ON THE CONTRIBUTIONS OF GEORG BOHLMANN TO PROBABILITY THEORY

ULRICH KRENGEL

Institut für Mathematische Stochastik der
Universität Göttingen

IN HONOR OF MICHAEL LIN ON THE OCCASION OF HIS RETIREMENT

1. INTRODUCTION

The name of Georg Bohlmann (1869–1928) is known to few probabilists. Yet, it appears that he was the first to give the formal definition of independence of n events now used in all probability books. He took significant and original steps towards Kolmogorov's axioms. Moreover, Bohlmann gave the now universally accepted formal definition of conditional probabilities. Another very interesting contribution of Bohlmann is a method of smoothing time series, presumably the first method making use of a penalty term. It resembles the Hodrick–Prescott–filter.

The purpose of this work is to unearth these parts of his mathematical work, and to report on his life. Wilhelm Lorey wrote an obituary [53] soon after Bohlmann's death. It appeared in German in a German actuarial journal, and it does not give due credit to the work presented here because Lorey relied on the opinion of von Mises, who preferred his own approach to the axioms of probability.

2. VITA

Georg Bohlmann was born in Berlin on April 22, 1869. His father Otto Bohlmann was a lawyer who carried the honorary title "Justizrat". Eventually, he was a lawyer for the Supreme Court. Georg Bohlmann's mother Ottilia had the maiden name Brix. Georg Bohlmann was a Lutheran. He went to school in Berlin and Leipzig, and got his "Abitur" (final high school examination) in 1888 from the Wilhelms-Gymnasium in Berlin, a grammar school. His languages were Latin, Greek, and French. By a recommendation of a doctor, he was exempt from sports.

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Georg BOHLMANN
(1869-1928)

photo from an album donated to D.Hilbert on the occasion of his 60th birthday

Bohlmann studied mathematics in Berlin. His main teachers were Dr. Kötter (Analysis, Synthetic Geometry), Prof. Kronecker (Theory of Integrals, Arithmetic, Determinants, Algebraic Equations, Number Theory, Concept of Number), Prof. Fuchs (Differential Equations, Abel Functions, Special Functions, Complex Variables, Analytic Mechanics, Elliptic Functions). He also attended lectures in physics (Prof. Kundt, Prof. Planck), philosophy (Prof. Zeller, Prof. Dilthey) and psychology (Prof. Paulsen). It seems that Bohlmann started to work on his own on a problem on complex variables. It required the determination of all functions satisfying an algebraic addition theorem. It had already been solved by Weierstraß with complex variable tools, but Bohlmann noticed a connection with the theory of transformation groups. He gave a new solution making heavy use of Lie groups. Lorey [53] reports that this topic was not so fashionable in Berlin, and, hence, Bohlmann decided to complete his doctoral dissertation in Halle. He asked Prof. Albert Wangerin (1844–1933) to be his advisor. Wangerin was a highly respected professor mainly known for his work on spherical functions. (Later, he was even elected President of the Leopoldina, the famous Halle Academy.) Bohlmann obtained his doctoral degree in July 1892, see [73]. The title of his doctoral thesis was "Ueber eine gewisse Klasse continuierlicher Gruppen und ihren Zusammenhang mit den Additionstheoremen" (On a certain class of continous groups and their relation to the addition theorems).

Bohlmann then worked as an assistant in the Institute of Meteorology in Berlin. Probably, this was his first encounter with problems in Applied Mathematics. He participated in seminars given by Fuchs, Schwarz, and Frobenius. At the same time, he worked on problems on differential equations, and wrote two papers for the Crelle Journal für Reine und Angewandte Mathematik. He got the support of Felix Klein and went to Göttingen. Klein asked his colleague Heinrich Weber (1842–1913) to write an expert's report about Bohlmann's work [2], [3]. Weber was a professor in Göttingen between 1892 and 1895, and he had done important work on differential equations, see [57]. When this report turned out favourable, Georg Bohlmann obtained the Habilitation (permission to teach) in August 1894.

Klein had good use for Bohlmann. In 1893, Klein had attended the Congress of Mathematicians organized in Chicago during the World Fair. He was a main speaker. His call "Mathematicians of the World, Unite!" was an important incentive for organizing the first World Congress of Mathematicians in Zurich in 1897. During the Chicago Congress, Klein established contacts to the New York Mutual Life Insurance Company. After returning to Göttingen, he held a seminar on actuarial science, and he contacted his friend Ludwig Kiepert (1846–1934) who was professor of mathematics in Hannover and held a subsidiary office as director of the "Preussischer Beamten-Verein", a forerunner of the present Hannover Life Insurance Company.

