Studies in probability and statistics

vol. 27

Berlin, 2021

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I

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Decoding Cardano's Liber de Ludo Aleae

Hist. Math., vol. 32, 2005, pp. 180 – 202

Abstract. Written in the 16th century, Cardano's *Liber de Ludo Aleae*[1] was, in his time, an advanced treatment of the probability calculus. At the same time it could be viewed as a gambling manual. Several commentators on the book concluded that it was a mishmash of several sometimes contradictory results and statements written over an approximate 40-year period. Here, the LLA is examined as a Renaissance text written in the intellectual milieu of humanism. A close examination of the book shows that it was heavily influenced by Aristotle's *Ethic*, in particular his concept of justice. By reading the LLA in this way, it is shown that there is an internal consistency to the work with a common thread of justice (*ius*) and knowledge (*scientia*) running through it. These themes are examined in detail. It is also argued that some of Cardano's probability calculations related to dice might have been taken from a printed version of the late medieval poem *De Vetula*.

1. Introduction

Until the publication in the mid-17th century of Huygens's highly influential work on probability (1657), Girolamo Cardano's LLA written in the 16th century (1953, 1966) was the most complete treatment of the probability calculus. Among all the aleatoric calculations prior to the 1650s, it is only in Cardano's work that we find a discussion of the actual calculations and the assumptions behind them. Cardano calculated probabilities for the sum of the faces that show on two and three dice as well as some simple calculations related to card games of the time. He also provided a simple version of the multiplication rule for probabilities using dice. In one sense Cardano went beyond Huygens who devoted himself only to the calculation of chances, although he did use new concepts such as expectation. The LLA contains rules for games as well as advice on how to protect oneself against cheating. Cardano discussed methods of cheating that include false dice, marked cards, palming cards, tilted gaming tables, and the use of kibitzers. Although it was written in the 16th century (there is an internal reference that yields completion of the work in 1564 or later), it is unclear why the manuscript was unpublished in Cardano's lifetime, reaching print only in 1663, see Cardano (1966). Since it was unpublished, it is also unclear what audience did he intend to reach.

Cardano not only anticipated the development of the probability calculus, but was also the forerunner of a genre of publications that could be described as gambling manuals, or manuals of games. Like the probability calculus, these did not appear until the second half of the 17th century, and like the probability calculus, they developed much more fully in the 18th century. For example, in the English literature the first complete gambling manual was Cotton (1674), and the apex of this literature in 18th century England was Hoyles (1743). Consequently, the LLA has been viewed as a probability text, albeit a primitive one, or as a gambling manual with some helpful probability calculations in it.

A translation of the LLA into English (1953) revived the interests of historians of probability in this work. They have analysed the book mainly for its treatment of the probability calculus and have ignored or downplayed most of the other material in it. One such example is Mora Charles (1981) who translated extracts from the LLA into Spanish and for the most part provided a mathematical analysis of the work. Typically, historians of probability have asked questions such as *Are the calculations correct*? *And to what extent did Cardano anticipate later writers on the subject*? The other material gets brief mention and is sometimes treated in a pejorative fashion, such as Ore's (1953) discussion of Cardano's approach to luck. Franklin (2001 [no page number either here or in many other cases]), for example, has provided a good summary of the overall impact of the LLA on modern readers:

It is a confusing work; it is often not revised well enough to make the author's intention clear, and there remain in it sections explicitly contradicted by later ones.

Certainly, Cardano was not particularly concerned about consistency in his writings. Jensen (1994), for example, has examined the inconsistency in some other of Cardano's writings, in particular, the *De Subilitate* (Sub) and the *De Rerum Varietate* (Rerum).

Aristotle's influence on Cardano's writings has been known for some time. Margolin (1976), for example [these two words occur without fail], has looked at Cardano's interpretation of Aristotle in Cardano's works such as the two named above. Some recent efforts have been made to connect the LLA to its Renaissance context. Tamborini (1999) discussed Cardano's approach to luck or *fortuna* in the LLA and related it to some Aristotelian concepts. In spite of these kinds of efforts, much more can be done to understand the LLA in context. By not reading it as a Renaissance text, many analysts of this work have overlooked its basic structure and have not fully appreciated the source material that Cardano relied upon.

A secondary benefit to putting the LLA in its Renaissance context is the discovery of likely connections to earlier probability calculations. The typical interpretation of the calculations prior to the Pascal – Fermat correspondence of 1654 has been that these calculations were disconnected, almost random occurrences. There is a very strong evidence that Cardano's initial dicing calculations were based on a reading of *De Vetula,* a medieval poem first written ca. 1250[2].

2. Girolamo Cardano (1501 – 1576) and his educational background

Before proceeding with an analysis of the LLA it is useful to present a brief biography of Cardano and to describe some of the mathematical milieu in which he worked and studied. Besides his work in mathematics, Cardano was widely known in his day for his work in medicine and astrology. Extensive biographical material on Cardano may be found in Fierz (1983), Ore (1953) and Rose (1975) as well as in his own autobiography (1930). The combination of those three fields is, in a sense, natural to this time period. Medieval university professorships in mathematics were actually professorships in mathematics, astronomy and astrology. Astronomy and astrology were seen as more important, with a background in mathematics serving as a preparation for the study of these subjects. Professorships in mathematics alone emerged by the end of the 16th century and astrology was very much on the decline by that time [Tycho, Kepler!] although serious study of it continued well into the 19th century. Astrology and medicine were also closely connected, with astrology playing the role of servant to medicine. The positions of celestial objects were thought not only to have an influence on the lives of people in general, but also specifically to have an influence on the progress of disease. Knowledge of astrology could then be used in a variety of cures (Grendler 2002, pp. 408 – 409; Grafton 1999, p. 42). Some of the mathematical, astrological and medical elements of Cardano's career are seen in the LLA.

There are three strands that contributed to the development of mathematics during the Renaissance. The first is the development of commercial arithmetic, which led to an increasingly numerate population. The second is the development of other branches of mathematics, particularly geometry and related areas, through the recovery of classical mathematical manuscripts. The final strand is the teaching of mathematics at the universities. Cardano's mathematical work has ties with all three of these strands.

