*Известия НАН Армении. Математика, том 44, н. 1, 2009, стр. 25-34*

# THE ARMENIAN CONNECTION: REMINISCENCES FROM A TIME OF INTERACTIONS

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This is a narrative of personal reminiscences written largely, perhaps excessively so, in the first person singular. Such a practice is to be frowned on in the staid world of mathematics and is liable to expose the writer to the charge of self-indulgence but I cannot help choosing this as my way of paying tribute to Klaus Krickeberg's indomitable spirit and mathematical enterprize, since our paths were intertwined, crucially for me, at one stage four decades ago. I cannot think of a better way of putting on record my great indebtedness to him and I must beg the reader's tolerance for the strong autobiographical element of these notes. They should be read as a memoir of random interactions involving Klaus and not as a contribution to scholarship or to an appreciation of his wide-ranging work.

Encouraged by Hans Zessin I have put a particular focus on a sequence of events around 1970 that led to some fruitful developments on the border between geometry and probability, which attracted the interest of others in the years that followed. It may sound strange but Klaus and I never actually worked 'together' on this border. However, as the story will reveal, we were for a while just one step removed, and there was a fortuitous encounter in 1971 which proved opportune.

I first met Klaus in 1962 when he visited Greece to participate in a symposium organized by D.A.Kappos, with whom I was working at the time at the University of Athens. Klaus was a key participant but I remember that he did not confine his interest to the symposium alone; true to his perennial eagerness to get to know peoples and cultures, he crowned his visit with a trip to the countryside and among other things familiarised himself with the Greek Easter celebrations and the solemn as well as the noisy manifestations that go with them. I imagine this must have been the time he took his first steps in learning Greek (he was already master of several languages) and sharpening his taste for Greek holidays, which he indulged in more recent years.

Some time later I received an invitation from him to go and work with him for a couple of years at the University of Heidelberg where he then was.

I accepted conditionally: at that stage I was committed to going to the USA, where I was to spend the years 1963-65 and it was only after this that I was able to join him at Heidelberg during the years 1965-67 as an Alexander-von-Humboldt scholar.

As it happened, just prior to this Klaus had a visiting post at Columbia University in 1964-65 and since I was in Washington D.C. at the time we kept in touch, mainly by correspondence but also through a trip my wife and I made to New York where we were guests at the Krickebergs' for a few days early in the academic year and enjoyed their marvelous hospitality. Klaus was at that point interested in transformations that 'mix' infinite measure spaces and this led to some very interesting connections with strong ratio limits of Markov chains, an area in which John Kingman, Steven Orey and William Pruitt were making contributions at about that time. Klaus's paper on the subject was presented at the Fifth Berkeley Symposium on Mathematical Statistics and Probability in the summer of 1965. My own paper on the subject was written later while I was in Heidelberg and owed much to questions raised by Klaus, who had a sure touch in knowing exactly what questions to ask. It was another of his questions, on isomorphisms of topological measure spaces, that led to some joint work between Klaus, W.B"oge and myself.

My two years in Heidelberg were unforgettable, and they were decisive for all subsequent development of my work as they sealed my Pauline conversion to probability, which I had previously shunned. Not all the work I did in Heidelberg was directly related to Klaus's but I have no doubt that it would not have been done had I not been there. Our mathematical group was then housed in an old annexe and it was only near the end of my stay that the department moved to its grander, more modern accommodation. We had the benefit of numerous visitors and the Krickebergs' hospitality was boundless as ever.

When I left Heidelberg in the summer of 1967 Klaus had, as far as I know, not yet turned to Stochastic Geometry as such (or Geometric Probability as I ought to call it in the context of that early stage), although he had always been interested in things geometric. This was certainly true in my case. The stimulus for me came later from Cambridge and had a name: Rollo Davidson. Rollo was to die tragically in a mountain climbing accident in 1970 but not before he got both Klaus and myself interested in his intriguing mathematical problems in the area of stochastic line processes. He was a student of David Kendall's who wrote his Ph.D. thesis in 1967, a modest and unassuming young man with a penetrating mathematical mind. I have no doubt that his untimely death was a great loss to mathematics. But it was not in Cambridge that I first met him. He came to London to see me, at the Imperial College where I was spending the year 1967-68 with Harry Reuter. His visit was instigated by David Kendall. Here, however, I must backtrack a little.

