1980.

6.3 Subjective Probability

I: In some of your work you have also provided some insight into an approach to Bayesian statistical inference based on finitely additive conditional probabilities.

E: I started getting involved into research on finitely additive conditional probabilities after reading some papers by R. Scozzafava in the first half of the 1980s. In fact, I grew convinced that countable additivity was not justifiable—as a necessary condition—unlike finite additivity which is necessary for the validity of de Finetti's coherence principle. Therefore, finitely additive probabilities have to be considered as admissible and I became interested in revisiting known results in probability as particular cases of the finitely additive framework. In particular, I found the interpretation of the definition of conditional probability, as given by Kolmogorov, unsatisfactory. Conditioning is based on classes of events that partition the whole sample space and that become finer and finer: conditional probability is then obtained through a limiting process in terms of a Radon–Nikodým derivative, and depends on the class of events one conditions on. In de Finetti's approach, a conditional probability, given an event, is defined through a natural, and unavoidable, strengthening of the coherence principle. De Finetti himself had hinted at such a possibility, without developing his idea in general mathematical terms. I tried to make this more explicit in some papers I wrote during the 1980s in Regazzini (1985, 1987). These topics have been object of further investigation by my friends P. Berti and P. Rigo. An important point is that many situations that appear as paradoxical if one refers to Kolmogorov's conditional probabilities can be justified within the finitely additive framework.

A: Can you provide us with an example?

E: The most well-known is probably Borel's paradox. Indeed, if a uniform distribution on the surface of a sphere is defined, with respect to a specific choice of geographic coordinates (namely, latitude and longitude), one would expect that the conditional distribution for latitude, given a fixed longitude, is uniform. However, this does not happen in Kolmogorov's framework. In de Finetti's approach, instead, one can adopt the more intuitive probability assessment even if it would be nondisintegrable. The reason for such a behavior can be traced back to the specific notion of conditional probability according to Kolmogorov's approach, since it does not admit the evaluation of the probability of an "isolated event" with probability zero.

On the contrary, de Finetti's setup is open to different solutions: indeed, disintegrability turns out to be *not necessary* for coherence.

I: Another amusing aspect of finitely additive conditional probabilities emerges from your work on well-calibration of systems of predictive distributions.

E: Loosely speaking, well-calibration corresponds to situations where the distance between weighted averages of forecast probabilites and empirical observations converges to zero as the number of observations, and forecasts, increases. Kolmogorov's theory always yields well-calibrated predictions or forecasts. This corresponds to a somehow unrealistic situation in practice, since one would also expect cases of not well-calibration. With P. Berti and P. Rigo we were interested in checking whether the same was true within de Finetti's theory as well. Our curiosity to this problem was stimulated by a paper of Phil Dawid (Dawid, 1982). The answer we got was naturally affirmative for *strategic* conditional probabilities. The term strategic was coined by Dubins and Savage in their well-known monograph where they resorted to de Finetti's theory to solve quite complicated measurability problems. Strategic conditional probabilities do indeed preserve, in a finitely additive setting, the disintegrability property that characterizes Kolmogorov's definition. As for well-calibration, we were able to show that, beyond strategic evaluations, there exist not well-calibrated coherent Bayesian predictors with positive probability.

A: Many critics of de Finetti's subjectivistic standpoint in probability theory use, as an argument for supporting their position, de Finetti's sentence "probability does not exist." What can be replied to such objections?

E: First of all, one should consider the provocative nature of de Finetti's sentence. Moreover, its meaning should not be decontextualized. According to de Finetti, if one wants to give probability an objective meaning, one should prove its existence. In other words, there should be an existence theorem, a clear proof of the existence of an object termed "probability." For example, the interpretation of probability as a limiting frequency cannot be considered as a proof, even if just empirical, of its existence. Hence, he used the expression "probability does not exist" just to make the point that probability has simply a subjective meaning. It is also to be said that most of the criticism raised against subjectivism basically refers to the contents of his two-volume monograph, de Finetti (1970), which is, according to its subtitle, "a critical introductory treatment." In my opinion, de Finetti's position can be better discussed by relying on his early works, which are more concise, go straight to the point and display more mathematical and formal details.

