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Encounters with the Italian Statistical School: a conversation with Carlo Benedetti

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1. INTRODUCTION

The idea of this conversation came to mind during a profitable exchange of opinions with prof. Samuel Kotz between December 1995 and April 1996 concerning my "entry" on the Bonferroni concentration index [Giorgi (forth.)] for the Encyclopedia of Statistical Sciences. He disclosed the interest of the international scientific community regarding not only the theoretical-methodological aspects of scholars' scientific contributions but also those aspects which do not appear in official biographies. That is to say the interest for details such as family background, education, marriage and children as well as relations, if any, with collaborators, academic institutions etc. In this way it was thought that scholars and their research could be understood better if more was known about the period and place in which the scholars lived and worked. During the above-mentioned period, I shared an office at the Department of Statistics, Probability and Applied Statistics at "La Sapienza" University with prof. Carlo Benedetti, once a pupil of Carlo Emilio Bonferroni (1892-1960), then a collaborator of Corrado Gini (1884-1965) and now an outstanding repre-

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sentative of the Italian statistical school. I therefore decided to take this opportunity to find out more about the lives of Bonferroni and Gini from someone who had been in very close contact with them.

2. Short biography

Carlo Benedetti was born in Castellina in Chianti (province of Siena) in Tuscany on 20 November 1921. He graduated from Florence University in 1948 with a degree in Economics and Business Administration. He was full professor of statistics at the University of Trieste in 1966 and then at the "La Sapienza" University in Rome from 1967 to 31 October 1996 ([1]).

3. The Florentine period and meeting with Carlo Emilio Bonferroni

Giorgi: Professor Benedetti, before talking about your "master", could you tell us something about your schooling and University studies as well as the hardship during the last world war?

Benedetti: After elementary school in Castellina in Chianti, I went to live with my maternal grandparents near Siena and here I attended a technical school. Afterwards I spent a year in Castellina not knowing what to do and then with the financial help of my maternal grandparents I became a boarder at an agricultural school in Scandicci near Florence where, two years later, I obtained the diploma of "farm bailiff". At the age of 17, I began my career as under farm bailiff on various farms.

I was not happy with the life I was leading and following the influence of certain readings (mainly *Martin Eden* by J. London) I succeeded in persuading my parents to send me to the Technical Agriculture Institute in Arezzo where three years later I obtained the diploma of agrarian surveyor which permitted me to go to University. It was 1940. I was awarded this diploma during military service in June 1943.

I decided to enrol at Florence University but my diploma only

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([1]) Prof. Carlo Benedetti left the University when he reached retiring age on 1st November 1996. His most important scientific results have been collected in two volumes of *Biblioteca di Metron* [Benedetti, 1996].

granted me entrance to the Faculties of Agriculture and Economics and Business Administration.

As a matter of fact, during the period in which I worked as under farm bailiff I led a very lonely life in the country and in order to make this existence more bearable I studied mathematics which I had previously found rather incomprehensible. I even went on to study infinitesimal calculus.

The University reopened in January 1945 and I immediately took my first exam which was mathematics with prof. Bonferroni.

G.: Do you remember how you met Bonferroni and what made you decide to do a thesis on actuarial mathematics?

B.: I knew about Bonferroni through his *Elements of Mathematical Analysis* [Bonferroni, 1957]. The first time I met him was at the exams in January 1945. I chose to do the thesis with him because his three exams had gone very well and I was now fascinated by the world of mathematics which I had entered into with difficulty after teaching myself in the lonely surroundings of my small fifteenth century tower in Castellina in Chianti.

G.: Your thesis was on actuarial mathematics but you decided to do research in statistics. Why did you make such a choice and how much was it influenced by the meeting with Bonferroni?

B.: I used to talk to Bonferroni also about statistics and through his *Elements of General Statistics* [Bonferroni, 1941] I realized that mathematics was of fundamental importance. Another reason, and maybe a more important one, was the fact that the world of mathematicians was (and still is today) a very closed one and therefore people like me who had not attended a mathematics faculty did not have many opportunities, even though I loved this field. Bonferroni said to me "hurry and take up a career in statistics because when mathematicians become aware of the possibility which it offers them, there will not be any more opportunities for people like you".

G.: Once you had got your degree you went to work for a private company and then for a bank in Florence for a period of time. What led you to this choice and did you continue to study statistics during this period?