I had made the acquaintance of David Kendall and Harry Reuter at another symposium organized by D.A.Kappos, at Loutraki in Greece in late May and early June 1966. Klaus was there too. The talk I gave (on strong ratio limits) raised David's interest because of its connections with the research of David Vere-Jones who had worked with him a few years previously (and who later paid us a brief visit in Heidelberg). As a result, David extended an invitation to me to spend a year in Cambridge and since this could not be arranged in time for the academic year 1967-68 I found myself spending that year in London, courtesy of Harry Reuter.

Rollo's visit to me had nothing to do with ratio limits. His interest was in the topic of line processes. This topic, which formed part of his thesis, was suggested to him by Kendall (see [2], p.74) and he was at that time puzzled by the difficulty of constructing even a single example of a line process of a desirable kind that was substantially different from a Poisson line process. This was his Big Problem ([2], p.70).

A little mathematics at this point is in order. A line process on the plane is, roughly, a random set of oriented straight lines satisfying certain desirable conditions. Choosing an origin $O$ and an axis through it in the plane, we can assign coordinates $(\theta, p)$ to any oriented line $l$ by taking $p$ to be the (signed) distance from $O$ to $l$ and $\theta$ to be the angle between the perpendicular from $O$ to $l$ and the chosen axis ($0 \leq \theta < 2\pi, -\infty < p < \infty$). Then each line becomes a point on the cylinder $L = C \times R$, where $C$ is the 'circle' $[0, 2\pi)$. The chief category of line processes Davidson was interested in was that of simple point processes on $L$, stationary under the transformations which represent Euclidean displacements of the plane and with a square integrable random number of 'points' in any bounded Borel subset of $L$.

Rollo imposed on his line processes the further condition that they should include no pairs of parallel or antiparallel lines. For the sake of convenience let us call line processes satisfying all these conditions 'regular'. An easy example is the Poisson point process on $L$ having rate measure $dl = d\theta dp$, the latter being invariant under Euclidean displacements. Going one step up, one may construct a Poisson process built on a random rate measure of the form $Z(d\theta)dp$, where Z is a random finite diffuse measure on C, stationary under rotations of the latter. Poisson point processes with randomized rate measure were called doubly stochastic Poisson by D.R.Cox who introduced them, and this is what Rollo called them. They are sometimes termed Cox processes and we will adopt this terminology. Cox line processes exhausted the supply of concrete examples of regular line processes Rollo was able to identify. In his thesis he obtained many properties possessed by line processes but his Big Problem was the construction of a regular example that was not Cox. On p.55 of [2] he effectively asks: are all regular line processes Cox?

Line processes had been considered before Davidson, although in the more classical spirit of integral geometry. R.E.Miles had begun his pioneering series of contributions to geometric probability in the early sixties and M.S.Bartlett had already looked at the spectral analysis of line processes. P.A.P.Moran was another probabilist interested in geometric probability in the sixties. David Kendall's interest in the subject dated back many years earlier and he was developing his theory of random sets at about the time Davidson was doing his thesis. In his introduction to [2] (p.5) Kendall views the transition from classical geometric probability to the study of random sets on manifolds as marking the advent of stochastic geometry. In this spirit Davidson was now treating line processes strictly as point processes on coordinate space and going beyond the Poisson (i.i.d.) case.

Almost everything special about the structure of line processes has to do with the observation that if $l_1, l_2$ are two parallel lines and $l_3$ a third line intersecting them, then there is a translation of the plane that maps the pair $l_1, l_3$ on to the pair $l_2, l_3$. Some time after his visit, while I was still in London, Rollo sent me a letter from Cambridge which included a simple but remarkable insight: if $X(l), l \in L$ is a stochastic process indexed by $L$ and stationary under the Euclidean group, then under mild conditions $X(l_1) = X(l_2)$ almost surely whenever $l_1$and $l_2$ are parallel lines, i.e. $X$ is in effect a stochastic process on the circle $C$. The essence of this insight is of course that, under any pseudometric $d$ in $L$ which is invariant under translations and continuous with respect to the topology of the cylinder $L$, the distance between parallel lines is zero. (Proof: with $l_1, l_2, l_3$ as above, $d(l_1, l_2) \leq d(l_1, l_3) + d(l_2, l_3) = 2d(l_1, l_3)$ and the last term can be made arbitrarily small by choosing an $l_3$ that makes a small angle with $l_1$.) Thus under such a pseudometric the cylinder collapses to the circle $C$. An immediate consequence is that the stationary measure $X(l)dl$ on $L$ with density $X(l), l \in L$, with respect to $d\theta dp$ has an obvious product form $Z(d\theta)dp$ and is thus invariant under translations of the plane.