I: A noteworthy scholar who contributed to the theory of finitely additive probabilities was Lester Dubins. You were also a good friend of his and had the chance to host him in Milano.

E: Dubins had been in Italy several times and he delivered courses at summer schools. He was very fond of Italy and, in the second half of the 1980s, I invited him once to stay for a month in Milano. We had discussions on various research topics. He was the source of many ideas that I later developed in my research. In those years I was mainly working on technical aspects of nonparametric inference, whereas he could provide me with many insights into theoretical issues related to finite additivity that turned out to be of great importance to me.

6.4 Probabilistic Methods for Mathematical Physics

A and I: You have lately become interested in some problems in mathematical physics. How did it happen?

E: Moving to Pavia in 1998, I joined a Mathematics Department with a few internationally well-known mathematical physics scholars. I started interacting with them and at some point a colleague of mine, Ester Gabetta, showed me some papers where the Central Limit Theorem was used to describe the convergence to equilibrium of the solution of certain kinetic equations. In particular, I read two papers, McKean (1966, 1967), that spurred my enthusiasm for the topic. I tried to understand and extend the connections with probability, and could count on the collaboration of my colleagues to help me understand the problem from the perspective of physics. Furthermore, the encouragement from Eric Carlen and Maria Carvalho has been important for pursuing my research in this direction. In fact, they liked our first results and suggested us to publish them (see Gabetta and Regazzini, 2006): there we obtained some identities that came in handy for later developments of the work in this area.

A and I: Was this line of research as rewarding as others you have pursued in your career?

E: I would say I am happy about what I have achieved so far with my coauthors. Starting from the Kac model, which is generally considered as a toy model, we obtained some interesting results concerning the characterization of the initial data in order to gain convergence to equilibrium. We have also considered situations where the energy, interpreted as the variance of the initial datum, is infinite and we performed an analysis of the speed of convergence. In these studies, I have also collaborated with Lucia Ladelli and Federico Bassetti. Later, I have supervised the thesis of Emanuele Dolera, a Ph.D. student in Pavia. This work has required a strong effort that was rewarded by the achievement of a noteworthy result proving the validity of a conjecture formulated in the 1966 McKean paper. In the last 40 years many scholars have worked hard with the aim of proving it.

7. THOUGHTS ON FOUNDATIONAL ISSUES AND RESEARCH IN STATISTICS

I: In some of the previous questions we have lingered on the subjectivistic interpretation of probability. What is the most relevant impact this has on statistics?

E: A crucial point to understand is whether it is worth preserving an axiomatization based on countable additivity. Of course, I think it does not generally have a statistical justification that makes its use necessary. If finitely additive probabilities are also admissible, then a considerable number of results in the literature should be revisited. I have already mentioned that one should reconsider the definition of conditional expectation. Moreover, a number of limiting theorems should be reformulated in order to account for this more general framework. These issues are also of great relevance in statistics regardless of the approach, either frequentist or Bayesian, one adopts.

A: Does this lead, among others, to a rethinking of Bayesian procedures?

E: Indeed, Bayesian procedures are typically implemented by assuming complete additivity and this leads to assume some of its implications as necessary. Let us consider, as an example, the Dirichlet process. A well-known result is that the Dirichlet process selects, almost surely, discrete probability measures. However, such a property holds true for the countably additive extension of the collections of finite-dimensional probability distributions of the process. There are other non σ-additive extensions for which the Dirichlet process selects nondiscrete distributions with positive probability. This points to the fact that in statistical practice one should avoid assessing a probability for objects devoid of empirical evidence. For example, take the proposition stating that de Finetti's measure is the law of the (almost sure) weak limit of the empirical distribution: thus, it depends on infinitely many observations and concerns "transcendent"—in de Finetti's words—conditions not directly verifiable. The conclusion of such a proposition could be obviously false with non σ-additive extensions. On the other hand, the fact that de Finetti's measure is the weak limit of the low of the empirical distribution, as the sample size increases, is, in any case, true: in my opinion this suffices with respect to sound statistical goals. I think this is an important foundational aspect, which is often neglected and should be further investigated.