On September 5, 1895, Klein and Kiepert met with Friedrich Althoff (1839–1908), a ministerial director, who was in charge of all matters pertaining to universities in Prussia, and with Ernst Höpfner (1836–1915), trustee (Kurator) of the university of Göttingen. They decided to found a "Seminar für Versicherungswissenschaft" (seminar for actuarial science). It was inaugurated on October 1, 1895, and Wilhelm Lexis (1837–1914) was asked to serve as its first director. The seminar had two branches, a "mathematical class" and an "administrative class". Lexis was professor of Political Economy and taught courses for the administrative class, e.g. economy. Victor Ehrenberg (1851–1929) gave courses on law. Bohlmann was asked to teach the courses for the mathematical class, see Koch [45].

W.Lorey, in his obituary, mentioned that he was a student in the first course given by Bohlmann on actuarial mathematics. He liked the lecture a lot. He also mentioned that, in the very first session, Bohlmann explained how to work with a calculating machine, a "Brunsviga". He guessed that this was the first time a calculating machine was used in a lecture in Germany.

During these years, Bohlmann did not have a paid position in Göttingen. Klein complained about this in Berlin, and his efforts resulted at least in a stipend given to Bohlmann. In 1897, he was given 1200 Marks, and in 1899 the stipend was increased to 1600 Marks for the whole

year. Of course, Bohlmann's father had to provide additional support. (This was not unusual for a "Privatdozent" in Germany at that time.)

Bohlmann was also asked to do other work for the mathematical community. He wrote a report for the German Mathematical Society (DMV) on the development of calculus books from the time of Euler (when the presentation still lacked rigour) to the end of the nineteenth century. By that time the influence of Weierstrass had helped a lot to increase the mathematical standards of rigour. Bohlmann also was the editor of the second edition of the book of Serret–Harnack, and he translated and revised an Italian textbook of Genocchi–Peano jointly with A. Schepp, see [6], [7].

Below, we shall discuss the contributions of Bohlmann to the axiomatic foundation of probability theory. A first version of the Bohlmann axioms appeared in 1900 in his extensive and impressive article "Lebensversicherungsmathematik" [11] in the "Enzyclopädie der Mathematischen Wissenschaften", a project initiated by Felix Klein. A second greatly improved version [18] was presented on the occasion of the World Congress of Mathematicians in Rome in 1908. (This will be explained below.)

Within a short time, Georg Bohlmann had established himself as a leading expert for the mathematics of life insurance. In 1899, the Ministery of the Interior of Prussia approached Prof. Lexis and Dr. Bohlmann asking them to write an expert's opinion about renewing the license of the New York Mutual Life Insurance Company (MONY) in Berlin. Actually, this company had opened a branch in Berlin in 1885, but the Prussian Government had stopped the selling of new contracts after they began to suspect that the MONY sold contracts of the Tonti–type. (That was illegal in Germany.) Lexis and Bohlmann were to investigate this reproach, which may have been a move by the competitors. The handwritten version of this expert's opinion (virtually totally in the handwriting of Bohlmann) can be found in the "Handschriftenabteilung" of the university library of Göttingen. It is a huge document [78]. Apparently, Bohlmann studied many details quite carefully. The conclusion was that the reproach was unfounded, and the licence of the MONY was renewed.

In 1899, Klein proposed to the ministry in Berlin that Bohlmann should be granted the title of extraordinary professor. This application was accepted in 1901. (Extraordinary professors are allowed to carry the title "Prof.", but do not have a payed professorship.)

In 1902, the MONY made a very attractive financial offer to Bohlmann. He accepted it in 1903 after consulting Klein [79]. Bohlmann had hoped to get a paid professorship in Germany or even in Göttingen, but this seemed unlikely. After some time, Bohlmann became Chief Mathematician of the Berlin branch of the MONY and their Chief Representative for the German Board of Control.

Yet, he continued to do research in mathematics, to publish papers, and to correspond with Bortkiewicz, Chuprov, and others, see Sheynin [66]. In 1981, H.Cramér [34] regarded Lexis, Bortkiewicz, Chuprov, Markov and Bohlmann as originators of the Continental School of Statistics.

In 1904, Bohlmann married Ellida Brix (possibly a cousin), a daughter of a high naval officer. They had a son who studied law.

Little is known about the later part of Bohlmann's life. His wife died in 1919, and in the years thereafter Bohlmann became seriously ill. He had considerable problems with his eyes and was almost unable to read. Bohlmann died in Berlin on April 25, 1928. The MONY informed me that their archives do not contain material on Bohlmann. Here, material from Lorey [53] and from [73], [76], [77] has been used.