The study of commercial arithmetic in Italy grew substantially between the 13th and 16th centuries. The need for this arithmetic and the concomitant introduction to the West of the Hindu – Arabic number system resulted from increased trade in the Mediterranean area with the Muslim world (Lieber 1968; Mack 2002). Out of this interaction between Muslim and Italian traders, Italian merchants learned Muslim commercial practices such as bills of exchange and the recording of business transactions. To train merchants in these trading practices, schools of arithmetic or abbaco [abacus] schools were started in many Italian city-states (Grendler 1989). Associated with these schools were arithmetic or abbaco books. Van Egmond (1981) has provided an extensive list of these books up to the year 1600 with Leonardo of Pisa's *Li-ber Abaci* (Fibonacci 2002) the earliest on the list. The abbaco books were usually meant as manuals for teachers or for merchants already working in trade, rather than as student textbooks. These books contain discussions of the basic arithmetical operations of addition, subtraction, multiplication and division as well as discussions of fractions and the extraction of square and cubic roots. To this point the abbaco books can be viewed as strictly derivative of Arabic arithmetic books. The difference between the two is that the arithmetic in the Arabic books is followed by a development of mathematics for astronomy. See for example a 10th century Arabic arithmetic by Uqlidisi (1978). The Italian abbaco books take a different direction, often going well beyond the basic arithmetic operations by including, for example, business problems and recreational mathematics problems. These problems were often, but not always accompanied by a discussion of elementary geometry and algebra as well as miscellaneous material such as calendars and astrology (Van Egmond 1981). The geometry in the abbaco books is mostly arithmetical, dealing with lengths, areas and volumes rather than Euclidean in the sense of abstract mathematical proofs of geometric relationships (Peterson 1997). The rise of Venice as the centre of trade is tied to the publication of abbaco books. According to the data in Van Egmond (1981) between the earliest printed abbaco book in 1477 and 1600, the terminal date for data collection, 55% of all editions of abbaco books were printed in Venice. The next highest percentage was Naples which had less than 8% of the total.

Mathematics was taught in the medieval universities as part of the quadrivium composed of arithmetic, geometry, astronomy and music. The leading Italian universities in mathematics were Bologna and Pavia, in that order. Following on the centuries-old curriculum of the church and cathedral schools, the writings of Boethius (ca. 480 – 524) were central to the teaching of arithmetic and geometry in the medieval university. His arithmetic is not what we would call arithmetic today. It involved little or no calculation and instead was confined to the study of the properties of numbers including ratio, proportion and fractions (Kline 1972; Masi 1983; Schrader 1967). In the 12th and 13th centuries the curriculum changed slightly as some major Greek mathematical texts coming from Arabic sources were translated into Latin, Adelard of Bath’s translation of Euclid being the prime example. The material in the university curriculum was transformed even more during the Renaissance as more mathematics manuscripts from antiquity were discovered and translated. The recovery of antiquity with respect to mathematical manuscripts is described in detail in Rose (1975). The major mathematical works in their original Greek were by Euclid, Archimedes and Apollonius. The work of Archimedes had a distinct impact on the applications of mathematics in the Renaissance, but not on the university curriculum (Laird 1991). Taken over four years, the typical Renaissance curriculum in mathematics was Euclidean geometry and Ptolemaic astronomy. In the first year there was a treatment of arithmetic and algebra as well as the introductory topics in geometry and astronomy. Subsequent years covered more advanced topics in geometry (later books of Euclid, for example) and astronomy. Also within this curriculum were topics in astrology because of its perceived relationship to medicine. Grendler (2002) has described the typical early Renaissance curriculum in mathematics and the changes that occurred during the Renaissance. On completion of the quadrivium students could pursue higher studies in law, theology or medicine as doctoral degrees.

Cardano was in part a product of the university system. He himself (1930) has described his education in mathematics. Initially Cardano’s father Fazio, a lawyer, had taught the son arithmetic and the first six books of Euclid at home. Fazio Cardano was himself an able mathematician. In addition to his legal work, he lectured on geometry at the University of Pavia and at the Piatti foundation in Milan. At the age of 18 (about 1520), the son Girolamo entered the University of Pavia where he would have followed the quadrivium. Three years after that Cardano gave public lectures in Euclidean geometry. Lecturing and disputation were part of the normal learning process for a university student of the time. Students were required to give lectures and public disputations on a variety of topics and questions prior to their examinations for their degrees (Grendler 2002). Cardano left Pavia because of war in the district and stayed at home with his father for about two years. He entered the University of Padua in 1524 where he completed his studies in medicine.

Cardano's first publication in mathematics (1539) shows him straddling both worlds of the abbaco school and the university. The work is listed in Van Edmond's (1981) extensive catalogue of abbaco books and manuscripts, and so this publication may be classified in part as an abbaco book. It is both typical and atypical of the genre. Cardano had never been a teacher in an abbaco school, nor had he learned his mathematics in such a school. What we have is an individual who had no experience as a merchant and who had not been formally trained in commercial arithmetic, writing a book that contained that arithmetic. To a certain extent it shows. The book was written in Latin rather than the vernacular, as the vast majority of Italian abbaco books were written. It was also written at a *higher level* than the normal abbaco book. Smith (1970) has described the 1539 book as

One of the most pretentious arithmetics of the 16th century, but one that *did much to influence the advanced teaching of the subject*.

It shows up in the Bodleian library catalogue of 1620 (James 1620) and so probably became part of the new arithmetic curriculum at some universities. Cardano probably had a different market in mind than a reference manual for abbaco teachers and merchants. He probably wanted to advertise his mathematical skills to a wider and more mathematically adept audience across Europe. Indeed, Maclean (1994) has asserted that Cardano's reason for writing that 1539 book was a mixture of self-promotion and money. The 10 crowns he received from the publisher was probably used to finance the publication of some of his works in astrology, which in the long run might have been more lucrative to him. He may have also used the 10 crowns to obtain a protective license from the Holy Roman Emperor to publish certain books he had written, in particular the books given on a list printed in the 1539 publication.

Sixth on Cardano's list of 34 books that he was ready to publish was one on games, *De Ludis*. Cardano had been gambling seriously from about 1525. A quotation describing his early gambling in his own words appears in Ore (1953). David (1962) has provided a reference to the quotation placing it in the 1551 edition of the Sub. At the same time that he started gambling seriously, Cardano began collecting facts about games. Later he expanded the collection of facts into a book written in the vernacular, the *De Ludis* listed in 1539. The book was divided into four parts, of which the second was about games of chance. Tamborini (1999) has listed several references to this work throughout Cardano's writings. Since we have only the 1663 printing (Cardano 1966) of the *De Ludo Aleae* (LA) manuscript, it is impossible to establish the exact relationship between the two books.

In the past and, for some, to the present day, many have viewed Cardano as a superstitious charlatan. This view originates with Gabriel

Naudé who wrote the preface to the first publication of Cardano's autobiography *De Propria Vita Liber* (Vita) in 1643. Jean Stoner (Cardano 1930, p. xiii) who translated the autobiography into English, has summed up Naudé's position:

Gabriel Naudé edited the book with a prefatory indicium which had long influenced every estimate and every picture posterity has held of the Milanese[3]*, for he implies that Cardan was a moral monster in general and in particular superstition ridden and careless of the truth*.

The charge of superstition probably came from Cardano's work in astrology and from his interpretations of dreams. The label of charlatan comes from both his astrological and mathematical work. He cast a very positive horoscope for the young King Edward VI of England: Edward died shortly thereafter. In mathematics he has come out badly in his dispute with Tartaglia over Cardano's publication of the solution for the roots of the cubic equation. His solution was published in his major mathematical work (1968). Feldmann (1961) has documentted the dispute and has put Cardano in a much better light than earlier interpretations. In the past few years general opinion on Cardano has become more positive and several scholars have now studied different aspects of his career. See Grafton (1999) on Cardano and astrology and Siraisi (1997) on Cardano and Renaissance medicine as well as a collection of articles on a wide range of his work edited by Kessler (1994).

