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SOME PERSONAL RECOLLECTIONS OF THE DEVELOPMENT OF STATISTICS AND PROBABILITY[1]

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Foreword. The Department of Statistics of the University of North Carolina at Chapel Hill was honored to be host to Professor Harald Cramér for two weeks in the early part of March, 1970. On two occasions, Professor Cramér generously agreed to record some of his personal reminiscences of the development of statistics and probability. These *are* purely personal reminiscences and they should not be taken as a complete historical account.

The recordings of Professor Cramér were transcribed and edited by Edward J. Wegman of the Department of Statistics of the University of North Carolina at Chapel Hill. The work of Professor Wegman was supported by National Science Foundation Grant GP-23250.

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Recollections of Professor Cramér.

At this time, I propose to give you some personal recollections from the early development of probability theory and mathematical statistics. They will be purely personal recollections. I am going to speak of some of the people I have known who have played a great part in that tremendous change in the field which you all know has taken place in the last fifty years. It, by no means, will be a complete historical account, just some sketches and personal recollections. I'm taking dates, names and developments straight out of my memory without having much time to check my statements so I have to apologize beforehand for any incorrect statement that I might make in what I'm going to say. Perhaps I might start by saying a few words about my personal background to show you how I came to be interested in probability and statistics at a very early date.

I got my Ph.D. degree from Stockholm University in 1917, more than fifty years ago. At that time, I had written a thesis about Dirichlet's Series and my interests were purely mathematical. I was working with the analytic theory of numbers, particularly with prime number theory and Riemann zeta function theory and such things. There was always the problem for a young mathematician of that generation to get a job in order to be able to support a family. University jobs were few, there were only a small number of full professorships, very difficult to get and entirely beyond the reach of a young fresh Ph.D. There were several subordinate jobs, but very few and very badly paid. To become a school teacher was not considered very attractive. There were comparatively few and not very well paid jobs in that line and a great number of candidates. You had to wait for many years before having any chance of getting a better paid job. However, there was one line that was open to mathematicians that offered well paid jobs, jobs within a comparatively short time. I noticed a sign outside your department office entitled "An Actuary - What's That?". That was the question that you had no need to put to a young Swedish mathematician of my generation. It was well known to us all that there was a tradition of Swedish insurance companies to employ high standard university mathematicians as actuaries and to offer them well paid positions. Many mathematicians of my generation took that line, especially from Stockholm University since all the insurance companies were situated, more or less, in Stockholm. I followed the stream and got at a comparitively early age a job as an insurance actuary. It was in this way through my practical work as an actuary with probability problems and statistical questions that my interest was directed towards probability and statistics. I soon began to feel that there was a change coming in these fields, there were many new ideas breaking through and it seemed to be a promising field for a young research worker.

At that time, the time when I got my degree in 1917 and the immediate following years, the situation both in probability and in statistics was entirely different from what we know today. Probability theory was hardly a mathematical discipline. There was the great fundamental work of Laplace, but since his time there had been a great development in the standards of mathematical rigor and the work of Laplace was entirely nonrigorous from our point of view. Later there had been some isolated brilliant works, many by Russian mathematicians, people like Tchebychev, Markov, Liapounoff. Still there didn't exist any organized theory. The situation was so badly felt that about 1920 Von Mises in course of an attempt to lay better foundations for probability theory, made the statement that today probability calculus, as then it was called, is not a mathematical theory. The change was imminent.

In statistics, the situation was not very different. In Sweden and on the whole in continental Europe, statistics was very different from what we know today. It was something connected with state science and consisted largely of accumulating a lot of data concerning demographic and economic facts and of arranging them in tabular form; no scientific theory or practically none was behind it all. In England, we had the biometric school headed by Karl Pearson but their mathematics didn't appeal very much to us at that time. We thought it was entirely unrigorous and directed more towards descriptive statistics, especially in biometric fields, than statistical inference. Hardly any theory at all of statistical inference existed at that time.

My own experience in the development in probability and statistics set in about the same time I got my actuary job. At a very early stage, I came into contact with some papers on what we now call "risk theory" written by the Swedish actuary F. Lundberg; papers that were very difficult to read and understand, but that we can now recognize as forerunners at a very early date of the theory of stochastic processes as we know it today. This man in his thesis at such an early date as 1903 used and described what we now know as the Poisson Stochastic Process. He later developed the theory of risk in an insurance company which we now look upon as the theory of a particular kind of stochastic process with independent increments. At that date, these papers and the ideas contained in them were not generally understood and during the 1920's we had to try among Swedish actuaries to work out these ideas and to bring them into contact with all the new developments that were taking place in probability.