B.: After getting my degree on 8th July 1948 I was unemployed for more than a year and it was only thanks to prof. Giuseppe Parenti (who I had contacted following advice from Bonferroni) that I was able to begin my first job as a graduate. It was a prestigious post for me, at the fi-

nancial firm "La Centrale" which controlled electrical, mining and agricultural companies. The firm wanted to train young graduates to become managers of the companies belonging to the group. There were two of us, but the firm soon realized that I was more suited to an office dealing with econometric studies rather than becoming a manager. After, I accepted a position in an actuary office at the Banca Toscana which Bonferroni procured me. I did not like the work (we were all in one big room in the old Portinari building in via del Corso in Florence with a head-clerk keeping an eye on everything we did from behind his raised desk). Every now and then they let me carry out some actuarial reserves in connection with the bank's private pension funds, but generally it was boring routine work in a personnel office. At the same time I was a temporary statistics assistant to prof. Parenti and often I visited the Bonferroni home (a lovely small villa in via Masaccio which has now been demolished) and this lifted my spirits.

G.: How did your work with Bonferroni begin and how long did it last?

B.: Collaboration with Bonferroni began in 1948 and continued to 1960 (the year of his death). I was in personal contact with him from 1948 to February 1952 and then from 1952 (the year I arrived in Rome) I kept in touch by letter.

In 1948, as soon as I graduated, he suggested I studied the θ_n function linked to Eulero-Mascheroni constant in order to keep myself occupied. He did not think I would ever have got to the bottom of it, but after some months, when I took him the results he was very surprised and sent the manuscript to his friend Oscar Chisini, who at that time was director of the *Periodico di Matematiche*. The paper was immediately accepted for publication [Benedetti, 1950].

G.: Could you describe Bonferroni the man and Bonferroni the scholar?

B.: He was a very kind, refined person. I was moved by the fact that when he met my employer at the financial firm "La Centrale" he had told him to treat me gently as I was a very sensitive boy. He was a tall, strongly built man with light blue eyes and not very much hair. He was protective towards me in the same way a father would be. He enquired about the company I kept and the life I led in Florence (I was not yet 30 and sometimes while I was out with a girl I would bump into him).

G.: Today Bonferroni is mainly remembered for his "inequalities" and for his contributions to the study of concentration. However, we all

well know that he has carried out scientific work in other fields. What can you tell us about this? What, in your opinion, are his most original contributions?

B.: As far as his "inequalities" are concerned, it was me who pointed the subject out to him in a book by Feller [1950] which had been taken from a monograph by Fréchet [1940, p.55-61], an appraiser of Bonferroni. He complained that only foreigners ever noticed his results. I have already spoken about the contributions made by Bonferroni in an article which appeared in *Metron* [Benedetti, 1982]. Now, very briefly, I can say that they are rather varied, ranging from pure mathematics to mathematical statistics and to actuarial mathematics. More than anything else however I was struck by his personal style and the simplifying solutions to the very complex procedures which he proposed. You only need to open his texts [Bonferroni, 1941, 1942, 1957; i.e. *Elements of General Statistics, Foundations of Actuarial Mathematics* and *Elements of Mathematical Analysis*] to discover this. He was also obsessed with printing mistakes and errors which are noticed only once the paper has been printed. In fact, I remember that once I had got hold of the latest edition (academic year 1940-41) of his *Elements of General Statistics* which he used at the "Bocconi" University in Milan, he did his utmost to get it from me and then he never gave it back again. Who knows what mistakes he had found. He also told me of an episode which had occurred concerning the famous mathematician Emil Borel who, in the first edition of his *Eléments de la théorie des probabilités* had made a mistake in determining the probability in a simple problem regarding the tossing of coins. Bonferroni, at that time a student at Turin University, found the mistake and sent the correct solution to Borel. He did not receive a reply but after a new edition of the French book appeared with the correct solution.

Now I would like to bring up a fact which I have already mentioned in my article in *Metron* [Benedetti, 1982]. That is that Bonferroni was scandalized by how Gini and his collaborators defined the maximum values of the indices of absolute variability. After having chosen a certain index, let's say S, of absolute variability, they defined $max\ S$ not by solving the problem of the conditional maximum and therefore finding the maximizing distribution in which the extreme of S is located, but by imposing it *a priori* on the basis of an intuitive concept of maximum variability. Even if, in practice, this often led to the same results, from a conceptual point of view there was the risk that the true mathematical maximum of S could be true for another distribution.

4. The Roman period and collaboration with Corrado Gini

Giorgi: Professor Benedetti, you came to Rome University in 1952. I lived in Siena and taught at the University there for about 25 years so I am well aware of the reluctance of the Sienese to leave their home town. I am therefore curious to know what made you decide to move to Rome.