I: Are you saying that one should have clear in mind the different levels at which mathematics and statistical applications operate?

E: More or less, that is what I mean. Indeed, it is true that mathematics makes parameters interpretable as limits of (or of functionals of) empirical processes, but it does not automatically grant that inference on them are legitimate.

A: Does this position contrast with the usual way of presenting a Bayesian model as the combination of a likelihood and a prior?

E: Let me start by making an important point that reflects my view on statistics: if inference is seen as a decision problem to be solved under uncertainty and if one agrees that probability is a tool to resort to, then there is no other choice but the Bayesian approach. Nonetheless, I agree with what Diaconis and Ylvisaker say at the beginning of their paper Diaconis and Ylvisaker (1985): Bayesian statistics cannot be reduced to the elicitation of a prior and the automatic application of Bayes' theorem. Hence, I would give an affirmative answer to your question if one conditions on unobservable quantities. But this is not limiting the scope of Bayesian inference at all. Indeed, one can think of inferential procedures that can still be implemented in this more general framework, even when unobservable parameters are involved. The previously mentioned "weak" interpretation of the de Finetti measure says that a prior distribution can be viewed, in any case and with no distinction between observable and not observable parameters, as an approximation of the law of a frequency distribution or of some functional of it. Moreover, prediction can be carried out without relying on the Bayes–Laplace paradigm: it is enough to specify the system of predictive distributions connected to the exchangeable sequence. And I have appreciated very much the work by J. Pitman which, in the spirit of de Finetti's stance, relies on the proposal of systems of predictive distributions that are then proved to be associated to an exchangeable sequence.

A: I also guess that a subjectivist would not agree on the notion of posterior consistency as a frequentist validation criterion of Bayesian nonparametric methods.

E: I have to admit that, besides the Bayesian context, I am skeptical on the use of consistency in a frequentist setting as well. On the one hand, these limiting results are very neat and beautiful from a mathematical point of view. But, on the other, they lack a sensible statistical interpretation. This is very well discussed in de Finetti (1970), Volume 2, in the section devoted to the laws of large numbers where he motivates why results, such as consistency, do not represent justifications of statistical procedures under the assumption of stochastic independence. The same can be said for the Glivenko–Cantelli theorem. Of course, my position encompasses commonly used frequentist validation criteria adopted in a Bayesian framework. A different role must be attributed to approximation results, like Central Limit Theorems or, also, the "weak" interpretation of the de Finetti measure, for which these concerns do not apply.

I: So, what are the kind of asymptotic problems that you think are interesting for Bayesians?

E: The kind of results I like are those in the spirit of Blackwell and Dubins (1962) where they investigate the phenomenon of the merging of opinions. This is a very nice finding both from a mathematical and from a statistical point of view. On the one hand, it is a general result valid even beyond exchangeability. On the other hand, it has a nice statistical interpretation for Bayesians since it hints at the predominance of empirical findings over different subjective prior opinions as the sample size or, in other terms, the amount of information, increases. Other results of great interest are those that currently are designated as Bernstein–von Mises type theorems for the posterior distribution. It is worth noticing that among the first contributions to this topic there is also an important paper, Romanovsky (1931), published on *GIIA*.

A: From what you said up to now on the interplay between Bayesian inference and de Finetti's interpretation of probability, a valuable research topic would focus on the analysis of the asymptotic behavior of the predictive distributions.