3. Axioms for probability theory

In the winter term 1898–1899, David Hilbert taught a course on the elements of geometry. He emphasized that the essential point was the relation of the considered objects to each other. He explained: "One must be able to replace the words points, straight lines and planes, by the words tables, chairs, and beer mugs. Then, if the tables, chairs, and beer mugs satisfy the axioms, the

assertions of the theory hold for them, too. It is not important to say what a straight line is" (See [41], [62],p. 57). This point of view contrasted with the traditional opinion that axioms were apparently true statements from which other statements could be derived.

At about this time, Bohlmann was working on his article "Lebensversicherungs–Mathematik". In its beginning he stated his conviction that the mathematical foundations of insurance mathematics had to be built on the theory of probability. (He mentioned that this view was not commonly accepted. He quoted a recent book of K. Wagner [69] on life insurance in which the author asserted: "Wahrscheinlichkeitsrechnung und Versicherung haben innerlich nichts miteinander zu tun." (Probability and insurance have basically nothing to do with each other.)

In his article [11], Bohlmann introduced probability in an axiomatic way in 1900. This was clearly inspired by Hilbert's approach to geometry. Bohlmann gave the following definitions and axioms:

Definition1: The probability of the happening of an event E is a positive proper fraction $p(E)$ associated to E.

Definition 2: Two events E_1 and E_2 are called incompatible if the probability that both of them happen is 0.

Axiom 1: If E is certain, then $p(E) = 1$, if E is impossible, then $p(E) = 0$.

Axiom 2: If two events E_1 and E_2 are incompatible, the probability of the event E that E_1 or E_2 happen is $p(E_1) + p(E_2)$.

Axiom 3: If p'_2 is the probability that E_2 happens, provided it is known that E_1 happens, then the probability of the event E^* that E_1 and E_2 happen is

$$p(E^*) = p(E_1)p'_2.$$

Definition 3: E_1 and E_2 are called independent if

$$p(E^*) = p(E_1)p(E_2).$$

These definitions and axioms had obvious deficiencies. The concept "event" remained undefined. The requirement that $p(E)$ be rational may have been an outdated concession to the definition given by Laplace. As p'_2 remained undefined, axiom 3 really is a definition of p'_2, and not an axiom.

But what is more important: For the first time, probabilities were defined as functions of events, functions with certain properties. What a difference to Laplace's definition! Von Plato [58] , uncharacteristically, belittles this idea: He writes that Bohlmann "does not do much more than call some of the basic properties of probability calculus by the name of axioms". He seems to have little feeling for the magnitude of this step, which is even more evident if one thinks about the long time it took for this idea to be accepted. Bohlmann mentioned the book of Poincaré [59] as a reference. In [59], the finite additivity is a theorem derived from properties of relative frequencies and not an axiom.

It is frequently taken for granted that the axiomatic approach to probabilty was initiated by Hilbert with his talk on the occasion of the Second International Congress of Mathematicians in Paris, on August 8, 1900. In his sixth problem he had asked for the axiomatization some "subjects of physics": "In view of our studies of the foundations of geometry, we are led to the problem of treating those subjects of physics axiomatically, in which mathematics already plays an important part now. We name in the first place probability theory and mechanics. With respect to the axioms of probability theory it seems desirable to combine their logical study with a satisfactory treatment of the method of mean values in mathematical physics, in particular in the kinetic theory of gases."

As a footnote to his sixth problem, Hilbert quoted the paper [10] of Bohlmann. This paper reproduced some talks which Bohlmann had given during the Easter vacations of 1900 to a group of high school teachers. In it, he had already announced the axiomatics of probability theory, which

was to appear in [11]. On the other hand, C.Reid [62], p. 70 reports from the correspondence of Hilbert with Hurwitz that by the end of of March 1900, Hilbert had not determined the subject of his Paris talk yet. By June, he had not produced a lecture, and the program for the Paris Congress was mailed without the title of Hilbert's talk being listed.

To my knowledge, Hilbert had not shown interest in probability theory prior to his Paris talk. But he had always been interested in physics. Therefore, it seems that the physics aspects of the sixth problem are genuinely due to Hilbert. But Bohlmann was the first to suggest that probability theory could be made rigorous by using an axiomatic approach. Hilbert included this in his sixth problem. It seems nearly certain to me that the problem of axiomatization of probabilty was proposed to Hilbert by his colleague Bohlmann. Hilbert made no secret about the fact that he had solicited various problems by talking to other mathematicians.