Benedetti: Let me first of all tell you that I have never felt Sienese, even if from an administrative point of view Castellina in Chianti is part of the province of Siena. In fact, as far as dialect and mentality are concerned my home town is much nearer to Florence than to Siena, maybe because historically Castellina was an important Florentine outpost of the "Chianti League". Lorenzo il Magnifico gave Giuliano da Sangallo the task of strengthening the medieval fortress (now the home of the town hall) as well as the various ramparts-redoubts (including my aforementioned "small tower"). I would also like to add that from choice I like to consider myself as belonging to Florence where my most fondest memories lie, unfortunately I cannot say the same for the period which I spent in Siena.

G.: What impression did Gini make upon you in your first meeting? Were you shy of this scholar who was famous both in Italy and abroad?

B.: My first meeting (6th January 1952) was in the Faculty of Statistical, Demographic and Actuarial Science, founded by Gini himself in 1936, which was situated on the top floor of an old building, looking out onto Piazza Esedra, in via Terme di Diocleziano. You could say that the Institute of Statistics directed by Gini, together with his other "creation", that is the Italian Committee for Population Studies (CISP) practically took up the whole of the premises. At that time he was also Dean of the Faculty and remained so until 1954. The professors of other subjects only came to do lessons as they did not have anywhere to put their bags down. They used to sit at Piccarozzi Bar (under the arcade in Piazza Esedra) to discuss and make arrangements for theses. Gini's assistants and some other professors used to work in small glass boxes, fitted with a microphone which Gini could use to listen and talk, but the occupier of the box could only reply if spoken to. There was only one amphitheatre style classroom.

Going back to the first meeting, I had the impression that he was not a very healthy man, he appeared older than he was (68) and he seemed kind and rather meek. I soon changed my mind! I did not feel intimidated and I was prepared to put up with anything providing I could leave

my job at the bank. Besides, even though Gini's fame was unquestionable for me I was still influenced by the fascination of Bonferroni's mathematics and the papers I had read by Gini seemed to me rather inelegant from a mathematical point of view. Obviously I was wrong! It took me some years to realize how ingenuous I was.

Above all, I was surprised by how quickly he made decisions. A few days after our meeting a letter arrived in Florence inviting me to become a temporary assistant of his. Basically it meant receiving a two-year grant (financed by the Confederation of Industry) which was integrated with the monthly salary of a temporary assistant, making a total payment of 60,000 Italian lire a month.

G.: Which topics did you begin to work on together?

B.: Complying to the reasons for which the Confederation of Industry had awarded the grant, I began work on the index numbers of salary purchasing power in various Italian municipalities. Gini supplied me with the essential bibliography and gave me precise indication of the sources of the data necessary for the research. I contacted the Institute of Nutrition for the foodstuffs basket (at that time foodstuffs affected the average family's balance much more than it does today), the statistics offices of various municipalities for the prices and the Confederation of Industry for information regarding salaries.

G.: What was Gini's attitude towards his collaborators and his students?

B.: As far as his collaborators were concerned (professors, assistants, clerks, ushers) I made some mention to that earlier. He did not even notice the professors of other subjects (mathematics etc.), they did their lessons and left. As for the university students, Gini was practically unapproachable. His assistants were considered like those of Karl Pearson, who R.A. Fisher [1956, p. 2] described as "an army of industrious robots responsive to a magic wand". Our timetable was from 8.30 a.m. to 1 p.m. and from 4 p.m. to 8 p.m. Once, during my first piece of work on the comparison between the purchasing power of salaries and wages, he went to Central America and on his return fifteen days later he checked the number of numerical elaborations I had made during his absence (the paper, later published in *Metron* [Benedetti, 1953] involved many numerical tables) and as he thought I had not worked enough, he threatened to sack me. He was not the type of person to threaten in vain! The idea of being unemployed, of failing and regretting having given up a secure, even if boring, job in the bank in Florence, took away all my peace of

mind. Luckily, he changed his mind and then satisfied with the theoretical results reached (theorems on non-circular indices etc.) he changed completely.

As far as lessons were concerned, I have to say that Gini very rarely did them because of his numerous commitments connected to his research activity (frequent trips abroad for conferences and scientific expeditions linked to the CISP etc.). During his absences his deputy dr. Stefania Gatti substituted for him both didactically and administratively. Although dr. Gatti had carried out quite a lot of interesting research work in the field of statistics, the results were kept in a drawer in her desk as she never had time to publish them, given that running the Institute took up all of her time. I will come back to this subject again later which, as you will see, deeply affects me.