E: You are right. In my opinion, an important issue to address is the analysis of the distance between the predictive and the empirical distributions. Instead of looking at the limiting behavior, it would be more interesting to analyze how such a discrepancy changes for any sample size n and, *a fortiori*, as n increases. Since the predictive can also be obtained as a functional of the posterior distribution, one can also gain some insight if one relies on convergence theorems, which say that the posterior converges, in some sense, to a distribution concentrated on the limit of the empirical process. In this respect, Bernstein–von Mises type results are useful.

I: You have had a large number of students, and by now also descendants, working in many different universities in Italy and abroad. In your opinion, what is the background a statistics student needs to perform well in nowadays research and what are the topics you would suggest to pursue?

E: As I said earlier, I see statistics as inductive reasoning under the supervision of probability theory. Therefore, it is natural that I firmly believe that statisticians should have a solid background in probability: the more the better. However, a statistician must also be able to think through the logical and philosophical aspects of what she/he is doing. This concerns modeling, the understanding of practical implications yielded by the mathematical formulation that is used, and the interpretation of results. Mathematical skills are not enough, logical and conceptual rigor being a necessary complement. One needs to be able to handle statistics since it is a powerful instrument, which allows one to make substantial steps forward, compared to traditional deterministic procedures. Statistics can get you close to the best solutions, avoiding overwhelming technical and mathematical difficulties that often arise within deductive deterministic reasoning. The latter approach lacks the flexibility of a learning mechanism, whereas in the probabilistic framework everything is kept under control: you have a law which governs everything and, unless you change the learning mechanism, it allows one to learn from experience in a way that is transparent and controlled by Bayes' theorem.

A: In modern science the specialization of researchers is constantly increasing. Even probability and statistics, which have grown in close relationship to each other, seem to be drifting apart.

E: You can observe the fragmentation of fields all over the place. This phenomenon also originates from an excessive specialization that characterizes most undergraduate studies. The situation was, in the past, quite different and there were many scholars with a wide spectrum knowledge and diversified cultural and scientific interests. De Finetti and Gini are excellent examples in this respect. That said, fragmentation in research is unavoidable and it would be unrealistic to try reversing it. It is just a pity to see that it tends to create duplications and repetitions, whereas a more cohesive scientific community could produce better results in a collective effort. In statistics, Bayesian statisticians have kept to themselves for some time in reaction to the then mainstream statistics, which was certainly not in favor of Bayesian methods. Now with Bayes statistics well-established, I note that younger generations are more open to interactions with non-Bayesian, which in my opinion is certainly beneficial. A different issue is the specialization in education, which should be contrasted to some extent because it precludes possible paths to future researchers. As I have already said, every statistician should have a solid background in probability and every probabilist should know the basics of statistics, which is a noble and fascinating, at least to me, field of application of probability.

I: How should, in your opinion, a good statistics paper be structured?

E: Well, first of all the definition "good" is to be considered with reference to the historical period. Until some years ago theoretical papers were very appreciated, whereas nowadays applied work plays an increasing role thanks to the computational tools. However, I think that, in general, different forms of motivation are equally valid: an enrichment of the available tools, an improvement over other existing contributions or a useful application are all fine. However, in all cases it is crucial that the paper is logically sound and coherent with its motivation. This is essential since we write for the scientific community and not for the general public, which is another job. For instance, I do not like methodological papers, to which an illustration has been evidently added only for editorial needs. A methodological contribution can stand on its own if its motivation is sound. While I was young I experienced some of the last manifestations of Gini's school, which as a rule of thumb required publications to include data, a table and a plot. To me this does not make sense. I also do not like applied papers in which one sets forth a model, analyzes a couple of data sets and concludes that the model works well. Any so-called empirical validation does not show anything and is not enough to assess the suitability of a model. Indeed, there should also be a sensible methodological motivation in the sense that one should explain which features of a certain model make it more appropriate for the problem at hand.