Hilbert felt that the key ideas of Bohlmann were basically sound in spite of the deficiencies mentioned above. Leo Corry [33], in a profound study of the work done on the axiomatics of physics, quotes lectures of Hilbert [40],[75] given in 1905 on "Logical principles of mathematical thinking". In these lectures, Hilbert introduced probability theory in the Bohlmann way except for a change of notation. (Hilbert used vertical bars for conditional probabilities.) His only mild criticism was that "at this stage of the development it is not clear yet which statements are definitions and which statements are axioms." Apparently, Hilbert thought that Bohlmann had understood his general ideas about axiomatics.

In a subsequent note in this journal, a scan of [75], and the interesting story connected with it shall be presented.

We mention that G. Boole [29],p. 288, had already made a remark in favour of the axiomatization of probability in 1854. He wrote "The claim to rank among the pure sciences must rest upon the degree in which it (the theory of probability) satisfies the following conditions: 1st :That the principles upon which its methods are founded should be of an axiomatic nature." (He listed two other general scientific conditions.) But in spite of his voluminous writings on probability, he did not try to formulate any axioms himself. On p. 244, he wrote "The probability of an event is the reason we have to believe that it has taken place or that it will take place." On p. 255, he listed some "principles which have been applied to the solution of questions of probability", among them the finite additivity. But he did not connect this with the axiomatic approach. Thus, his above remark remained rather vague.

Hilbert remained interested in a more complete treatment of the subject. His student Ugo Broggi completed a thesis [31] with the title "Die Axiome der Wahrscheinlichkeitsrechnung" in 1907. Broggi started to use set theory to describe events. He also introduced σ–additivity. But he claimed that σ–additivity was implied by additivity. Later, Steinhaus showed that this was false, see [68]. Broggi made use of Lebesgue measure, and restricted his attention to measurable sets. "Es sind die messbaren Mengen die einzigen, die wir in Betracht ziehen wollen." (The measurable sets are the only sets which we want to consider.) But Broggi did not include these notions into his system of axioms. His main interest was in considerations about independence, completeness, and consistency of his set of axioms.

(Broggi was born on December 29, 1880, in Como, Italy. He studied in Milano, Berlin, and Göttingen. Later, he taught for many years in Argentina and Italy, mainly on mathematical economics and actuarial problems. He was among the founding members of the Argentine Mathematical Society. He died on November 23, 1965, in Milano. An obituary was written by G.Ricci [63]. More detail can be found in the internet in an article by Manuel Fernándo López of the University of Buenos Aires, see [52].)

Let us now look at the Bohlmann axioms as formulated in the Rome paper [18]. He wrote:

We postulate that the events which we consider have a probability which is expressed by a number with the following properties:

Axiom 1: The probability that an event E happens, in short: the probability of E, is a positive

number $p(E)$.

Axiom 2: If the event E is certain, we have $p(E) = 1$.

Axiom 3: If it is impossible that both E_1 and E_2 happen, and if E is the event that E_1 or E_2 happen, then $p(E) = p(E_1) + p(E_2)$.

He concluded right away that $p(\text{non}-E)+p(E) = 1$, that finite additivity holds, that $0 \leq p(E) \leq 1$, and that $p(E) = 0$ if E is impossible. Apparently, by positive Bohlmann means nonnegative.

Bohlmann stated explicitly, that a study about the concept "event" was missing in his presentation.

He explained that he wanted to get away from the scheme of defining probabilities by distinguishing favorable and possible cases.

He then studied the situation with m mutually exclusive events $E_1, E_2, \ldots, E_m$, and stated that for him the words "gleich möglich" (equally possible) simply meant the assumption $p(E_1) = p(E_2) = \ldots = p(E_m)$. (Note that there had been unending discussions about this concept.)

At that time, it was controversial, in which cases the assumption "equally possible" was justified. In a report for the DMV, Czuber [35] discussed a controversy between Karl Stumpf and Johann von Kries. The latter insisted in 1886 that the assumption of equal probabilities required objective knowledge. The former stated in 1892 that the assumption was only satisfied if we know absolutely nothing about the outcome. Even Markov felt that the lack of knowledge supported the assumption of equal probabilities. In Markov's textbook [54],p. 5–6 he gave an example of an urn containing balls with four different colors. He said that if we know nothing about the numbers a, b, c, d with which the colors occur, each color is equally likely. But if we know $a < b < c < d$ we cannot say anything about the probabilities. He did not distinguish the uncertainty connected with the lack of knowledge of the numbers from the uncertainty connected with the random drawing from the urn. Today, we know that Markov would have needed a statistical model with different probabilities.