At another comparatively early date at a congress of Scandanavian mathematicians in Helsinki, Finland in 1922, I met the mathematician, Lindeberg, who we all know is the author of the famous Lindeberg conditions in connection with the Central Limit Theorem. I think that my acquaintance with him was largly responsible for my growing interest in the Central Limit Theorem and all connected probability theorems. Lindeberg was a wonderful person. He acted as professor at the University of Helsinki and, at the same time, he had a farm very beautifully situated in eastern Finland. When people reproached him for not doing enough mathematics, his answer was, "well, I'm really a farmer", and when he was reproached for not cultivating his farm very properly he said, "well, of course, my real job is to be a mathematician!". Anyhow, he made this great contribution to probability theory, the Lindeberg conditions, which were at that time regarded as sufficient conditions for the validity of the Central Limit Theorem. As you will all know, more than ten years afterwards Feller and others proved that under certain additional conditions, the Lindeberg conditions are also necessary for the validity of the Central Limit Theorem.

I should say already at an earlier date, 1920, during a stay in Cambridge, England, I made a contact which has since been very interesting to me to have. I met in Cambridge a young American mathematician called Norbert Wiener, about the same age as myself. Since that date, we have kept a certain contact all through our lives until his sudden death in Stockholm during a visit only a few years ago. Of course, it was only a couple of years after our first meeting in Cambridge that he published his famous paper concerning the Wiener measure, what we now know as the Brownian movement process. For me, it was very interesting to have this contact and to be able to follow the works of Wiener.

In 1925 an event occurred which came to me as a revelation, and that was the publication in 1925 of *A Treatise on Probability* by Paul Lévy, in France. I think that was perhaps the first time that there appeared a treatise on probability satisfying, at least approximately, the standards of modern rigor. The measure theoretic approach was not yet fully developed but you may say that Lévy had it at least intuitively. If you look at his old book of 1925, you can see that there is the measure theoretic view of the subject, so to speak, behind the lines. But there were, of course, ... still some years to pass before full clarity was gained into the real nature and background of probability theory. However, during the 1920's the mathematical techniques in probability, which we know today, for example the technique of characteristic functions, were gradually developed so that about 1930 these techniques were fairly well known by everybody interested in the subject.

In statistics, the 1920's also were remarkable by the coming forward of new ideas. There were the numerous works of R. A. Fisher and his collaborators and immediate followers, works on multidimensional distributions, correlation and estimation. They were admirable works but still not quite satisfactory from the point of view of mathematical rigor. At a somewhat later stage, towards the end of the 1920's, there began to appear the famous papers of Neyman and Egon Pearson and then you all know the development that was taking place during the 1930's. I should say, perhaps, that I personally never met Karl Pearson. I met his friend and collaborator William Elderton who became one of my greatest friends. At a somewhat later stage, I made the personal acquaintance of R. A. Fisher who was, as you perhaps all know, certainly a character! He was very kind to me and I visited him in England on several occasions, and perhaps, if time permits, I will tell you later of an encounter with him in Paris which was quite interesting. The remarkable thing in statistics, about the turn of the decade between the 1920's and 1930's, was the appearance of the Neyman-Pearson ideas and various works of their immediate collaborators such as Harold Hotelling and many others.

In probability, the first years of the 1930's signified a real breakthrough and the beginning of a new era. There were the works of Paul Lévy, whom I have already mentioned, and above all the works of the Russian mathematicians Khintchine and Kolmogorov. They each published in 1933 a small volume translated into German (the Russian original was a couple of years earlier). These volumes, particularly that of Kolmogorov, were remarkable. Kolmogorov gave an entirely new approach, an approach which had existed as I said before, behind the lines of several works of various authors, but he gave a connected and well-organized account of this new approach through measure theory. This breakthrough has since served as a foundation of the development which has since then taken place in pure probability theory. At that time, we had in Stockholm University a small probabilistic group interested in this theory, and to us the works of Kolmogorov, Khintchine and Paul Lévy came as revelations. We were interested particularly in fields like the theory of characteristic functions, and their applications to the Central Limit Theorem, and to the various expansions in series starting from the Central Limit Theorem which are largely used for statistical purposes. We were also interested in the development of risk theory. So far, until sometime in the 1930's, this risk theory, built on the early works of F. Lundberg, was more or less a Swedish field. There were very few writers outside Sweden working in this field, but in our Stockholm group we tried to develop this theory and to build it on the foundations laid by Kolmogorov, Khintchine and Lévy.