Finally, on this point, I would like to mention a student (many had already graduated in Economics and Business Administration or Mathematics) who asked to do his thesis with Gini. He had to forgo the idea because he was completely discouraged by all the preparatory studies that were asked of him.

G.: Professor Benedetti, you had to leave the University because of the lack of posts and in 1954 went to work for the Confederation of Industry in the labour statistics office. What do you remember about this experience?

B.: The two-year grant from the Confederation of Industry foresaw the hiring of the grant-holder by this association if Gini's judgement was positive. It was and so I was taken on. However, it was a compulsory move for me because in the same year Gini was to leave his teaching post and it was Gini himself who advised me to go temporarily to the Confederation of Industry as his successor had already shown his intention to substitute Gini's entire team. Gini's deputy, Stefania Gatti, was to pay the highest price; after having sacrificed almost ten years of her life fulfilling burdensome didactic and administrative duties she was thrown out and found herself teaching in secondary school. At that time, in accordance with the law, those who after ten years had not been awarded the "libera docenza" (a qualification for university teaching) were downgraded. Therefore, dr. Gatti who had graduated brilliantly in mathematics with Luigi Fantappié and had been immediately engaged by Gini because of her gifts as a scholar, had to go to a secondary school in Frosinone to teach mathematics. You may understand from what I have said now and earlier that this affected me very deeply as my private life was also involved. Stefania and I were married in October 1955 and after a

brief period of happiness, depression set in which, following her interrupted academic career, began to sap her health. After five years of "highs and lows", Stefania died on 26th May 1960 leaving me a two and a half year old baby girl (Adriana).

G.: Was the period you spent at the Confederation of Industry in some way useful for your subsequent studies?

B.: The experience at the Confederation of Industry which began in August 1954 allowed me to study and write articles for *Metron*. Much of my research work regarding inequalities and that on theories and techniques of index numbers was carried out during that period. There was an excellent library as well as colleagues who I have very good memories of.

G.: Did your relations with Gini loosen?

B.: Relations with Gini never loosened. On the contrary he telephoned me continuously even at home at unearthly hours. Given my difficult family situation due to my wife's bad health and a baby girl, I was sometimes quite impolite but he was never offended. We now ran *Metron* together, I was the referee for many articles sent to the journal, I kept the correspondence with the authors and corrected the second proofs. There was only one old, grumpy usher (Capitoni) who, despite arguments and protests, never abandoned him.

Gini remained in the building in Piazza Esedra as it also housed his other "creation", the CISP. The other professors and followers had abandoned him once he had left his chair. Furthermore, Gini's successor and his team were already organizing the transfer of the Faculty to the University Campus (better known in Rome as "Città Universitaria") where, in a new building, there were large rooms and one was able to forget the subjection and restrictive circumstances of the Gini period.

G.: How did you succeed in going back to the University?

B.: Basically I did not belong to this new circle inasmuch as I was part of Gini's team. But Gini did not let go, in fact when, in 1960, his successor left the chair and was substituted by Vittorio Castellano, Gini used all his influence so that I was given the opportunity to join the Faculty again, even if it was only to hold a course on index numbers. We organized a numerous series of short theses on the Konüs index for the students. In that period Gini published my monograph *Theories and Techniques of Index Numbers* in *Metron* [Benedetti, 1962] and then also thanks to Castellano and in particular to Nora Federici, I became a member of the Institute of Statistics again. Nora Federici did even more: as she had

won a position as full-professor in demography, she asked me to substitute for her in teaching statistics at Perugia University.

I asked the secretary general of the Confederation of Industry if I could have two or three days off per week in order to fulfil this commitment but permission was refused. Therefore, at the age of 41 (in the meantime I had remarried) with two small daughters (Pieranna was born in 1962) I had to decide to leave a secure job for a position as temporary professor which, if everything went well, was renewable year by year. At the same time I was also offered a similar position at the Faculty of Statistical, Demographic and Actuarial Science of Rome on a diploma course (it was a kind of short degree) and so I decided to leave the Confederation of Industry. By the way, something else helped me to make my decision. Now and again at the above Confederation (which had its own social security fund) I was asked to carry out actuarial computations and therefore I was given an anonymous list of the staff's salaries. Despite the fact that it was anonymous I noticed that my salary was among the lowest. As a result on 1st November 1962 I began to commute between Perugia and Rome.