After deriving some consequences of the above three axioms, Bohlmann states a fourth axiom as follows:

Axiom 4: Let $p(E \text{ and } F)$ denote the probability that E and F happen. The ratio $p(E \text{ and } F)/p(F)$ denotes the probability $p(E|F)$ that E happens if it is known that F happens. Then the 4th axiom is expressed by $p(E \text{ and } F) = p(F)p(E|F)$.

Actually, Bohlmann used the notation $p(E \text{ w } F)$ instead of $p(E|F)$. The letter "w" abreviated the word "wenn" (if).

The last sentence in axiom 4 is redundant. What really is interesting is the beginning. Here, Bohlmann gives the presently universally accepted definition of conditional probabilities.

We remark that in 1901 Hausdorff [37] had discussed the concepts of conditional probability and independence (of two events) in the context of experiments with finitely many equally likely outcomes. For example, he pointed out, that the consideration of $p(E|F)$ may make sense if E happens prior to F, and if there is no causal relation. He defined the conditional probability of E given F, say $p(E|F)$, as the ratio of the cardinality of $E \cap F$ and the cardinality of F. Then he called E and F independent if $p(E|F) = p(E|\text{non}-F)$.

W. Purkert [61] has given detailed comments on Hausdorff's paper [37]. In these comments he reports the state of the historical development of the concept "conditional probability" in 1901. He concludes that Hausdorff (in the special case considered by him) has been the first to realize the fundamental importance of this concept. Hausdorff did not quote Bohlmann's paper [11] presented in 1900, and Bohlmann did not quote [37] in 1908. Apparently, they independently emphasized the importance of "conditional probabilities".

In the list of Boole's "principles" mentioned above, principle 4 states: "The probability that if E happens, an event F also happens, is equal to the probability that both happen divided by the probability of E." This might be read as a definition, but as the other principles are rules on how to compute certain probabilities from others, it seems unlikely that Boole meant it as a definition. The novelty of the Bohlmann approach consisted in turning things around. He used properties

which formerly had been stated as results as definitions and therefore gave vague concepts a precise meaning.

To my knowledge, the only reference discussing the Rome axioms is the unpublished thesis of H. Bernhardt [25],p. 46 on the von Mises Theory. She pointed out that the axioms 1–3 correspond to Kolmogorov's axioms III–V, [46]. Of course, Kolmogorov not only had sound set theoretic foundations, but he also succeded in deriving a tremendously fruitful theory.

Kolmogorov [46] started his exposition with a section on finite probability spaces. And he could have just stated that for this it was sufficient to adopt the Bohlmann axioms and to say that an event was a subset of the set of numbers $1, \ldots, m$. But apparently the paper of Bohlmann had escaped his attention in spite of the fact that he visited Göttingen a short time before he wrote his book, and the Proceedings of the Rome Congress could not be called an obscure journal. (In his book, Kolmogorov mentioned only three previous systems of axioms, those given by von Mises, by S. Bernstein [26] and by Łomnicki [51].) Kolmogorov has repeatedly quoted the work of Lebesgue, Fréchet, Carathéodory, and the book [38] of Hausdorff, but it would be futile to speculate if Bohlmann would have given a different system of axioms, had these works been available to him.

A detailed study of the "Sources of Kolmogorov's Grundbegriffe" is due to Shafer and Vovk [65]. It also contains an overview of several other attempts at finding axioms for probability theory, like those of Laemmel [49], Łomnicki [51], Bernstein [26] and others. As they have little to do with the work of Bohlmann, we refer the reader to [65]. Also Ivo Schneider [64] provided a discussion of this area. His book also contains very useful excerpts of some of these references.

Perhaps the axioms of Bohlmann appeared too simple. Erich Kamke gave a survey for the DMV on different attempts at an axiomatization of probability in 1933, [44]. In this survey he mentioned a joke which he had heard in Göttingen during his student days (between 1909 and 1913). "A probability is a number between 0 and 1 about which nothing else is known." It appears that this aimed at ridiculing Bohlmann. He had been the only one defining probabilities as numbers between 0 and 1 with some properties, and his Rome paper had just been published.

The discussion about the axiomatization of probability soon was dominated by the approach of von Mises [55] and subsequent work by many authors. A short summary of these topics has been given in my article [47] written on the occasion of the 100th anniversary of the foundation of the DMV. A thorough discussion of the work of von Mises and Tornier has been given by Hochkirchen [42]. There are still claims that the approach of von Mises (in spite of its logical flaws) is closer to applications than the axioms of Kolmogorov. But no example illustrating this claim is known to me. It is incredible how much effort has to be used in the von Mises setting to describe even a single toss of a coin. I agree with the opinion that von Mises tried to give a model which contains an abstraction of a way of determining probabilities. But the Euclidean geometric axioms do not say anything about measuring angles either. Such descriptions seem not to be appropriate within axioms.