3. The Liber de Ludo Aleae as an argument

Beginning with Ore (1953) this writing has been analysed carefully, and in detail, as a mathematical work. It was examined in less mathematical detail by Todhunter (1865) who was critical and dismissive of the work. One of the major issues that had never been addressed by historians is that Cardano himself did not view it as a mathematical work. In his autobiography Cardano (1930) listed his work on games of chance under the heading *Various Arguments* rather than among his mathematical works. If he considered this book to be an argument of some sort rather than a work of mathematics, what argument was he making and on what subject? A variety of arguments are given in the book, but the question he addresses is not clearly stated in the text. Since the book is about games of chance and the gambling associated with these games, it is useful to look at what other arguments were made about such games during the Italian Renaissance.

In general, there was no blanket condemnation of gambling rather the treatment of this issue was mixed. Prior to the Renaissance, Thomas Aquinas (1225 – 1274) took up the question of gambling during his discussion of almsgiving in his *Summa Theologiae* (2a2e.32.7: Thomas Aquinas 1975). He made a distinction between civil and divine law. With respect to the Church, he wrote:

For in the first place, some things are forbidden by divine law: for instance, winnings at the expense of minors and those out of their minds, who have no power to alienate their property; or out of sheer greed to induce someone else to gamble; or again, to win by cheating.

He noted that in some cases civil law also prohibits gambling. Aquinas went on to say that not everyone is subject to civil law and that the law may become outdated and changed. Later, in a discussion of avarice, he (2a2e.118.8), 1972, referred to the connection Aristotle made between dice players and this vice. The appropriate quotation is from *Ethics* (Book IV, i, 43; Aristotle 1955):

But the dicer and the pickpocket belong to the illiberal class, because they are sordidly avaricious: it is for gain that both types follow their profession and submit to a bad reputation, the one accepting the greatest risks for the sake of their pilfering, the other profiting at the expense of their friends, to whom they ought to give, so both are sordidly avaricious, because they want to make gain from a wrong source.

This quotation was an important impediment to Cardano in his argument about games of chance and gambling. As will be seen in § 4 Cardano used Aristotle to support his argument about games of chance, By contrast the quotation above is distinctly negative.

Near the beginning of the Italian Renaissance Petrarch (1304 – 1374), see Petrarca (1991, Book I, 26 and 27 and Book II, 16) made several arguments against gambling and games of chance. Among these arguments is one that comes close to Aristotle's reasoning: Petrarch claimed that the winnings from gambling are illicit gains. The gains to be made from gambling are also unstable. Putting words into the mouth of *Reason*, he stated:

There is no profit in gambling, only evil and misery, because he who loses suffers and he who wins is tempted and lured into the trap.

Pietro Aretino (1492 – 1556) made a similar point in a diametrically opposed context. In his pornographic work (1971, pp. 222 – 223) the character Nanna tells her daughter Pippa how to be a good prostitute. She advises Pippa to stay away from gambling and to advise her men to stay away as well. Men who lose their money in gambling would be unable to shower money on her. Petrarch also admitted to some of the benefits of dice games but immediately downplayed these benefits.

Others played up the positive aspects of games of chance. The Roman humanist and Vatican librarian Bartolomeo Sacchi (1421 – 1481) writing under the name Platina praised games of chance in the context of a meal. Unless cheating is involved, playing games of chance after supper aids in digestion (Platina 1998, p. 109).

The individual who came closest to the question that Cardano asked is Baldesar Castiglione (1478 – 1529), an Italian courtier, soldier and diplomat. Based on his experience in the court of the Duke of Urbino, he wrote the highly influential Renaissance courtesy book (1967) whose perfect courtier became a model for the educated classes of Europe. Castiglione was concerned with maintaining the façade developed by the courtier. The following dialogue discussing whether or not a courtier should play at games of chance is taken from that book (p. 140):

It seems to me, replied Federico, *that we have given the courtier a knowledge of so many subjects that he can readily vary his conversation a great deal and adapt himself to the qualities of those with whom he has dealings, assuming that he possesses good judgement and allows himself to be ruled by that, and depending on circumstances, attends sometimes to grave matters and sometimes to festivities and games*.

And which games, asked signor Gaspare.

Federico answered with a laugh: *For this let us go for advice to Fra Serafino, who invents new ones every day*.

Joking apart, answered signor Gaspare, *does it seem to you that it is wrong for a courtier to play at cards and dice*?

To me, no, said Federico, *unless he does so too assiduously, and in consequence neglects things of greater importance, or indeed for no other reason than to win money and cheat his partner, and then, when he loses, is so dismayed and angry as to prove his avarice*.

Cardano, an avid gambler for much of his life, asked a question similar to signor Gaspare's. Rather than the question being specific to the courtier, he asked for different reasons,

Does it seem to you that it is wrong for a man to play at cards and dice?

Another way of phrasing the question is in terms of the Renaissance concept of justice:

Under what conditions can the act of gambling at card and dice be considered a just act?

Castiglione gave his own answer in the final sentence of the quotation; Cardano took several pages to make his arguments, and adressing all the points made by Castiglione and many more. Chief among Cardano's additional points is that he used mathematics to argue when it is not wrong to play at games of chance. That the question addressed in his argument concerns justice id evident in his approach to games of chance. At the beginning of Chapter 6 of LLA Cardano (1953) stated his basic assumptions:

The most fundamental principle of all in gambling is simply equal conditions, e. g., of opponents, of bystanders, of money, of situation, of the dice box, and of the die itself. To the extent to which you depart from that equality, if it is in your opponent's favour, you are a fool, and if in your own, you are unjust.

The key word is *unjust*. In the previous chapter Cardano had given his reasons for writing this book. First he said that gambling has useful features and that it has some advantages. The word he used is a form of *utilitas*, which also translates to utility. The ancient and later Renaissance concept of utility was tied to the concept of justice. For example, Cicero (*De Officiis* 2000, Book II, 10 and 20), a popular author in the Renaissance, said that what is just is also useful and what is useful is also honourable. A useful act is also just so that when Cardano was looking to the useful features of gambling he was also looking into the parts of gambling which are just.

It is impossible to say whether or not Cardano took the question he was arguing directly from Castiglione (1967). The existing evidence is circumstantial. First, Cardano claimed an indefinite family connection with Castiglione. In his autobiography he (1930, p. 1) wrote of his ancient and noble lineage, hinting that the Cardano family was really a branch of the Castiglione family. Second, there is a connection of opposites between Cardano and Castiglione in terms of their approach to courtiers. Castiglione was himself a courtier, while Cardano appears to have despised courtiers. Early on he LLA advised that a prince should not gamble. On this point Castiglione and Cardano are in basic agreement. Cardano's discussion shows his attitude to courtiers:

This fault is particularly detestable in princes and is defended by no one except courtiers and flatterers of the prince, who do it either from fear or because they receive gifts if the prince is lucky.