A and I: Moving away from statistics and probability, we already mentioned your passion for music. How did you get fond of music and what else are you interested in when you do not do research?

E: Being born in Cremona, my passion for music is quite natural: it is 20 km away from the places Giuseppe Verdi grew up in, melodrama is popular and there is a great tradition. It is also the hometown of Claudio Monteverdi and of Amilcare Ponchielli, two famous composers. Last but not least, it is the town of lute makers, the most renowned being Antonio Stradivari. Even the general public knows opera very well. Then, starting from opera when I was young, my interest extended to symphonic music. I have also been fond of visual arts since I was a kid: I loved paintings, architecture and sculpture since I related them to Italian history. I remember having a great teacher at school who used to emphasize links between history, arts and literature. A peculiar feature of Italy is that, if you are interested in any historical aspect, you necessarily end up considering also painting, sculpture and architecture since they are all intimately connected. We obviously benefited from Christian culture that played a fundamental role, after the fall of the Roman Empire, in preserving the wonders inherited from classical Greek and Roman traditions and in promoting arts in forms we can today admire while visiting churches, historical buildings, squares and museums. Moreover, during the Renaissance, there were a large number of small states and many of them had patrons who liked and could afford being surrounded by artists. Hence, many towns developed their peculiar artistic heritage.

A and I: Eugenio, thanks a lot for patiently answering all our questions.

E: Thanks to you for listening to all this!

ACKNOWLEDGMENTS

Many thanks to Elena Di Biase for a careful reading of the manuscript and several useful suggestions which improved the presentation. This work is partially supported by MIUR, Grant 2008MK3AFZ.

REFERENCES

ALLEN, R. G. D. (1956). *Mathematical Economics*. MacMillan, London.

BLACKWELL, D. and DUBINS, L. (1962). Merging of opinions with increasing information. *Ann. Math. Statist.* **33** 882–886. MR0149577

CASTELNUOVO, G. (1919). *Calcolo delle Probabilità*, 1st ed. Soc. Ed. Dante Alighieri, Milano.

CIFARELLI, D. M. (1975). Contributi intorno ad un test per l'omogeneità tra due campioni. *Giorn. Econom. Ann. Econom.* (*N.S.*) **34** 233–249. MR0433704

CIFARELLI, D. M. and REGAZZINI, E. (1974). Ancora sull'indice di cograduazione del Gini. Technical Report Serie III, No. 5, Istituto di Matematica Finanziaria dell'Universita, Torino.

CIFARELLI, D. M. and REGAZZINI, E. (1978). Problemi statistici non parametrici in condizioni di scambiabibilità parziale: Impiego di medie associative. Quaderni Istituto di Matematica

Finanziaria Dell'Università di Torino, Serie III, N. 12 1–36. English translation available at http://www.unibocconi.it/wps/allegatiCTP/CR-Scamb-parz[1].20080528.135739.pdf.

CIFARELLI, D. M. and REGAZZINI, E. (1979). A general approach to Bayesian analysis of nonparametric problems. The associative mean values within the framework of the Dirichlet process. II. *Riv. Mat. Sci. Econom. Social.* **2** 95–111. MR0574040

CIFARELLI, D. M. and REGAZZINI, E. (1990). Distribution functions of means of a Dirichlet process. *Ann. Statist.* **18** 429–442. [Correction in *Ann. Statist.* (1994) **22** 1633–1634]. MR1041402

DALL'AGLIO, G. (1956). Sugli estremi dei momenti delle funzioni di ripartizione doppia. *Ann. Scuola Norm. Sup. Pisa* (3) **10** 35–74. MR0081577

DAWID, A. P. (1982). The well-calibrated Bayesian. *J. Amer. Statist. Assoc.* **77** 605–613. MR0675887

DE FINETTI, B. (1926). Considerazioni matematiche sull'ereditarietà mendeliana. *Metron* **6** 3–41.