4. Independence

While the system of axioms described by Bohlmann was incomplete due to the lack of a mathematical definition of the concept "event", there is no question that the priority for the rigorous mathematical formulation of the concept "independence" belongs to him. Of course, probabilists had multiplied the probabilities of independent events for centuries, but there was no formal definition. Two events E_1 and E_2 were called independent "if the occurence of one of them does not affect the probability of the occurence of the other one." The meaning of this sentence was not explained. For example, Boole [28],p.255, gave the following definition: "Two events are said to be independent, when the probability of the happening of either one of them is uneffected by our expectation of the occurence or failure of the other." Many books on probability contained a "multiplication theorem" of probability theory. This sounded as if one can prove the product formula.

Kolmogorov, in his "Grundbegriffe" [46] attributed the rigorous formulation of the definition of independence to a book of S. Bernstein [27], written in 1927 in Russian.

In 1901, Bohlmann stated the following formal definition: Two events E_1 and E_2 are called independent, if the probability p that both E_1 and E_2 happen is the product of $p(E_1)$ and $p(E_2)$. This was only a beginning.

In the Rome paper [18],§3 a more systematic study was given. Consider m events $E_1, E_2, \ldots, E_m$. Let E_i' denote the event that E_i does not happen. For $a_i = 0$ or $= 1$ let $p(a_1, \ldots, a_m)$ denote the probability of the event that E_1 happens a_1 times, E_2 happens a_2 times ... etc. For example, $p(0, 1, 1, 0)$ is the probability of the event that E_1 does not happen, E_2 does happen, E_3 does happen, and E_4 does not happen. Let $p_i = P(E_i)$ and $q_i = 1 - p_i$. Then $q_i = p(E_i')$. Bohlmann called $E_1, E_2, \ldots, E_m$ independent if, for all $\mathbf{a} = (a_1, a_2, \ldots, a_m)$, the identity

$$p(\mathbf{a}) = p(a_1, a_2, \ldots, a_m) = \prod_{i=1}^{m} p_i^{a_i} q_i^{1-a_i}$$

is valid. In other words, the product formula must hold for all events defining the numbers $p(a_1, \ldots, a_m)$.

For $m = 3$, Bohlmann gave an example showing that pairwise independence does not imply independence, [18],p. 257. (This, too, was attributed to S. Bernstein by Kolmogorov.) Bohlmann's example was complicated: Assume, an urn contains 16 indistinguishable capsules. Each capsule contains 3 balls, numbered $1, 2, 3$, which can be white or black. Let us say that a capsule has characteristic $(0, 1, 1)$ if the first ball is black, the second is white, and the third is white. Assume that there are 3 capsules of characteristic $(1, 1, 1), (0, 1, 0), (0, 0, 1)$, and $(1, 0, 0)$ each, and one capsule of characteristic $(0, 1, 1), (1, 1, 0), (1, 0, 1), (0, 0, 0)$ each. Let A_i be the event that in a capsule drawn at random, the i–th ball is white. Bohlmann checked that these events are pairwise independent but not independent.

For general $m \geq 3$, Bohlmann showed that the following set of conditions is necessary and sufficient for independence:

(1) Any two of the events are independent;

(2) Any event E_i is independent of any event "E_j and E_k", that E_j and E_k happen, for $i \neq j \neq k \neq i$.

(3) Any fourth event is independent of the happening of three different events, ..

etc.

In other words: The product formula must hold for the conjunction of any subset of the m events.

Bohlmann also discussed the relation to conditional probabilities. He showed that E_1 and E_2 are independent if and only if $p(E_2|E_1) = p(E_2|E_1')$.

Another observation stated by Bohlmann is the assertion : The events $E_1, \ldots, E_m$ are independent if and only if the generating functions satisfy

$$\sum_{\mathbf{a}} p(\mathbf{a}) x_1^{a_1} \cdots x_m^{a_m} = \prod_{i=1}^{m} (q_i + p_i x_i)$$

In other words: The generating function for the compound experiment of observing all E_i's is the product of the generating functions of the separate experiments.

Bohlmann then studied the expectation and the variance of the number $T = \sum_{i=1}^{m} I_{E_i}$ of i's with $1 \leq i \leq m$ for which the event E_i happened. This was done both in the independent and in the dependent case.

For example he showed

$$\text{Var}(T) = \sum_{i=1}^{m} p_i q_i + \sum_{i \neq k} (p_{ik} - p_i p_k),$$

where $p_{ik} = p(E_i \cap E_k)$.