Writing some years later in his autobiography Cardano (1930, p. 124) made another revealing statement about his attitude to courtiers. He claimed that he never searched out honours saying that the search usually brought grief:

Again, a zeal for honours urges us to the verge of death itself by ways too numerous to recall – duels, wars, quarrels, disgraceful litigation, attendance upon the favours of princes …

The initial form of Cardano's argument concerning the justice of gambling and games of chance is part of the source of confusion about the work. After a brief introductory chapter in the LLA that describes games in general, Cardano opened his argument in Chapter 2, but did not say what argument he was making or what is was about. Rather, he began by listing some of the pros and cons of gambling. They alternate one by one. Gambling is permitted at funeral banquets, but is also condemned by the Titian and Cornelian laws from ancient republican Rome, Cardano said. Play at cards and dice is beneficial during times of grief, stress and anxiety. On the other hand one's time is better spent doing more worthwhile activities. This method, giving both sides of opposing positions, is the method of arguing *in utramque partem*. It was a method of argumentation that is based on Ciceronian rhetorical methods and principles. It was very popular among Renaissance humanists. Cardano returned to this method of argumentation in a short Chapter 4. There the pros were grouped together followed by a group of cons.

Franklin (2001) has called attention to contradictions in the LLA, some of which result from the argument *in utramque partem*. For example, in Chapter 2 Cardano wrote that gambling *arouses anger and disturbs the mind*, and then in Chapter 4 asserts that gambling can provide the opposite, relaxation from anxiety. In a similar vein Cardano advised playing only for small stakes in Chapter 3, but in the following chapter stated that the beauty of large stakes is that it can provide insight into the character of an opponent. It may seem confusing to us today, but for his time Cardano was using a standard form of argumentation.

4. Aristotle's concept of justice and Cardano's probability calculations

Justice is the major theme of the LLA. Not only did it motivate how Cardano approached all his probability calculations, but it also guided his approach to subjects such as cheating in games of chance. His fundamental principle of gambling in the LLA rests on equality and hence on justice. Usually equality is taken to mean equal chances for the players in a game of chance. Cardano, on the other hand, took equality well beyond equal chances in the probabilistic sense. The idea for this fundamental principle comes directly from Aristotle's *Ethics* (Book V, iii, 5 – 6 [1955, pp. 177 – 178]). Aristotle defined what is unjust as what is unequal and what is just is what is equal. He went on to say that

A just act necessarily involves at least four terms: two persons for whom it is in fact just, and two shares in which its justice is exhibited. And there will be the same equality between the shares as between the persons because the shares will be in the same ratio to one another as the persons. For if the persons are not equal, they will not have equal shares, and it is when equals have or are assigned unequal shares, or people who are not equal, equal shares, that quarrels and complaints break out

That Cardano relied on Aristotle for his definition of the fundamental principle of gambling is confirmed by his discussion of games in which the participants have unequal chances to win. In this discussion he used the term *circuit* to describe the *sample space* or the set of possible outcomes of the throw of the dice. He (1953, p. 18) wrote:

Other questions must be considered more subtly, since mathematicians also may be deceived, but in a different way. I have wished this matter not to lie hidden because many people, not understanding Aristotle, have been deceived, and with loss. So there is one general rule, namely, that we should consider the whole circuit, and the number of those casts which represent in how many ways the favourable result can occur and compare to that number to the remainder of the circuit, and according to that proportion should the mutual wagers be laid so that one may contend on equal terms.

This is a direct application of Aristotle's rule for a just act. In modern terms, suppose that one player is wagering an amount x against another player who is wagering y. The probability of the first player winning the wager is p and so the probability of winning for the second is $1 - p$. In the modern context, the game is fair or just if the expected gains of both players are the same, i. e., […]. This can be written as $y/x = (1 - p)/p$ which is the same as saying that the ratio of the wagers must be the same as the ratio of the chances of winning, or Cardano's rule. This follows Aristotle's prescription that *the shares will be in the same ratio to one another as the persons*, where the shares are equivalent to the stakes and the measure of *justness* of the individual is the probability of winning.

Historians of probability have looked to Aristotle when searching out the genesis of probabilistic ideas. See for example Sambursky (1956), Sheynin (1974), Hacking (1975)[4] and Styan (1998). Typically, they have concentrated on Aristotle's ideas of the meaning of a chance event and the subsequent Scholastic interpretations of chance. They have also looked to the evolution in the meaning of probability as it applies to an argument that is probable or has reasonable grounds for acceptance. Cardano's use of Aristotle is entirely different, relying on the definition of justice rather than chance.

The mathematical discussion of games of chance begins in Chapter 9 of the LLA where a basic description of dice is given. Cardano described two kinds of dice, the regular die of six sides and the *talus* or *astragalus*, a die with four sides typically made from the knucklebones taken from the hind legs of sheep or goats. At this point in the book only a passing reference is given to the *talus*. The main discussion is concerned with the regular die of six sides. Central to Cardano's initial mathematical argument is the circuit or the modern day sample space as it applies to dice. There are six sides to the die and Cardano reasoned that the die should complete the circuit of all six possible faces after six throws of the die. Later, in Chapter 15, he was very explicit about the concept of the circuit, saying

The magnitude of the circuit is the length of time which shows forth all forms.

This is a rather odd definition in view of its strictness. Cardano also knew that this definition did not hold empirically. At the beginning of Chapter 9 he stated:

The die has six [faces], *in six casts each point should turn once; but since some will be repeated, it follows that others will not turn up*.

Based on this initial definition of the circuit, Cardano described the number of possible outcomes in the circuits for two and three dice, 36 and 216 respectively. After some discussion about some of the outcomes in the throws of these dice, he calculated in Chapters 10, 11 and 12 the probabilities of the points in a game called *Sors* and another called *Fritillus*. The former game is straightforward. The points are the sum of the faces that show in a throw of the dice, be there two or three. Cardano's calculations for *Sors* are correct, see Table 1 for three dice. The point system for *Fritillus* is more complicated. Ore (1953, p. 161) has given a reconstruction of the rules of the system.

Table 1 lists the number of chances for sums 3(1)18 of points.

Ore (1953) has also given a detailed analysis of Cardano's mathematical probability calculations on dice. One of his major insights into Cardano's mathematical argument is a method purportedly used by him. Ore called it *reasoning on the mean*. For a single die Ore's reasoning goes as follows. The probability that any particular face shows in the throw of a single die is one in six since the length of the circuit is six. If the die is thrown three times then the expected or mean number of times the face shows is 1/2. From this Cardano concluded that there are equal chances for one particular face to show at least once in three tosses of the same die. In translation, Cardano (1953, Chapter 9) expressed his reasoning as follows:

One half of the total number of faces always represents equality. Thus the chances are equal that a given point will turn up in three throws, for the total circuit is completed in six, or again that one of three points will turn up in one throw.