After I moved from London to Cambridge in 1968 Rollo and I once or twice in our conversations touched on the subject of his big problem but otherwise our interaction was largely social. He once graciously invited me to join him at Trinity College for a college feast but I declined, saying that it was far too grand an occasion for me. Instead, I accepted another invitation of his to a humble college lunch. However, his big problem stayed in the back of my mind and I was beginning to develop an idea that came to fruition after I left for Columbus, Ohio in the summer of 1969. It was in the spring of 1969 that Klaus came through Cambridge and it was then that he and Kendall planned the Oberwolfach meeting on Integral Geometry and Geometric Probability that was held in June 1969. I wish I could report on what went on at that meeting, since it must have been important for the development of the subject, but unfortunately I did not attend it as I was preparing for my migration. There is reference in [2] (p.382) to a three-week collaboration between Klaus and Rollo in Heidelberg, following the Oberwolfach meeting. I was unaware of this collaboration but will make reference to it later.

It was after I arrived in Columbus that I worked out the idea I had in Cambridge: if the Palm probability $P(.|l)$ of the line process (i.e. the probability conditioned on $l$ being a member of the sample realization) is absolutely continuous with respect to the unconditional probability on events occurring in $L \setminus \{l\}$ then, according to Rollo's observation, the corresponding Radon-Nikodym density $Y_0(l)$, which defines a stochastic process indexed by $L$, is constant on sets of parallel lines. Applying 'shear' invariance arguments resting on this fact one can prove that the line process is Cox. I presented the result at a conference held in Columbus and according to the proceedings of that conference my talk was given on Easter Sunday, 29 March 1970. On exactly the same day Rollo sat down to write to me the letter that was published later in [2] and was to be his mathematical farewell. (For the date see page IV of [8] and page 379 of [2]. The result is on p.239 of [8].) He was to die four months later, on 29 July 1970, before he could realize a planned visit, sponsored by the Royal Society, to Rouben Ambartzumian in Armenia. He never saw the account of my result which was written up at about the time of his death and which, I should like to think, would have interested him. Some time in the autumn of 1970 I received a letter from Kendall informing me of Rollo's tragic death and of the plans that were afoot to publish what were ultimately the two memorial volumes Stochastic Geometry ([2]) and its companion Stochastic Analysis. I immediately sent David my manuscript, which he acknowledged as the first contribution received for publication in [2]. (A misprint on p.145 of [2] turned the year 1970 into 1971.)

Even after this I was unaware of an idea of Klaus's that emerged from his collaboration with Rollo in 1969. I was to remain unaware until the summer of 1971, doing in the mean time other work on point processes. In his thesis Rollo had shown that if the correlation measure $\sigma$ of a regular line process has a density $g(l,l')$ with respect to $dldl'$ outside the diagonal of $L \times L$ , then $\sigma$ is invariant under reflections. In doing this he had made use of the fact that $g(l,l')$ is a function of the angle between $l$ and $l'$ alone. Klaus showed him how to do away with the need of a density.

Klaus's method was to disintegrate the measure $\sigma$ with respect to the equivalence classes into which $L \times L$ is partitioned by the equivalence relation $\sim$, whereby $(l_1, l_2) \sim (l_3, l_4)$ if $(l_3, l_4) = (Tl_1, Tl_2)$ for some Euclidean displacement $T$. This led to Klaus's paper [5] and Davidson's Theorem in [1], which constitute the Armenian Connection of the title of the present article. These two papers were reprinted in [2] and it is worth mentioning that two more contributions to [2], one each by L.A.Santaló and R.E.Miles, were reprinted from the same volume of the Izvestia of the Armenian Academy of Sciences, which also included an article by Rouben Ambartzumian on line processes. The disintegration method provided the right tool for the study of moment measures of line processes and processes of higher-dimensional flats in Euclidean spaces and led to Klaus's more systematic treatment of moment measures in [6]. Klaus later proceeded to use it in the statistical analysis of hyperplane processes.