DE FINETTI, B. (1929). Sulle funzioni a incremento aleatorio. *Atti Reale Accademia Nazionale dei Lincei, Serie VI, Rend.* **10** 163–168.

DE FINETTI, B. (1930a). Funzioni caratteristiche di legge istantanea. *Atti Reale Accademia Nazionale dei Lincei, Serie VI, Rend.* **12** 278–282.

DE FINETTI, B. (1930b). Funzione caratteristica di un fenomeno aleatorio. *Atti Reale Accademia Nazionale dei Lincei, Mem.* **4** 86–133.

DE FINETTI, B. (1931). Sul significato soggettivo della probabilità. *Fund. Math.* **17** 298–329. [English translation in de Finetti (1992) 291–322.]

DE FINETTI, B. (1933). Sull'approsimazione empirica di una legge di probabilità. *Giornale dell'Istituto Italiano degli Attuari* **4** 415–420.

DE FINETTI, B. (1935). Il problema della perequazione. In *Atti della Società Italiana, per Il Progresso delle Scienze* **2** 227–228.

DE FINETTI, B. (1937a). La prévision: Ses lois logiques, ses sources subjectives. *Ann. Inst. H. Poincaré* **7** 1–68. MR1508036

DE FINETTI, B. (1937b). A proposito di "correlazione." *Supplemento Statistico Ai Nuovi Problemi di Politica, Storia ed Economia* **3** 41–57. English translation available at http://www.brunodefinetti.it/Opere/AboutCorrelations.pdf.

DE FINETTI, B. (1939). La teoria del rischio e il problema della "rovina dei giocatori." *Giornale dell'Istituto Italiano degli Attuari* **10** 41–51.

DE FINETTI, B. (1940). Il problema dei "Pieni." *Giornale dell'Istituto Italiano degli Attuari* **11** 1–88. MR0019248

DE FINETTI, B. (1959). La probabilità e la statistica nei rapporti con l'induzione secondo i diversi punti di vista. In *Centro Internazionale Matematico Estivo (CIME) Induzione e Statistica* 1–115. Cremonese, Roma. [English translation in de Finetti (1972) 147–227.]

DE FINETTI, B. (1970). *Teoria delle Probabilità. Sintesi Introduttiva Con Appendice Critica. Vol.* 1, 2. Einaudi, Torino. [English translation: De Finetti, B. (1974 and 1975). *Theory of Probability: A Critical Introductory Treatment. Vol.* 1, 2. Wiley, London.]

DE FINETTI, B. (1972). *Probability, Induction and Statistics. The Art of Guessing*. Wiley, London. MR0440638

DE FINETTI, B. (1992). *Induction and Probability*. A supplement to the journal *Statistica* **52** (P. Monari and D. Cocchi, eds.). CLUEB, Bologna.

DE FINETTI, B. (2006). *Opere Scelte, Voll. I e II*. Unione Matematica Italiana.

DE FINETTI, B. and SAVAGE, L. J. (1962). Sul modo di scegliere le probabilità iniziali. In *Biblioteca del Metron, Serie C, Note e Commenti*, 82–15. Istituto di Statistica Dell'Università, Roma. [English summary in de Finetti (1972) 143–146.]

DEGROOT, M. H. (1970). *Optimal Statistical Decisions*. McGraw-Hill, New York. MR0356303

DIACONIS, P. and KEMPERMAN, J. (1996). Some new tools for Dirichlet priors. In *Bayesian Statistics* **5** (J. M. Bernardo, J. O. Berger, A. P. Dawid and A. F. M. Smith, eds.) 97–106. Oxford Univ. Press, New York. MR1425401

DIACONIS, P. and YLVISAKER, D. (1985). Quantifying prior opinion. In *Bayesian Statistics* **2** (A. K. Bernardo, M. H. DeGroot, D. V. Lindley and A. F. M. Smith, eds.) 133–156. North-Holland, Amsterdam. MR0862488