A remarkable event occurred to us in 1934. This was in the bad days of the Nazi regime in Germany. Will Feller, who was employed at a German university, was driven out of Germany, and through the intermediary of my very good friend Harald Bohr in Copenhagen, came to Stockholm in 1934 where he stayed on for five years working mainly with our Stockholm probability group. This, of course, was a God-sent thing for us and all of us were very grateful for this opportunity to collaborate with Feller. It was during his

Stockholm years that he published his works on the Central Limit Theorem and on Markov processes, the latter work built on another paper published by Kolmogorov about the same time, perhaps 1933 or 1934, giving the foundations of the theory on what we now know as Markov Processes. Feller wrote a paper completing the Kolmogorov paper which has since been followed by many other papers both by Feller and various Soviet mathematicians in the same field. He stayed on in Stockholm until the summer of 1939, just before the outbreak of the war, when he got a job in the United States and left us, to our great regret. As time went on, we thought it was better for him to be in the United States than to be more or less a refugee in Sweden.

During the 1930's, (I'm still talking about the development from the point of view of the Stockholm probabilistic group) we had intimate contacts with several of the people I have now mentioned, such as Khintchine and Kolmogorov. Personally, I never met Khintchine, but I met Kolmogorov. I met him many times and had opportunities to talk probability with him, opportunities which I treasure very highly. Kolmogorov is, without any doubt, a great man. A man who impresses you as a most intelligent man, and a leader in the group of Soviet probabilists. We also had intimate contact with the French mathematicians, people like Fréchet and Lévy. From my point of view, I should like to say perhaps that, between these two there is a great difference. Fréchet is a brilliant mathematician, who did outstanding work in fields like functional analysis. On the other hand, Paul Lévy, of course, is responsible for a great number of the new and really successful ideas which have come forward in probability theory.

I was invited in the spring of 1937 to give some talks in Paris. I had met Fréchet before, but this was my first encounter with Paul Lévy. He greatly impressed me. The situation at that time was that Paul Lévy had published a conjecture about the normal distribution function, to the effect that if a normally distributed random variable is the sum of two independent random variables, each of the two must be normally distributed. He said several times that he was unable to prove that conjecture. I succeeded in proving it and, of course, was then invited to Paris to talk about these things. I was very well received by Paul Lévy and had many very interesting talks with him. At the same time, I met some of the young French mathematicians, people like Loève and Fortet, names which are very well known to all of you. They were young students in Paris at that time. I met them both in Paris in the spring of 1937 and later on at the conference in Geneva in the fall of 1937.

At the Geneva conference I met Neyman for the first time. I had had some correspondence with him, and had studied his works, published partly independently and partly jointly with Egon Pearson. They had impressed me very much, but at that time they were still somewhat difficult to understand. They were written, so to speak, in a tone between the old and new school in statistics. They were written more or less in the frame of the old Pearson theory, but the ideas were definitely modern, definitely new. That made the papers very difficult to read and understand. At that time, the Neyman idea of confidence intervals was something quite new and unheard of. I had read about this in some of the papers which Neyman had sent me, and at the Geneva conference in the fall of 1937, he was going to give a talk about these things. Unfortunately, I was acting chairman of the meeting where he was giving his talk. My French has always been rather poor, and in the middle of his talk Neyman was interrupted by Fréchet and Lévy who wanted to discuss and critize Neyman's ideas. I had to use my poor French in order to calm them down and let Neyman finish his talk and have the discussion later. Fréchet and Lévy I don't think were at all convinced that there was something really valuable in Neyman's ideas. I remember him asking me personally what I thought about the subject and I believe I gave a sensible answer that I think this is something really valuable but not quite clearly expressed. In the course of time, the Neyman ideas were more clearly expressed, and soon people were able to grasp their contents and appreciate their great significance.

I should mention also another name, the Italian mathematician Cantelli, with whom we also had contact in our Stockholm probabilistic group. Cantelli, like myself, was also working as an actuary. As a matter of fact, he had been, among other things, the actuary of the pension board of what was then called the Society of Nations in Geneva. When he resigned this position, I became his successor. That gave me a contact with Cantelli which I value very much. You know his name from the Borel-Cantelli condition. He had written several very valuable papers on probability, papers which have perhaps not received quite the attention that they really do deserve. He was a very tempermental man. When he was excited, he could cry out his views with his powerful voice: a very energetic fellow. The last time I was in Rome, a couple of years ago, he was there now as a very old man. He couldn't come to any meetings, but I had a short conversation over the telephone with him and we remembered together the good old days.