This "Bohlmann formula" is mentioned several times in the correspondence of Chuprov with Markov, see [56], [64]. Bohlmann's work influenced Chuprov's work on dispersion theory, see Chuprov [32]. Markov used $\mathrm{Var}(T)$ for dependent summands somewhat earlier than Bohlmann. As he had been skeptic of the axiomatic approach he was surprised that it led Bohlmann to his "nice" formula. When the German translation [54] of Markov's book on Probability Theory appeared Bohlmann wrote a very positive four page review [23] explaining the novel features.

Bohlmann was also interested in empirical methods of checking independence, see [18],p. 250. To decide if E_1 and E_2 were independent, he looked at the determinant D of the 2×2 matrix with elements $p(j, k)$. He checked that the events E_1 and E_2 are independent if and only if $D = 0$. Then he considered an experiment consisting of n independent repetitions of the basic experiment. Let $n(1, 1)$ be the number of experiments in which E_1 and E_2 happen, $n(1, 0)$ the number of experiments in which E_1 and E_2' happen, $n(0, 1)$ the number of times when E_1' and E_2 happen, and $n(0, 0)$ the number of times when E_1' and E_2' happen. Clearly, $n = n(1, 1) + n(1, 0) + n(0, 1) + n(0, 0)$. It is clear that $n(j, k)/n$ is an estimate for $p(j, k)$. Therefore it is natural to look at $Y = n(0, 0)n(1, 1) - n(0, 1)n(1, 0)$. Bohlmann computed the expectation and variance of Y. The expectation is $E(Y) = n(n - 1)D$. Using the Bienaymé–Chebychev inequality, Bohlmann showed that $Y/n(n - 1)$ tends stochastically to D. He concluded that the hypothesis of independence of E_1 and E_2 can be accepted if $Y/n(n - 1)$ does not deviate from 0 by more than can be expected by looking at the value of the variance. He did not try to find the distribution of Y, which would have been needed to perform a statistical test. (Apparently, he did not know that Karl Pearson had proposed a chi–square test for testing independence in 1900.)

In the remaining part of the paper, Bohlmann studied applications to actuarial problems.

5. Time series and limit theorems

Actuarial work led Bohlmann to the consideration of the following problem on the smoothing of time series:

A polygon is given by equidistant abscissae $1, 2, \ldots, n$ and corresponding ordinates $y_1, y_2, \ldots, y_n$. The problem is to replace the original polygon by a smoothed polygon with ordinates $z_1, z_2, \ldots, z_n$. The deviation of the two polygons from each other is measured by $A = \sum_{i=1}^{n}(y_i - z_i)^2$, and the oscillation of the smoothed polygon is measured by $B = \sum_{i=1}^{n-1}(z_{i+1} - z_i)^2$. We fix a strictly positive number w, called the weight. The problem is to find the "smoothed" polygon which minimizes $A + wB$. Bohlmann showed that this problem has a unique solution, and showed how to compute it. He illustrated it by an example, and he gave graphical methods. His work was presented to the Göttingen Academy in 1899, see [8]. Today with the use of computers, it should not matter that the computations are more involved than the commonly used moving averages. It must be admitted, though, that the method has the disadvantage that the smoothed polygon usually has a smaller slope than the original one. By applying the smoothing repeatedly, one gets a limit which is a horizontal line. Perhaps, one should introduce the boundary conditions $y_1 = z_1$ and $y_n = z_n$ to avoid that. The method of Bohlmann resembles a tool now frequently used in macroeconomics to separate a trend component and a cyclical component of a time series. In this method, called Hodrick–Prescott–filter, the term B of Bohlmann is replaced by the new "penalty term"

$$\sum_{i=2}^{n-1}(z_{i+1} - 2z_i + z_{i-1})^2$$

In other words, the sum of squares of the differences is replaced by the sum of squares of the second differences. This method was first proposed by C.E.V. Leser [50] in 1961, but it became widely known after the publication of a study of Hodrick and Prescott [43] in 1997. Leser refered to an older publication of E.T. Whittaker [70] which appeared in 1923. Whittaker measured smoothness

by using the third differences, and derived his result by a maximum likelihood argument. In his method, B is replaced by the sum of squares of the third differences. None of these papers refer to Bohlmann. Of course, Bohlmann had no applications to macroeconomics in mind.

Bohlmann also sketched a continuous time version of the smoothing method. Following a suggestion of E.Zermelo, he applied it to the famous Weierstrass function which is continuous and nowhere differentiable. In contrast to the discrete time case, he obtained a smooth curve which was independent of the considered time interval. It was twice differentiable, but the third derivative did not exist anywhere. For a probabilist, it seems interesting to know what can be said about the Bohlmann–smoothing of a Brownian motion. Surely, the ideas in this paper deserve further attention, and interesting work remains to be done. Perhaps, this paper demonstrates the considerable analytic power of Bohlmann better than his axiomatic work.