In [1] Davidson showed that the product form $Z(d\theta)dp$ for stationary random measures on $L$, referred to above, is still valid if we drop the assumption of the existence of a density $X(l), l \in L$ and replace it with the assumption that the stationary random measure almost surely charges no set of parallel lines. In the summer of 1971 Klaus came through Columbus and gave a seminar talk on moment measures of point processes, which included the assertion just made and his 'disintegration' approach to it. Before he finished his talk I knew what my next task was going to be: to construct the random measure $W$ which stands in for the measure with differential $Y_0(l)dl$ if no density $Y_0(l)$ exists, and to use the factorizability of this $W$ to establish Cox structure in cases where the Palm probability may not be absolutely continuous. I knew where to look for this $W$ in view of my earlier work on point processes on the real line.

Klaus and I drove together from Columbus to Yorktown Heights, NY (and got stuck in a traffic jam) where a conference on Stochastic Point Processes was held in August 1971 and I remember that on the way I formulated a conjecture that was fairly close to the result I later proved in [7], a result which demonstrated Cox structure under weak conditions and shed a little more light on Davidson's Big Problem. To be sure, the problem remained open but at least this paper indicated that, whereas for ordinary point processes Cox structure is the exception, in the case of line processes Cox structure is the rule rather than the exception. The paper included a rather laborious construction of the random measure $W$ for a general spatial point process, which I called the 'conditional intensity measure' in view of its intuitive meaning: in sloppy language, $W(dl)$ is the conditional expectation of $N(dl)$ (where $N$ is the point or line process), given the events occurring in $(dl)^c$.

The cause of line (and hyperplane) processes was later taken up by Olav Kallenberg who, in a series of papers beginning in 1976, explored further not only the structure of line processes but their asymptotic behavior as well, when the plane is 'translated away'. Among other things he pointed out that the assumption of stationarity under rotations is redundant. In 1977 he published in [3] his ingenious counterexample to Davidson's question and won for it the Rollo Davidson prize, an annual prize established in memory of Rollo Davidson. Thus Davidson's Big Problem was at last solved. The answer to his question may have turned out to be in the negative but the counterexample itself shows how exceptional non-Cox line processes are.

In another direction the concept of conditional intensity measure of a spatial point process (on which much light was shed by Kallenberg's work in 1978) turned out to have intimate connections with the theory of Gibbs processes in statistical mechanics, which was also the focus of intense activity at that time. In 1979, following hard on the heels of Nguyen Xuan Xanh and Hans Zessin who dealt with a case of an absolutely continuous Palm probability, K.Matthes, W.Warmuth and J.Mecke established the general link with so-called 'specifications' of Gibbs random fields in terms of local potentials. This work was continued by several authors (E.Glötzl, B.Rauchenschwandtner, A.Wakolbinger). The concept of conditional intensity measure was further elucidated by P.C.T. van der Hoeven, prompted partly by a problem suggested by Klaus, and a little later, in the third edition of his book on Random Measures ([4]), Kallenberg presented his comprehensive treatment of condition al intensity measures and their variants. Davidson's problem in stochastic geometry had come a long way.

The seventies were a lively time for stochastic geometry and for spatial point processes. There were sessions on stochastic geometry at European meetings (ISI $40^{th}$ Session in Warsaw 1975, $10^{th}$ European Meeting of Statisticians in Leuven 1977). In 1976 an international conference sponsored by the Armenian Academy of Sciences was organized in Armenia by Rouben Ambartzumian to mark the $200^{th}$ anniversary of Buffon's problem of the needle, probably the first problem in geometric probability ever posed. International conferences on point processes were held in the German Democratic Republic. The book on infinitely divisible point processes by J.Kerstan, K.Matthes and J.Mecke came out in 1974 (in German), followed by an English edition four years later and a Russian one in 1982. Chris Preston's book on Random Fields as well as the first edition of Olav Kallenberg's book on Random Measures appeared in 1976. G.$Mathéron's$ book was published in 1975. But it is not my intention to give a survey of these areas as they were evolving in the seventies.