Cardano's reasoning is incorrect: the probability of obtaining an ace, two or three in one throw of a single die is 1/2, for example, while the chance of obtaining at least one ace in three throws of a single die is 91 in 216 or 0.42. Ore's comment on this particular derivation is:

The value is fairly satisfactory since the correct figure [the number of tosses of the die required to obtain a probability of 1/2 that a certain face shows at least once] *lies between* 3 *and* 4. *It may well have been this result, conforming to his gambling experience, which brought Cardano to place so much faith in his reasoning on the mean. Also in other in-stances one can see that he attempts to generalise on evidence which is very slim indeed*.

From a modern perspective Ore's approach is a very nice interpretation of what Cardano was trying to do. The problem with the analysis is that it is based on a well-developed concept of expectation, a concept that Ore has perhaps read into Cardano's writings rather than extracted from them. I would contend that Cardano's initial and incorrect arguments based on the circuit and Ore's *reasoning to* [on!] *the mean* is tied to Aristotle's concept of justice, which Cardano interpreted as equiprobable outcomes under equal stakes. There are six faces on a single die, of which three, for example, the ace, two and three, are of interest. The ratio of the total number of faces to the faces of interest is 6/3 or 2. There are also six possible casts of the die. To maintain the same Aristotelian ratio 6/3 for justice Cardano assumed that the number of casts of interest (for example, containing at least one ace) should be 3. Cardano has incorrectly made the correspondence between the three different faces in a single toss and a single face in three tosses. At this point in the LLA Cardano wanted his mathematics to force events to be equiprobable or just, rather than having the mathematics show where the justice lies. It was not until later in the book after these initial incorrect calculations have been given that Cardano realised his error in mathematical reasoning and calculated the chances correctly. As Ore has shown, these incorrect calculations, whatever their philosophical origins, provide insight into some of Cardano's analyses of the point systems in certain dice games. Ore has also noticed that Cardano has made some attempts to reconcile the correct calculations with calculations based on reasoning on the mean. If we interpret the reasoning as toward justice instead, we can see why Cardano's more obscure statements in the LLA would, in Ore's words,

Be concerned precisely with the problem of bringing the two points of view [the correct method and reasoning on the mean] *into harmony*.

Cardano was concerned that the correct calculations show a just act. The approximate agreement between the two approaches may have led Cardano (1930, p. 195) to comment in his autobiography,

It is all like trying to calculate one's chances in gambling; the system comes to naught or is ambiguous.

Cardano made some minor contributions to the calculation of chances in card games, in particular the game primero. He treated a type of division-of-stakes problem for primero, but one that is simpler than the classic problem of points, a problem solved by Pascal and Fermat that led to the formal development of the probability calculus. In primero, when two players remained in the game with a card draw each left to be made, the player with the lower number of points in his hand could ask for a *fare a salvare*. At that point in the game the pot could be divided into two parts if the player asked for it. One part of the pot was split, usually evenly between the two players, and the other part was played for and taken by the winner. To maintain justice in the game, Cardano stated that the *fare a salvare* should be decided upon before the game begins since the actual play of the game may provide information on what cards are outstanding so that the underdog may find it sometimes advantageous and sometimes disadvantageous to invoke this rule when the decision is left entirely to him. Using his mathematical criterion of justice and basing his calculations on the chances that the outstanding cards to be drawn will lead to a win, Cardano showed that the division rule favours the underdog.

There are no calculations regarding the chances of each type of hand in primero. These can be obtained by some relatively simple combinatorial calculations similar to poker hand probabilities. Upward of six years after the completion of the LLA Cardano had developed the necessary mathematical theory to make these calculations. As part of a larger work Cardano (1570) published techniques using an arithmetical triangle to calculate the number of combinations of objects taken from a group of dissimilar objects. The chances of card hands can be calculated by extending and using Cardano's triangle. Neither card games nor any other games of chance were among the examples that he used. Boyer (1950) has incorrectly stated that Cardano applied the results of his arithmetical triangle to games of chance. Instead, Cardano used as his example the selection or arrangements of ten men sitting to dinner at a table, which was a typical problem appearing in a number of previously published mathematical books. For example, Edwards (1987) referenced three others who dealt with counting the number of ways 10 men can sit down at a table.

How did Cardano make the connection between Aristotle's definition of justice and the calculation of chances? One possible explanation might be found in Cicero's *De Officiis* (2000). There Cicero provided the solution to what is now known in philosophy and law as the lifeboat problem. The modern problem may be stated simply as follows. There are several people in a lifeboat that will sink unless one person is thrown overboard. Who should be picked? Broome (1984) has described the original historical situation in which the ship's mate made the decision to throw several men overboard while saving women and children along with the crew. Cicero's scenario was different, but the question essentially the same. He described the problem and solution as

Another question: assuming that there is one plank and two shipwrecked passengers, both of them wise men, should each try to grab it for himself or should one yield to the other? One should give way, yielding to the one whose life is more important whether intrinsically or to the state. But supposing the balance is equal on both sides? Then there will be no contest. One will yield to the other as if in a lottery or a game of chance.

When the two wise men are equal, the decision is left to pure chance or an equiprobable event. In a trial arising from the original lifeboat problem that occurred in 1841, the judge also decided that the choice should have been by lot.

5. Scientia and its relation to justice

Since Cardano was relying on Aristotle's concept of justice (Book V; Aristotle 1955) to support his arguments in favour of gambling, Aristotle's negative attitude to gambling as quoted in § 3 was something that Cardano had to deal with. The issue, as Cardano set it out in Chapter 10, is the nature of gain. He categorised various kinds of gain from gambling and said that certain kinds of gain through gambling are acceptable. Gould, the translator of the LLA, expressed Cardano's version of the best kind of gain as *from those who are willing and aware*. For some, the meaning of this translation can be a little obscure. […] Gould translated *volentia* as willing; it can also mean *inclination*. Likewise, *scientia* [included in the omitted Latin phrase] might be more commonly translated as *knowledgeable* or *skilled*. Consequently, another way of translating the passage is that the best kind of gain in a gambling situation is from those who have the inclination to gamble and who are skilled or knowledgeable in the game. Cardano ran through the various kinds of gain within his categorisation, describing unacceptable gain as that which is taken from those who are unwilling or disinclined to play and at the same time are unskilled in play. This categorisation of the various types of gain to be made from gambling handles the various objections to gambling, including those of Petrarch [§ 3]. After defining gain in gambling, Cardano returned to the method of *in utramque partem* to continue his argument. Citing Aristotle, he stated that gain from gambling is base gain and is therefore sordid and unacceptable. Then he finished his argument by taking the other side giving an argument in favour of gambling. Cardano claimed that the Church did not condemn gambling as such, being concerned mainly with the blasphemy that might accompany the act of gambling. The early Church had generally condemned gambling and games of chance. This has changed as canon law in the medieval Church developed. In addition to the restrictions noted by Aquinas (§ 3), the only general prohibition on gambling in Cardano's day came from the decisions of the Fourth Lateran Council of 1215 in which the clergy were prohibited from playing games of chance or to be present at them. See, for example, the article *Gambling* in Herbermann (1907).

The key to responding to Aristotle's objections to gambling is *scientia*. Knowledge, skill, or *scientia* is the second theme of the LLA. Cardano provided the reader with knowledge of games so that the reader with *volentia* will have the *scientia* to play. Whether the gain goes to the reader or to his opponent in a just game, it can be considered the best kind of gain.