G.: Let's go back to the founder of the Italian statistical school. From 1970 onwards we saw, particularly on the part of non-Italian scholars, a considerable re-evaluation and extension of the concentration ratio (see, for example Giorgi, 1990, 1992, 1993) which Gini would never have imagined. How did he talk about his index and what was his attitude to critics?

B.: Even if he did not say so exactly, you could understand that he was proud of his index and he was annoyed or rather irritated when someone, for example Bonferroni suggested a similar index. As far as I am concerned, as I already mentioned in my paper *Counter - Examples in Statistics and Probability* [Benedetti, 1980], when I presented him with a general formula which contained, as particular cases, apart from the concentration ratio, also numerous other indices similar to his, he was furious. I did not dare approach this subject with him again and the manuscript containing the aforesaid general formula remained in a drawer in my desk until 1980, fifteen years after his death.

G.: What do you think about this index, even in the light of recent contributions?

B.: Now, looking back, it is easy to think of general formulae in which the concentration ratio makes up a particular case. This, however is a purely mathematical view of the problem. You must consider that

Gini proposed it in 1914. His unquestionable merit was that he was a pioneer in the following sense: he created a simple index, which was and, to a certain extent, still is today very useful for studying income distribution and wealth, linked to the impressive geometrical representation (i.e. the Lorenz curve) immediately giving a view of the concentration or inequality.

G.: Although he became famous internationally also for his index, Gini had made other original contributions, some of which did not receive the recognition they warranted. I am referring, for example, to the so-called "identities".

B.: Gini dealt with lots of other things apart from statistics, for example economics, demography, biometry but, in my opinion, apart from his concentration ratio and his "identities" (as Ragnar Frisch [1936] called them) in the field of index numbers his most remarkable work was the criticism he made with *The Dangers of Statistics* [Gini, 1939] of R.A. Fisher's fiducial methods and of all the Neyman-Pearson framework for testing hypotheses. This made him unpopular but it contributed considerably to discrediting the illusion of evading the bayesian reasoning.

G.: What other contribution of Gini's do you think was not correctly evaluated?

B.: His criticism of statistical induction that I have just mentioned. At the time when it was written it was not given due importance. Naturally his criticism of the superficial anglosaxon formulation of the problem threw water on the fire of the enthusiasm that often, even in Italy today, accompany everything which comes from abroad. I believe that the majority were not even aware of Gini's criticism. This can be seen from the way in which certain tests of hypothesis are mechanically used in various applications and how the relative responses are accepted, almost as if they were infallible oracles.

G.: Gini was President of ISTAT (National Institute of Statistics) during the fascist period (see, for example Leti, 1996), did he ever speak about the important position he held and about that period?

B.: No, he hardly ever mentioned it to me.

G.: Gini was married and had two daughters. How was it possible to live such a frenetic scientific, academic life (he had over 800 publications including articles, conference papers, books etc.) and have a family at the same time?

B.: When he was President of ISTAT, I was told that he stayed there until 8 p.m. and after he went to the Faculty until midnight and all of

the staff had to stay there with him (assistants, clerks, ushers). With these working hours I do not think he had much time to dedicate to the family.

G.: Professor Benedetti, you have worked both with Bonferroni and Gini. Could you tell us in what ways they were different scientifically as well as from a human point of view?

B.: They were two completely different people. Bonferroni (who, among other things, was a musician: he was an excellent pianist and composer) came from the conservatory of music and it was his father who made him enrol at the Mathematics Faculty in Turin. In his mathematical papers in fact you can recognize a lightness and refinement which is almost musical. He was a sensitive and kind person even if he was very strong willed. I was very fond of him, particularly for the way he was protective and affectionate towards me.

Gini was the opposite. Basically he was an intuitive person. He was not at all bothered by complex mathematical formalism and high-sounding names of some foreign scholars. He often succeeded in demolishing a very elaborate construction by making just one remark. However, he too made blunders, when, for example, he believed that the transitive test and the circular test were two different things. When Ragnar Frisch pointed out, in a private letter to Gini, that they were the same thing, Luigi Galvani was made to study the problem. Galvani had to admit that Frisch's statement was true. Generally speaking however, his insight succeeded where others floundered despite lots of mathematics. Apart from rare exceptions he considered mathematicians as extravagant people who had to be watched closely when dealing with a concrete problem. As far as his character was concerned, he was extremely hard. His attitudes showed courage but they were often unpleasant and certainly unpopular.