It is not surprising that, after 1903, the papers of Bohlmann are mainly devoted to problems in actuarial mathematics. But in 1913, he wrote yet another long paper in probability. It appeared in the Mathematische Annalen [20]. It is of a rather technical character. Some colleagues had been making use of some rules of thumb without specifying the mathematical conditions, and without providing proofs. He acquired the respect of certain probabilists like Bortkiewicz and Chuprov by treating these subjects in a rigorous manner.

In essence, he considered the following problem: Let $X_1, X_2, \ldots$ be a sequence of random variables converging in an appropriate sense to a common expectation x_0.

Let f be a suitable real valued function (Bohlmann looked at algebraic functions.) Then, for large n, $E(f(X_n))$ can be approximated by $f(x_0)$. The analogous question can be asked for the standard deviation. Under suitable conditions, $\sigma(f(X_n))$ can be approximated by $|f'(x_0)|\sigma(X_n)$.

Bohlmann studied this problem by looking at the Taylor expansion of f at the point x_0. Bohlmann's conditions and formulations were complicated. Today we have much better machinery to study this problem.

6. Some Actuarial Mathematics

We now discuss some of Bohlmann's work on actuarial mathematics. It would be nice if somebody more at home in this field would study the actuarial work not considered here. We partly follow Purkert [60] and Lorey [53].

In his article [11] for the encyclopedia, Bohlmann stated two "axioms" for survival probabilities.

Axiom 4 : If (x) is a person having age x the probability $p(x, x + m)$ that (x) survives to age $x + m$ is a function of x and m. There is a bound w such that nobody survives to age w.

Axiom 5 : If (x) and (y) are different persons of age x and y, the events that (x) survives to age $x + m$ and (y) survives to age $y + n$ are independent.

Bohlmann considered populations with identical risk. This means that the probabilities depend only on the age of the persons and not on the persons. In this work, the term "axiom" really is a misnomer. The "axioms" describe assumptions about the considered group of persons. Thus, they describe a mathematical model of the considered reality.

The notation with (x) describing a person and x her age is not suitable since different persons can have the same age.

Under the stated assumptions, Bohlmann developed the main methods of actuarial mathematics of that time. The article [11] was successful, so that a second updated and translated article appeared in French [12] in the French edition of the encyclopedia. The editors of the French edition emphasized that the articles were written in very close cooperation of a French mathematician with the author of the German article. In Bohlmann's case, the French coauthor was H. Poterin du Motel. The article is substantially revised. (For example, the definition of independence given in Rome, appears here, too.) A comparison of the German and French version appears in [24],p. 13.

We now turn to a nice example of Bohlmann's research on actuarial mathematics. Assume we want to fix the price for insuring a risk described by a random variable Z, taking the values

$z_1, z_2, \ldots, z_n$ with positive probabilities $p_1, \ldots, p_n$. This risk would be turned into a fair game, if the insurance company B_0 would ask for a premium $E(Z)$. The fair game is described by $X = Z - E(Z)$, taking the values x_i with positive probabilities p_i, $i = 1, \ldots, n$. But the insurance company wants to add a charge for the administrative cost and a charge for carrying the risk. This second added charge may be a multiple either of the "mean risk" M, which coincides with the standard deviation of X or of the "average risk"

$$D = E(\max(X, 0)) = \sum_{i=1, x_i > 0}^{n} p_i x_i$$

which is the expectation of the potential losses of B_0. (This terminology seems to be due to Hausdorff [36], who advocated using M because of the additivity of the variances for independent risks. At that time it was more common to use D.)

Wittstein [71], [72] gave the following interpretation of D: D is the expectation of the potential losses of B_0, and hence it is the "fair part" of the cost for B_0 if B_0 wants to buy reinsurance from a company B_1. Let $D_0 = D$, and let D_1 denote the average risk of the reinsurance contract. If B_1 again wants to buy reinsurance from a company B_2, let D_2 denote the average risk of this re-reinsurance contract, etc. The idea of iterating reinsurance was proposed by Wittstein. Hausdorff has computed D_0, D_1, D_2, and D_3 for the normal distribution. Bohlmann [13] studied this more systematically. He proved: The sequence $D_0, D_1, D_2, \ldots$ is decreasing with limit 0, and the sum of these numbers is $\mathrm{Max}(x_i, i = 1, ..., n)$.

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Goldschmidtstr. 7, D–37077 Göttingen, Germany
E-mail address: krengel@math.uni-goettingen.de