Another event that occurred during the 1930's was the publication of a paper by Khintchine in the *Mathematische Annalen* in 1934, I think; a paper on stationary stochastic processes. That was the first time that one heard the name stationary stochastic processes. And Khintchine, in his paper, introduced the subject and proved some of the fundamental propositions of the theory.

In our group in Stockholm, he was followed by Herman Wold who took a great interest in this Khintchine paper on stationary processes and who wrote, in 1938, his thesis on stationary processes. I think it has appeared in several editions and is, I believe, quite well known. We are now approaching the time of the war. Shortly before the war in July 1939, there was another conference in Geneva, a conference of a more statistical nature, where I saw again R. A. Fisher (whom I had encountered several times before). This was my first meeting with people like Sam Wilks, Maurice Bartlett, and several others. I think Wilks, Bartlett and Fisher were my three main contacts from this Geneva meeting. But at that time, one had the feeling that something bad was coming. And all the way home through the continent it was more and more evident that something was coming. Wilks came with his wife for a short visit to Stockholm, passing through Germany, and I remember him saying (that was in the early days of August 1939), "The Germans are out for something but I don't know what". Of course, it was only a few weeks later that it became all too evident what the Germans were out for. After the outbreak of the war, of course, all these international contacts were cut off. This cut-off lasted all through the war years. In Sweden, we managed to stay out of the war, but Norway and Denmark were occupied by the Nazis. All communications to the West were very difficult to maintain. We couldn't get scientific papers, for instance, and we felt entirely cut off from the international developments. I thought I would use this forced isolation to do something useful. I started writing a book. I used to say to my wife that this book would be my entrance card to the new world after the war.

After the war, there was an entirely new era. This early stage of development of probability and statistics was passed. A new era had set in and new people came upon the scene. Right after the end of the war, I think it was only a year after, in the spring of 1946, I was invited to Paris to give some talks. I had been working hard with the Fisher and the Neyman-Pearson papers and all the new developments in statistics. I had worked out my ideas as regards concepts such as confidence intervals, fiducial probabilities and so on. I proposed to give a talk about these subjects. When I was writing some things on the blackboard before the lecture, who should enter but R. A. Fisher. (I had no idea he was in Paris.) This was not very agreeable to me, since I had planned to be rather critical of Fisher in my talks. I felt rather unhappy! But I gave my talk and gave statements as guarded as I could without holding back what I thought. After the talk, R. A. Fisher came up to me and I was very anxious to hear what he would say. He said simply that his French was not so good and that he had not been able to understand me, but he would like to talk with me privately about these subjects and listen to what I thought about it. Now, in Paris one year after the war, it was very difficult to find a decent eating place. I knew Paris quite well and I was able to take Fisher to quite a nice restaurant where we had a delicious meal together and talked. He took everything very gracefully. When we separated that night, he said that there was always a berth waiting for me in his college. I think that it was quite a happy ending.

But then there was something more. Leaving the mathematical institute where the lectures were held, one day I encountered a man whose face seemed familiar, that was Neyman. Apparently he was on his way back from a mission in Greece. He stayed on in Paris for a few days and of course I was very glad to see him again. We were all invited to a reception in the house of Emile Borel which was quite a memorable event because Neyman and Fisher were under the same roof for an hour! I watched them but I couldn't see any signs of violence of any kind!

In Paris, of course, I saw my old friends Fréchet and Lévy and also saw again some of the younger school of French mathematicians, people like Loève and Fortet. It was clear that these people were doing quite important work and that they were starting a new line of development, particularly in the theory of stochastic processes. During the war years, we had been quite isolated in Sweden, and, as I said before, I tried writing a book on the mathematical methods of statistics. I tried to connect the different lines of development of probability theory on one side and in statistics on the other and tried to show how the methods of mathematical statistics could be founded on a well organized mathematical probability theory. This book was hardly ready when I received the invitation to come to Princeton in the fall of 1946. This was my first visit to America. I was to stay in America for a whole year, first one term in Princeton, then one term at Yale and brief visits in the fall term at Chapel Hill and during the summer of 1947 at Berkeley. Of course, I had the occasion to meet, during this prolonged stay in the United States, quite a number of interesting people. At Princeton, I saw my old friend Sam Wilks whom I had already met in Geneva right before the war. He was the man responsible for my coming to Princeton and I was working in his department where I gave lectures to various people who have since been outstanding people in the field of probability and statistics. As my assistant, I had a young Chinese called Chung (a name which is now well-known as the name of an outstanding professor at Stanford). Another Stanford professor, who was among the audience coming to my Princeton lectures, was Sam Karlin. During my time in Princeton, where I gave lectures mainly on an introduction to the theory of stochastic processes, I also saw again my close friend Will Feller. When he left Sweden in 1939, he had first come to Brown University and after some time there he transferred to Cornell University in Ithaca where I went to see him. At Cornell, there was a very nice group of mathematicians and I spent a short time with all these people. I was happy to see my friend Will Feller again and to see how well he had gotten on in America. However, I soon returned to Princeton to follow up my lectures there and near the end of the term, I was invited to Chapel Hill where I stayed only for a week.