The promotion of *scientia* and its relation to justice is apparent in Cardano's first treatment of methods of cheating. Their first mention in the LLA is related mainly to cards games and appears in Chapter 6 entitled *The fundamental principle of gambling*. The discussion in this chapter begins with the fundamental principle as quoted in § 3. Then follows a description of kibitzers and other types of onlookers at games of chance. These people can provide information to an opponent or cause distractions making a player lose his concentration on the game. Cardano set the tone for his view of cheating when he made his first comments about kibitzers:

And so it happens that, if you play in a large crowd of people, you can scarcely avoid folly if they are against you, or else injustice if they are for you.

The connection to justice is explicit here. In his later descriptions of cheating at cards and dice in Chapters 17 and 7 respectively the connection to justice is not made explicit. What Cardano did in all these situations was to provide information about how cheating occurs so that the player can be aware (or have *scientia*) of these methods to protect himself and to avoid folly. At the same time the player should not exercise this knowledge in such a way as to lead to injustice.

Here is one example of how Cardano approached the subject of cheating. Fair dice are perfect cubes. He described three alterations that result in biased dice. Two results from altering a fair die. Through alteration one of the corners or edges of a cubical die can be rounded off, thus giving an advantage to one of the faces on the opposite side of the die. The other alteration is to apply pressure to opposite faces of the die. This will tend to make these two sides flatter in comparison to the remaining four, thereby giving advantage to the two flattened sides. The third alteration is to construct a non-cubical die that has two opposite faces square in shape and the remaining four faces rectangular in shape, leading to a bias away from the square faces. To combat these kinds of cheating with dice Cardano advised looking at the three sets of the opposite sides of the die to determine whether or not it is a true die. Curiously, he did not mention loaded dice.

Cheating at games of chance was not unique to Cardano's circle or to Italy in general. With the exception of the dice with rounded edges all the methods of cheating at dice, including loaded dice, are mentioned in the 16th century English literature of roguery. See Bellhouse (1993) for a description of this literature and its possible relation to the history of probability. This literature also mentions methods of cheating at cards, including stacking the deck and various methods of legerdemain to bring forward a desired card. Cardano has added to this list rings with mirrors on them to see the cards as they are dealt face down. He also included marked cards ad the soaping of cards to make them slide better over one another. In line with the promotion of *scientia*, Cardano gave advise about examining the deck of cards to guard against the typical methods of cheating.

The use of legerdemain at cards is linked to Cardano's work as an astrologer. In Chapter 17 of the LLA there is a description of cheating at cards through legerdemain. A more detailed description of this type of card sharping is given in Book 18 of Cardano's Sub. A translation of the appropriate passage is in Maxwell-Stuart (1998). What is of interest in the Sub passage is that Cardano watched the card sharper very carefully to see if he could figure out how the trick was done. He could not work out the trick but still concluded that the sharping was due to legerdemain rather than to spirits or to magic. It is apparent that Cardano did not believe in magic and divination through a randomising device. Rather, he was very careful to distinguish between true divination and pure chance. His work in astrology and the astrological milieu in which he worked is described in Grafton (1999).

According to Cardano, the best type of gain a person can make in a game of chance is from those who are both willing to gamble and skilled at the game. He promoted legitimate skill and did not equate highly skilled players with cheaters. Among the skills he saw as legitimate was the use of memory. In Chapter 23 Cardano recognised the importance of remembering what cards have been played. Earlier in Chapter 17 he stated that

Those, however, who know merely by close attention what cards they are to expect are not usually called cheats, but are reckoned to be prudent men.

Cardano condemned some of the obvious skills, already mentioned, of cheating at cards and dice. Some of the skills he condemned were not necessarily universally recognised as illegitimate. For example, Castiglioni (1967, Book IV) wrote:

For it is impossible to govern either oneself or others well without the help of God who to the good sometimes sends good fortune as His minister, to protect them against grave dangers, and sometimes adverse fortune to prevent their being so lulled by prosperity that they forget Him or human prudence, which often offsets ill fortune as a good player remedies bad throws of the dice by the way he places the board.

In Chapter 7 of the LLA Cardano condemned the practice of manipulating the gaming board as a method of cheating.

6. The LLA as a humanist document

Humanism was a major intellectual movement during the Renaissance. It was based on the belief that a study of the ancient classical texts, in particular the Greek and Roman literature, could provide a cultural rebirth (Nauert 1995). Humanists of the Renaissance were active in the recovery, annotation and publication of these ancient texts, as well as their translation, typically from Greek to Latin. Aristotle was a favourite classical author among the humanists and his *Ethics* was widely read to the point that Celenza (1999, p. 48) has commented:

If one were a humanist, then one way to achieve a connection with one's audience would have been to use terminology from the Nicomachean Ethics*, which at that point would have been fashionably familiar to the reading public*.

It is then not surprising that Cardano relied on the *Ethics* to justify his probability calculations and that he had to come to terms with Aristotle's comments on dicers to put forward his own arguments. We have already seen this and other humanist influences in the LLA. With the exception of a passing reference to humanism by David (1962), no one has looked at the LLA in the context of Renaissance humanism.

Cardano's humanism shows through from the very beginning of the LLA where there are several references to classical sources and a description of the ancient dice called *tali* or *astragali*. At this point in the book there is no mention of actual ancient games of chance. These games are treated in more detail near the very end of the book in Chapters 30 and 31 with some follow-up discussion in the concluding Chapter 32. The information about ancient games here is taken from Calcagnini (1544, pp. 286 – 300). The *talus*, as previously mentioned, is a four-sided die made from the anklebones of sheep or goats. A *tessera* is a regular six-sided die and a *calculus* is a stone. Cardano dealt with *tali* and *tesserae* only, ignoring any games with stones. His discussion of ancient dice not only underlines his humanist learning, but also reveals an attempt to relate the interpretation of dice games to Aristotle's *Ethics* through his doctrine of the mean.

In Chapter 30 of the LLA Cardano described *tesserae* and made reference to many ancient sources that were all taken from Calcagnini. Wi-thin the context of the ancients, Cardano continued with the themes of *ius* and *scientia*. He noted that the numbers on opposite faces of *tesserae* always sum to seven, which is still the case today. Hence it was easier to detect cheating with false dice that have some of the numbers one through six missing and other numbers repeated. Cardano went on to describe three other methods of cheating with dice that require legerdemain and skill at throwing the dice to get a desired result.

Chapter 31 is devoted to *tali*. With the four-sided die, the opposite faces are 1 and 6, and 3 and 4, again both summing to 7. In a typical game, four *tali* or *astragali* were thrown. After a brief description of *tali*, Cardano counted the number of ways in which each of the possible throws of four *tali* can occur. For example, there are 4 ways that the *tali* can be thrown such that the faces are all the same such as (3, 3, 3, 3), and 24 ways to get a throw with all different faces, such as (1, 3, 4, 6). What Cardano did was to enumerate the number of chances of each kind of throw. This enumeration does not immediately lead to the probability of various throws, and Cardano did not recognize this problem.