However, I did have the chance to know him in particular circumstances and can say that under that hard skin there was a certain dose of humanity. When my wife (Stefania Gatti) died he tried, at all costs, to occupy me with work and almost forced me to make a closing speech at the XX Scientific Meeting of the Italian Statistical Society in October 1960. I chose a subject (*Statistics of Statistics* [Benedetti, 1960]) which was a little strange and even though he was not very convinced he accepted it without discussion (quite unusual) because he wanted to keep me busy so that I did not think too much about my wife's death. On the whole, for him study and work were everything. Two days before he died, I had been to his house and he asked me if I could drive him (in my old

"Volkswagen beetle") to CISP in Piazza Esedra. After leaving him, as I was going down via Terme di Diocleziano I looked in my mirror and I saw, to my surprise, that he was waving good-bye to me. I never saw him alive again. When I returned from Perugia three days later I found news of his death.

5. Particularly significant relations with other scholars: Georgescu-Roegen and Konüs

Giorgi: Besides Gini and Bonferroni, you have also known both directly as well as through correspondence other important people including Georgescu-Roegen. Would you like to tell us something about him both as a person and as a scholar?

Benedetti: Nicholas Georgescu-Roegen is undoubtedly one of the most interesting scholars that I have ever met. I came to know of him through his work which appeared in the volume *Activity Analysis of Production and Allocation* [Koopmans, 1951] in which writings also appeared by George B. Dantzing, Paul Samuelson, Albert W. Tucher and others. I was particularly struck by his articles which appeared in this volume and when I received the off-prints of my paper on the inequalities connected to the range of statistical indices [Benedetti, 1957] I sent him a copy. Not long after, when Gini returned from a conference of the International Statistical Institute he told me that a foreign author had approached him saying that he had read my papers and gave him a manuscript for *Metron* [Georgescu-Roegen, 1959] which concerned my article on the aforesaid inequalities. I was pleasantly surprised to discover that it was no less than Georgescu-Roegen who I had admired for some time. Gini surprised me by saying "be careful, you can't trust these Levantines". He left me speechless! Going back to the above-mentioned work by Georgescu-Roegen, it concerned very elegant geometrical proof of my inequalities. From this article I learnt some important topological notions on convex polyhedral cones which immediately allowed important results to be reached. Something which was difficult using other methods. Some months later Georgescu-Roegen came to Rome and Gini invited us both to his house. On that occasion he realized the calibre of that mathematical economist of Roumanian origin. He had graduated in mathematics at the University of Bucharest and then with a scholarship he had studied with Karl Pearson in London and Emil Borel, E. Goursat, H. Lebesgue, M. Fréchet and

G. Darmois in Paris. After that, the Rockefeller Foundation gave him the opportunity to go to the United States and at Harvard he met Joseph A. Schumpeter who opened the horizons of economics up to him. This happened between 1934 and 1936. Then, before the second world war, he went back to Roumania where he held important positions in the state administration. Immediately after the war he was Secretary General of the Armistice Commission and was in contact with the Soviets who accused him of siding with the west. He preferred to escape from Roumania (in rather adventurous circumstances) and returned to Harvard where he had already made friends with Edward S. Mason and Wassily Leontiev. If you read the preface of his book *Analytical Economics* [Georgescu-Roegen, 1966a] by Paul Samuelson you will get an idea of the value of this man. He returned to Rome in 1966 for a meeting in honour of Gini a year after his death and then again for another conference on Pareto. On these occasions he came to my home with his wife Otilia and we became friends. He was a refined man with lots of interests. He told me, for example, that despite the hardness and discipline that existed in the Galton Laboratory directed by Karl Pearson, he was invited by Pearson himself to his country house outside London. When Georgescu-Roegen arrived at the station, Pearson was there waiting for him and, although he was more than seventy, he insisted on carrying Georgescu's case who, at that time, had not reached twenty-six. He considered Karl Pearson his master in philosophy as well as in statistics (naturally he was referring to the *Grammar of Science*) while Joseph Schumpeter was his master in economics.