This was my first visit to Chapel Hill, but it was by no means to be the last. It already gave me a strong impression of the good work that was being done here. The people that received me here were Professors Gertrude Cox and Harold Hotelling. Gertrude Cox was then teaching in Raleigh and Hotelling in Chapel Hill. I gave a couple of lectures in Chapel Hill where I met, among others, Herbert Robbins and the Chinese P. L. Hsu, who had made some important contributions which interested me very much, such as analogues of the Central Limit Theorem.

For the spring term of 1947, I went to Yale and again I lectured about the introduction to stochastic processes and also I gave some elementary probability lectures for undergraduates. Among the statisticians I met in Yale was Chester Bliss, who has since become one of my great friends. For the summer of 1947, I was invited for the summer term in Berkeley where I saw again my old friend Neyman and a group of his students, such as Eric Lehmann, Joe Hodges and Betty Scott. They gave me a strong impression of the powerful work that was being done by the Neyman group in Berkeley; work which has still been going on, taking form in the Berkeley Symposium. I think that one of the greatest things that my friend Jerzy Neyman has been able to do is that he could start, organize and follow up these symposia.

It was clear that the theory of stochastic processes was rapidly developing. That it was becoming more and more important from a purely theoretical point of view as well as for a steadily increasing number of applications in various fields. I think that it was on my way to Berkeley that I saw Joe Doob in Urbana where I spent a couple of days discussing stochastic processes with him. Also, of course, during these years, Feller continued his interesting work,which had started during his time in Stockholm, on various problems connected with stochastic processes. He also did, during this time, interesting work about the law of the iterated logarithm. He succeeded in finding necessary and sufficient conditions which are described in an outstanding contribution.

In our little Stockholm group, we had continued working in the field of stochastic processes. Particularly, we had followed up our work on the applications in insurance and risk theory of stochastic processes and tried to develop this into a rigorous mathematical theory. Towards the end of the 1940's, it was evident to me that one of my students was to be an outstanding man in the field of stochastic processes. His name is Ulf Grenander, and in 1950 he wrote a thesis concerning statistical inference in stochastic processes. I think it's fair to say that Ulf Grenander was a pioneer in this field which has since been the object of so many important works by a great number of authors, including recent important contributions by Grenander himself.

About 1950 I became heavily engaged in university administration. I held two different jobs in university administration during the eleven years, from 1950-1961; first, with the University of Stockholm (my old university), then for a short number of years I had a job in the joint administration of the Swedish universities. This, of course, meant that I had very little time left for research. Still I was able to take a leave of absence for one term in 1953 when I went to Berkeley. There I had a very interesting full term, meeting many old friends, making new ones, and lecturing as before about stochastic processes. Also during these years in university administration work, I happened to be invited, on the strength of my job at Stockholm University, to the bicentennial of the University of Moscow in 1965. This was my first visit to Moscow and I had occasion to meet personally many of the Soviet mathematicians, most of whom I already knew from the literature and by correspondence, but many of whom were new acquaintances. Khintchine I would like very much to have known but at this time he was seriously ill and died only a couple of years afterwards. In any case, I had made my first personal acquaintance with the work of Soviet mathematicians and statisticians, an acquaintance which has given me great admiration for the work these people are doing. Soon after this first visit to Russia, I was informed that my Soviet friends were going to start a new journal - *Probability Theory and its Applications*. This journal has since then developed and has contained, in practically every one of its volumes, many outstanding contributions to the development of our science.

I shall stop my recollections now. As I said from the beginning, it has only been some scattered personal recollections arranged according to my own personal memories and by no means a complete account. I have had occasion to mention some of those people who have played a part in those tremendous changes in the last fifty years.

Thank you.