The reason for the difference between the enumeration and the probability is that the faces of the *tali* are not equally likely to throw. The flat sides show with a frequency of about 4 in 10 throws. Hagstroem (1932) who had his daughters throw the *tali* several hundred times, obtained these numbers empirically. Cardano finished this chapter by naming and trying to describe various kinds of throws. The Venus throw which was considered lucky was a throw with all different faces and Cardano noted that it was the throw with the highest number of chances, 24 out of 256. The dogs was an unlucky throw. What exactly constituted this throw is uncertain; it involves throwing at least one face showing a one.

Cardano tried to clear up the little mystery of the dogs in Chapter 32, probably by using Aristotle's doctrine of the mean outlined in Book II of the *Ethics*. He defined the mean for a thing as that which is equidistant from the extremes. He also defined it for an individual as something neither excessive nor deficient[5]. The two means are not necessarily the same and Aristotle provided an example: the range of amounts of food, with a specific mean, available to an athlete in training and the mean amount of food that is appropriate to the athlete's needs, can be different. The mean should be striven for and the virtue for individuals lies in striving for the mean. Cardano initially defined his mean (he used the word *mediocris* meaning literally *in a middle state between too much and too little*) for a set of six dice in which only one face on each die is numbered and all the numbers one through six appear for the six dice[6]. He obtained the arithmetic mean in the usual way by adding up the first six integers and then dividing by six to obtain $3_{1/2}$. In this case he seems to have been concerned that there will be a tendency to numbers below the mean. Taking the blank faces on the dice to be 0, I have calculated that the probability that the sum of the faces that show is less than the mean is 0.548 and the probability that the sum is 0 is 0.342. From a modern viewpoint Cardano's concerns are to be expected: the distribution he has constructed has a long tail on the right, so that the mean of the distribution is greater than the median. After this initial description Cardano returned to the throw of four *tali*. The calculation of the mean in this case follows Aristotle's prescription for the calculation of the mean of things, the average of the two extreme numbers. Cardano may have Aristotle in mind when he concluded that the throw of a dog must have more than one die with its face showing a one. The smallest sum for a throw of four *tali* is 4 and the largest 24, so that the Aristotelian mean is 14. If two ones and two sixes show in that throw then the sum is also 14 so that a player can never go above the mean with a throw of two ones. Although Cardano made no mention of it, the *Ethics* may also explain him why the Venus throw was a lucky throw. Not only does this throw have the highest number of chances, but also the sum of the faces is always 14, the Aristotelian mean.

Cardano may also have been trying to use Aristotle's doctrine of the mean to justify the cut points between high and low bids in different primero hands. This discussion appears in Chapter 19. The mean point (or *mediocris*) that Cardano used in any type of primero hand that he discussed is the Aristotelian mean based on the point scoring assignment face cards are worth 10 points, an ace is worth 16, sixes and sevenths get three times their value and 2 through 5 get their value plus 10 points. A hand of four kings would be worth 40 points and a hand of four sevenths, 84 points, so that the Aristotelian mean for four of a kind (or a chorus hand in primero terminology) would be 62. In his discussion Cardano tried to tie a cut point to the Aristotelian mean obtained from the range of total points in various hands. When this simple mean did not work as the cut point he tried to obtain the mean, again using Aristotelian principles, but based on the structure of the hand.

Another attribute of humanist writing is the use of classical exempla, and the LLA is liberally sprinkled with them. A detailed examination of the three reasons Cardano gave in favour of gambling listed in Chapter 4 provides a good example of his exempla.

The first exemplium used by him to support gambling that is discussed here is actually the third one that appears in Chapter 4. It is a twist on an earlier condemnation of gambling. It is also the only one that is accompanied by a classical reference. In support of gambling Cardano stated:

It is also a means of gaining friendship, and many have arisen from obscurity because of the friendship of princes formed in play.

Then he quoted from Cicero's *Philippics*:

This is what Cicero meant in his Philippic by the words that fellow-player of yours, condemned for gambling.

The actual passage in the *Second Philippic* (Cicero 1986, p. 671) is distinctly anti-gambling. Cicero condemned Mark Antony for bringing back to Rome one Licinius Lenticula, someone whom Cicero considered a scoundrel, someone who actually gambled in the Roman Forum and was convicted of the offence. Cardano put the only possible positive spin on the episode. By gambling Lenticula had made friends with the powerful Antony and through his influence was able to come back to Rome.

Another reason in favour of gambling is the relaxation it provides. Cardano wrote

As advantages from well-managed play we obtain relaxation from anxiety and a pleasure from which we arise ready and eager for serious business.

One likely source for this sentiment is Cicero (2001). Specific reference to him and a quotation of his comments on dicing to pass the time during periods of bad weather are made in the final chapter of the LLA rather than Chapter 4:

Men who are accustomed to hard daily toil, when by reason of the weather they are kept from their work, betake themselves to playing with a ball, or with knucklebones or with dice, or they may also contrive for themselves some new game in their leisure.

Cardano wrote that this quotation is from Book 2 of *De Oratore*, actually from Book 3 (Cicero 2001, p. 240). A second exemplum from the same source related directly to relaxation, rather than passing the time during bad weather, is the example of Publius Scaevola, a Roman jurist and Consul in the Republic, who, as Cicero (200l. l. 217, p. 110) claimed, obtained his relaxation from work by playing at ball games and at a gaming board known as Twelve Lines. On the opposite side of the interpretation is Petrarch (Petrarca 1991, vol. 1, p. 79). Writing 200 years before Cardano put pen to paper, he condemned dice games. He stated his condemnation with classical references to those who enjoyed dice and board games and said, putting words into the mouth of the character Reason

Scaevola chose these games as a relaxation from the cult and ceremonies of the gods and the laws of men, in both of which he was enormously experienced, and Augustus to refresh himself from the cares of his great empire, which he governed long and well.

This quotation appears to favour Cardano's position and could have well been his source regarding dicing as a form of relaxation. The quote is taken out of context of the entire dialogue. In the words of Reason, Petrarch went on to say that one should not always try to imitate the *pe-culiar preferences* of *learned and prominent men*, since it can lead to disaster. One of the classical exempla on which Petrarch made his argu-ments is Suetonius (1914, p. 235) in which he was highly critical of Augustus's gambling habits. The comments of Cicero in this case were more tolerant of gaming.

A final point in favour of gambling in Chapter 4 of the LLA runs counter to the theme of dissimulation in Castiglione's *Courtier*. Related to this point, Cardano wrote

Knowledge of the character of our fellow-citizens is, as it were, a rack on which anger, greed, and honesty or dishonesty are made clear. For play both produces important evidence and is an actual torturer if the stakes are large.