Georgescu-Roegen was never afraid of going against the current. When, for example, in 1966 he came to Rome to the memorial for Gini by referring back to one of the Italian scholar's papers (*The Disappointments of Econometry* [Gini, 1956]) he spoke about the inadequacies of econometric models for predicting economic futures [Georgescu-Roegen, 1966b]. That was, according to the Roumanian scholar, his "death sentence as a fellow of the Econometric Society". In fact on several occasions he received clear messages of ostracism from econometricians (see Georgescu-Roegen, 1992, pp. 156-157). However, his work, beginning with the famous one which appeared in 1936 in the *Quarterly Journal of Economics* on the pure theory of consumer's behaviour [Georgescu-Roegen, 1936] to his last book *Entropy Law and Economic Process* [Georgescu-Roegen, 1971] show without any doubt, his high level as a scholar. He was a very cordial and generous person. I remember that during the conference on Pareto in Rome he invited me to dinner to introduce me to the Nobel prize

winner John R. Hicks but I was too shy and I found an excuse not to go. After the ceremony in which he was awarded an honoris causa degree in Economics in Florence in 1982, I did not meet him again.

G.: What can you tell us about the Russian scholar Konus?

B.: I corresponded with A.A. Konüs for about 25 years, but we never met each other! For me this type of contact was ideal (as I mentioned in the article in *Metron* [Benedetti, 1988] in which I published a collection of a large part of this correspondence) and I think it was the same for him because he was quite an age (1895-1990) and also because up until a few years ago citizens of that country were not allowed to travel easily. We also invited him for Gini's memorial in 1966 but he replied very evasively and with obvious embarrassment. However, as you can understand from the correspondence in question he was a kind person and always willing to discuss index numbers, probability and economics. He had a considerable culture, his mathematics were a little "dated" but he was always accurate and succeeded in solving the problem which interested him. In his long life he met many famous scholars. He worked with Eugene E. Slutsky at the Institute of Economic Conjuncture directed by Kondratiev and was in contact with Kolmogorov, Gnedenko etc. I was very upset by his death in 1990. It was the end of a period of exchange of ideas, and interesting impressions with a first class scholar born in the last century in Tzar Russia who had lived through the turbulent periods of the first world war and the Bolshevik revolution.

6. Some further comments

Giorgi: Professor Benedetti, I know you are a reserved person but I would like to ask you some other questions which involve you directly. You have made numerous and original contributions on index numbers and your monograph on *Theories and Techniques of Index Numbers* [Benedetti, 1962] is still today, 34 years since it first appeared in literature, an essential point of reference for scholars in this field. Your particular interest for the subject is also shown by the correspondence between you and Konüs. To what extent did your contact with Gini influence your research work in this field?

Benedetti: Gini certainly influenced all my work on index numbers. As I went into this topic further, I had to recognize that his article on the subject which appeared in *Metron* [Gini, 1924] was a mine of ideas, some

of which were concealed. In our correspondence, Konüs himself recognized that Gini had preceded him in the economic theory of index numbers. It was not easy to discover the meaning of some ideas roughly sketched in that complex paper. As far as I am concerned I am rather proud of having shown (apart from that theorem on the contradictions of non- circular index numbers) within a topological framework in the space of prices, based on the transitivity and asymmetry axioms, the need for a non-contradictory ordering of the price-points in the above-mentioned space and that proved the need for the circular test. However, even when I realized that from Gini's identities, through the assumption of a generic homogeneity of the utility function, Konüs' index could be obtained immediately, I felt great satisfaction. Gini, maybe distracted by other things, or maybe because he was not very familiar with Eulero's theorem on homogeneous functions, had not noticed it. We should remember that Gini graduated in law and even if he followed a course on mathematical analysis at the University of Bologna his nature caused him to avoid pure mathematical technique.

G.: Some of your results on index numbers were rediscovered a few years later by Anglo-Saxon scholars as has been shown also by other authors [Biggeri *et al.*, 1987]. Could you tell us about some of the most significant aspects of your contribution in this area of research.

B.: To be honest, after my work in 1972 I did not deal with the topic again if not only indirectly (see Benedetti, 1989a) and I did not take any further interest in the relative literature. When the paper by Biggeri *et al.* [1987] was published some mention was made to my results rediscovered in a more complicated way by Diewert [1976] some years later. I tried reading an article by this author in *Econometrica* [Diewert, 1978] but I was immediately discouraged by the complicated mathematical formulation even for simple concepts and I gave up trying to understand.

The core of my aforesaid work [Benedetti, 1972] consists in observing that even the old price indices, (Lowe, Dutot, average geometrical index, power general index etc.) can be considered modern constant utility functional indices and that for each of them conversion formulae are supplied which allows each of their corresponding utility functions to be determined. Criteria for accepting or refusing these indices on the basis of the required properties for the above-mentioned utility functions they implicitly contained also resulted.