FERGUSON, T. S. (1967). *Mathematical Statistics: A Decision Theoretic Approach. Probability and Mathematical Statistics, Vol.* 1. Academic Press, New York. MR0215390

FERGUSON, T. S. (1973). A Bayesian analysis of some nonparametric problems. *Ann. Statist.* **1** 209–230. MR0350949

GABETTA, E. and REGAZZINI, E. (2006). Some new results for McKean's graphs with applications to Kac's equation. *J. Stat. Phys.* **125** 947–974. MR2283786

GINI, C. (1939). I pericoli della Statistica. *Supplemento Statistico ai Nuovi problemi di Politica, Storia ed Economia* **5** 1–44.

KAC, M. (1949). On distributions of certain Wiener functionals. *Trans. Amer. Math. Soc.* **65** 1–13. MR0027960

KHINTCHINE, A. Y. (1937). A new derivation of a formula by P. Lévy. *Bulletin of the Moscow State University* **1** 1–5. (In Russian.) [English translation in the Appendix of Mainardi, F. and Rogosin, S. (2006). An origin of infinitely divisible distribution: From de Finetti's problem to Lévy–Khintchine formula. *Math. Meth. Econ. Fin.* **1** 37–56.]

KOLMOGOROV, A. N. (1932). Sulla forma generale di un processo stocastico omogeneo. Un problema di Bruno de Finetti. *Atti Reale Accademia Nazionale dei Lincei, Ser. VI, Rend* **15** 805–808.

LINDLEY, D. V. (1965). *Introduction to Probability and Statistics from a Bayesian Viewpoint. Part I: Probability*. Cambridge Univ. Press, New York. MR0168083

LINDLEY, D. V. and SMITH, A. F. M. (1972). Bayes estimates for the linear model. *J. Roy. Statist. Soc. Ser. B* **34** 1–41. MR0415861

LO, A. Y. (1991). A characterization of the Dirichlet process. *Statist. Probab. Lett.* **12** 185–187. MR1130354

MCKEAN, H. P. JR. (1966). Speed of approach to equilibrium for Kac's caricature of a Maxwellian gas. *Arch. Rational Mech. Anal.* **21** 343–367. MR0214112

MCKEAN, H. P. JR. (1967). An exponential formula for solving Boltmann's equation for a Maxwellian gas. *J. Combinatorial Theory* **2** 358–382. MR0224348

PIZZETTI, P. (1963). *I Fondamenti Matematici per la Critica dei Risultati Sperimentali* (reprint of 1892 edition). Cappelli, Bologna.

RAIFFA, H. and SCHLAIFER, R. (1968). *Applied Statistical Decision Theory*. MIT Press, Cambridge, MA. MR0226757

REGAZZINI, E. and SAZONOV, V. V. (2000). Approximation of distributions of random probabilities by mixtures of Dirichlet distributions with applications to nonparametric Bayesian statistical inferences. *Theory Probab. Appl.* **45** 93–110.

REGAZZINI, E. (1978). Intorno ad alcune questioni relative alla definizione del premio secondo la teoria della credibilità. *Giornale dell'Istituto Italiano degli Attuari* **41** 77–89.

REGAZZINI, E. (1985). Finitely additive conditional probabilities. *Rend. Sem. Mat. Fis. Milano* **55** 69–89. MR0933711

REGAZZINI, E. (1987). de Finetti's coherence and statistical inference. *Ann. Statist.* **15** 845–864. MR0888444

REGAZZINI, E. (2005). Probability and statistics in Italy during the First World War I. Cantelli and the laws of large numbers. *J. Électron. Hist. Probab. Stat.* **1** 1–12. MR2208347

ROMANOVSKY, V. (1931). Sulle probabilità "a posteriori." *Giornale dell'Istituto Italiano degli Attuari* **4** 493–511.

SAVAGE, L. J. (1954). *The Foundations of Statistics*. Wiley, New York. MR0063582