After the encounter in Columbus, Klaus and I used to meet from time to time: in Bielefeld, Paris, even Manchester where he visited when we both acted as external examiners for Adrian Baddeley's Cambridge PhD thesis, and of course at some of the conferences mentioned above. By the late seventies our mathematical paths had diverged. I cannot, however, conclude my recollections without mentioning one other period in which our paths crossed again. This was in the early nineties when I got him involved in the appointments process of the newly founded University of Cyprus, which opened its doors to students in 1992. His role was crucial in the successful launch of the Department of Mathematics and Statistics and what he did was an excellent illustration of his desire, manifest throughout his life, to help young states in their development.

It only remains for me to wish him a happy ninth decade.

## LETTER TO H. ZESSIN, $28^{th}$ SEPTEMBER 2008

>From **Fredos Papangelou**

[*This letter was sent to Zessin on his request to comment the historical development of the condition* $(\Sigma')$ *in the beginning of the seventies.*]

You ask me a couple of questions and since I am not absolutely certain what sort of answers you expect, I will outline my perspective and leave it to you to extract the answers from it.

The distinction between two levels that I draw is the following.

(A) Level 1. The case of an absolutely continuous conditional intensity measure, i.e. where a density function exists.

(B) Level 2. The more general case where the conditional intensity measure is not absolutely continuous but may satisfy some weaker smoothness condition.

The condition in Theorem 1 of my 1976 paper you cite corresponds to case (A) and is, in view of the definition of the Palm probability $P(.|l)$, only a short step away from the condition of absolute continuity $P(.|l) \ll P(.)$ on a reduced $\sigma$-field, which I first introduced in 1970 in connection with line processes. (See Lecture Notes in Math. 160 , page 239.) I will say something about how this condition came about in my article of reminiscences that I promised for the Krickebergband. Very briefly, Rollo Davidson had earlier observed that stationary stochastic processes indexed by lines must be constant on sets of parallel lines. My idea was then that if $P(.|l) \ll P(.)$ on the reduced $\sigma$-field, then the constancy of the corresponding Radon-Nikodym density on sets of parallel lines can be used to prove that the line process has Cox structure. This gave a partial answer to a question raised by Davidson. (The proof of the theorem I presented in 1970 was published in 'Stochastic Geometry'. See Theorem 9 on page 144 there.) When Klaus later extended Davidson's result from stochastic processes to random measures, I was motivated to construct the spatial conditional intensity measure in my paper in Zeitschrift f. Wahrschein. 28, 207-226 (1974) and use it to prove Cox structure under the weaker conditions of Theorem 7 there.

The condition $(\Sigma')$ assumed at the top of p.118 in Matthes et al. corresponds to case (B) and is equivalent to condition $(\Sigma)$ in my ZW 1974 paper referred to above. The equivalence was proved by Kallenberg in his 1978 paper in ZW 41, 205-220 but you have to look at the proof of his Theorem 2.11 on page 211 to find it. (See also Matthes et al., Theorem 2.4.) This is the earliest appearance of statement $(\Sigma')$ that I know of in a published paper and I therefore assume that Kallenberg was the first to state $(\Sigma')$ . I do not remember if it emerged in discussions at earlier conferences. In any case, this relaxation of the absolute continuity condition of the Campbell measure was not suggested by me. The kernel arising in $(\Sigma')$ is exactly the conditional intensity measure appearing in Theorem 2 of my ZW 1974 paper if the non-atomicity condition of that theorem is satisfied. (There are some discrepancies if there is a discrete component.)

As you well know, the condition $(\Sigma')$ replaces the formula

$$\eta(\mu, B) = \int_{x\in B} \frac{P(\xi - \delta_x \in d\mu|x)}{P(\xi \in d\mu)}\rho(dx) \qquad (\rho(.)) = E\xi(.)), \tag{0.1}$$

which holds when the conditional intensity measure is absolutely continuous, by the formula

$$\eta(\mu, B) = \frac{\int_{x\in B} P(\xi - \delta_x \in d\mu|x)\rho(dx)}{P(\xi \in d\mu)} \tag{0.2}$$

in the more general case. In the context of the Remark on p.110 of your 1979 paper with Nguyen Xuan Xanh the differential $V(x|\pi S)\rho(dx)$, which is essentially the same as $X(l,.)\nu(dl)$ on p.145 of 'Stochastic Geometry', is replaced in Matthes et al. by the more general $\eta(\pi S, dx)$ which may have no density.

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Поступила 25 ноября 2008