G.: Your results regarding certain inequalities connected to the range of statistical indices [Benedetti, 1957] have also recently been rediscovered. Could you talk about them for us?

B.: Well, they were rediscovered by two Chinese scholars from Taiwan, [Hwang-Hu, 1994], who with more complicated methods attained some of my results which appeared in *Metron* in 1957. I found out by chance while I was looking through *Statistics & Probability Letters* and I immediately wrote to both the editor of the journal (who has never replied) as well as to the authors themselves who kindly replied apologizing and promising a suitable correction in the journal, but up until today nothing has appeared. In the meantime Mario Badaloni, editor of *Metron*, has of his own free-will republished in English the old 1957 paper. I believe the results to be quite important inasmuch as they prove that Bravais' correlation coefficient cannot be a concordance or discordance index as I have written many times and has been clearly pointed out by T. Gastaldi [1990] because it can be almost zero even if there is perfect concordance or discordance.

G.: Apart from those already mentioned, your studies have also been extended to numerous other topics (see Benedetti, 1996). In particular I would like to remember your book *Institutions of Statistics* [Benedetti, 1989b], the first edition of which dates back to 1975, which, in my opinion, characterizes your statistical way of thinking. What can you tell us about this?

B.: Many topics appeared in my *Institutions of Statistics*. I would also like it to be thought of as a book for consultation especially if you consider "Complements and Exercises". In the book there are almost all the results of my studies, from index numbers to curve and trend fitting (polygonals, time series etc.) to correlation (various inequalities etc.), to probability and the history of statistics. If a student in Statistics does not get rid of it before graduating, he could find it useful for further study in other subjects taught in Statistics Faculties.

G.: For ten years (1981-1990) you were editor of *Metron*, the journal founded by Corrado Gini in 1920 and you collaborated on the journal at various levels practically since your arrival at Rome University. Therefore you have had the chance to examine numerous articles by Italian and foreign scholars. How have you seen research in statistics evolve?

B.: You could say that the only position I have held during my academic career is that of editor of *Metron*. In 1981 I tried to win my usual reluctance and took on the direction of the journal to which I had dedicated lots of time and interest since 1952. In the end I felt morally obliged to become editor. During this period statistics certainly modified further up to the point of exasperation in which for someone from my genera-

tion it became almost unrecognizable from a mathematical point of view. May be, when we were young our work made the same impression on a scholar from Gini's generation. One thing is certain, everyone belongs to his own time and we must have the courage to break away from things when novelties of time impose it upon us.

G.: Finally, to conclude this conversation, what advice would you give to a young person beginning a university career in statistics?

B.: I would advise him/her to read the authors from the past. For me the history of statistics should be a fundamental subject in a Faculty of Statistics. Young people should know how certain problems arose and what the relative solutions were so that after if they want to venture into various areas of research they will be able to use previous results which will not take anything away (on the contrary it will act as a reinforcement) from the satisfaction of adding something new to what already existed.

G.: That means that by studying the history of statistics the periodical rediscovery of already known results could be avoided?

B.: Exactly!

G.: Professor Benedetti, I would like to thank you for having granted us this conversation and for having thrown some light on the lives of some great scholars. You have helped us to understand better their personalities and mentalities which allowed them to reach considerable heights and not only from a scientific point of view. I would also like to hope that in the future you will also support us with your advice and experience. I wish you all the best in your retirement.

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Encounters with the Italian Statistical School: a conversation with Carlo Benedetti

SUMMARY

Through a conversation with Carlo Benedetti, the author aims to set some famous scholars and their scientific results in the period in which they lived. He does this by out-

lining the principal scientific and human characteristics of the scholars with whom Carlo Benedetti collaborated directly (e.g. Gini and Bonferroni) or with whom he had many contacts and exchanges of ideas (e. g. Georgescu-Roegen and Konüs).

Incontri con la Scuola Statistica Italiana: una conversazione con Carlo Benedetti

RIASSUNTO

Al fine di inquadrare alcuni famosi studiosi ed i loro risultati scientifici nel periodo in cui sono vissuti, l'Autore cerca di delineare, attraverso una conversazione con Carlo Benedetti, le principali caratteristiche scientifiche ed umane degli studiosi con cui quest'ultimo ha collaborato direttamente (Gini e Bonferroni) o ha avuto proficui contatti e scambi d'idee (Georgescu-Roegen e Konüs).

KEY WORDS AND PHRASES

Scientific and human characteristics of Gini, Bonferroni, Georgescu-Roegen and Konüs.

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