Gambling removes the façade constructed by the courtier and reveals the true nature of the individual behind the mask. The theme of dissimulation and the danger that gambling presents through destroying a carefully constructed façade appears elsewhere in Renaissance literature. Petrarch (Petrarca 1991, vol. 1, p. 83) wrote:

You will remember that Ovid, in the book about which he teaches an indecent and unnecessary art, inserting, however, something useful now and then, admonishes ladies in love to abstain from gambling and similar activities to disguise the vices of their souls, lest they displease their lovers who see them swollen with anger or devoured by greed. This advice is given even more appropriate for men, who should avoid giving offence, not only to the eyes of others, but to the eyes of God. Who sees all and Who loves good minds and decent manners.

Petrarch has misread Ovid in that the *admonishment* is for he men. He has also put his own distinct interpretation on Ovid (1852, p. 448). He advised men to devise games for their lovers since this activity can encourage love. He then cautioned the men to be careful since the heat of the game may reveal aspects of their own characters that should not be shown to their lovers, aspects such as greed and quarrelsomeness.

Many more humanist influences can be found in the LLA. The examples that have been given here illustrate the richness of the discussions of these topics in the Italian Renaissance and the breadth of the classical sources on which the discussion was based.

7. Relationship to De Vetula

Cardano's description of the throw of three dice is very similar to, but much more concise than the description in the pseudo-Ovidian poem *De Vetula*. Written in Latin in about 1250 and purportedly an autobiographical work of Ovid, it is divided into three books. The first book describes Ovid's youth, his love affairs, and some of his amusements and pastimes. The second book details a tragicomic love affair. Ovid becomes disillusioned with the pleasures of love and devotes himself to philosophical pursuits. The third book is about Ovid's conversion to Christianity. Robathan (1968) and Klopsch (1967) have provided a transcription of the poem based on various manuscript sources, as well as some textual commentary. The calculation of the chances that the various sums on the faces that show in the throw of three dice appears in the first book. Bellhouse (2000) has described these calculations as well as some relevant marginalia in one of the manuscript sources and has given an English translation of the relevant passage on dicing.

Both Cardano and the author of *De Vetula* approached their analyses of the dice throws by stating that there are 6 different throws when the faces are all alike (a triplet such as 1, 1, 1), 30 throws with two faces alike and one different (a doublet and another face such as 1. 1, 2) and 20 throws with all three faces different, such as 1, 2, 3. They both argued that the 30 throws are obtained from the product 6x5 since there are six ways to obtain the doublet and five ways to obtain the third face different from the other two. Only the author of De Vetula described how he obtained 20 as the number of distinct throws when all the faces are different. This description is somewhat obscure and at least one of its manuscripts has explanation as a marginalia. Using more modern combinatorial mathematics, the 20 throws are determined as the number of combinations obtained from choosing three objects (three different faces) from six different objects (the six faces of the die).

Cardano could have easily obtained this number from an arithmetical triangle such as given by Tartaglia (1556). Edwards (1987) extensively described arithmetical triangles and their derivations and uses prior to Pascal's triangle. Both Cardano and the author of *De Vetula* stated that there are three ways to get the same throw with a doublet and a different face (1, 1, 2; 1, 2, 1; 2, 1, 1), for example, and six ways to obtain the same throw with three different faces. The main difference between the two analyses, other than some excess verbiage in *De Vetula*, is that only its author provided a table to show how each of the different sums of the faces that show, given in Table 1 [only briefly explained above], is obtained. Another difference is in the use of language. When referring to the number of chances in the throw Cardano used the word *sortes* which relates to the number of lots, and pseudo-Ovid used *cadentia*, which relates to the number of ways the dice can fall.

The relative closeness of these two approaches may be contrasted to one given by Galileo (1952). David (1962) has given an English translation of this work. Galileo arrived at the same answer as Cardano and pseudo-Ovid but by a different route. He went to great length to show that there is only one way to obtain a triplet, three ways to obtain a doublet and one other face and six ways to obtain three different faces. Like *De Vetula*, Galileo made a table to calculate the various chances for the sums on the faces. In its form and layout his table is quite different from that in *De Vetula* and it seems clear that he did not rely on that source to solve the dicing problem.

I would put forward the interpretation that Cardano took his calculations for three dice from *De Vetula* and applied the same approach given there to the discussion of a single die and then two dice. Krischer (1994) has taken the opposite view that Cardano's calculations are not derived from *De Vetula* by noting, for example, that he did not quote from the poem itself. Kendall (1956) implicitly has taken a similar view, asserting that the probability results in *De Vetula* were *rediscovered* in Cardano's LLA. Nevertheless, the two approaches are very close and the interpretation of dependence cannot be rejected. As a humanist, it is quite possible that Cardano read *De Vetula*, most likely in printed rather than manuscript form. There are its two early printed publications, one ca. 1475 in Perugia and the other four years later in Cologne. The Perugia edition does not contain any of the numerical tables, which is the main difference in the treatments of the problem by Cardano and the author of *De Vetula*. Further, the tables appear in the part of the book where the word *cadentia* occurs. The 15th century publications were undoubtedly part of the recovery of antiquity that characterised the Renaissance and are likely the results of printers wanting to get an *ancient* work into print. Ovid was a popular author; the British Library's *Short-Title Catalogue of Books Printed in Italy* shows over 40 editions of various works of Ovid prior to 1500. The two editions of *De Vetula* are also typical of the printers of the time; the earlier edition contained many errors and in the next edition the printer stated that corrections have been diligently made (Robathan 1968).

8. Discussion and conclusions

Many commentators on Cardano's LLA have concluded that the book is a mishmash of several and sometime contradictory results and statements. Rather than the mishmash it is purported to be, I would argue that, however badly written, there is an internal consistency in the text and a logical progression to the whole work. The former results from Cardano's attempts to show the situations in which gambling could be considered a just act (*ius*). Moreover, Cardano provides knowledge (*scientia*) of various aspects of games to both to protect oneself against injustice and to provide a situation in which the gain from gambling can be considered to be of the best type. The way in which the LLA has been structured is an attempt to show that no matter what the classical authors concluded abut gambling and games of chance, justice has always been available in these activities. At the beginning of the text, classical and modern exempla were used through the method of *utramque partem* to raise the question of justice in gambling. As the discussion proceeded, Cardano demonstrated justice in gambling mathematically for the games of his day. Justice can be maintained, in part, through *scientia* and he provided the necessary information. At the end of the LLA Cardano returned to the ancient texts. By providing a mathematical, though in modern terms probabilistically incorrect treatment of ancient games, we are meant to conclude that the potential for justice in games of chance has always been present.

Initially, Cardano assumed equal stakes and equal chances. This may have been a prevalent assumption in the time between the composition of *De Vetula* and Cardano's own time. There is precedent for this going back to antiquity. Mention has already been made of Cicero's solution to a variation of the lifeboat problem. In that case, equal value of the individuals implied equal chances for selection. On the other hand, equal values of items can be constructed and then chosen with equal probability. An example from antiquity that illustrates this situation is the division of property from an inheritance. In ancient Greece inherited property was divided into portions of equal value. Then lots were cast to distribute the portions. See for example Thalmann (1978). By the 16th century this method of property division had even found its way into English law to settle disputed estates. Gataker (1619) has a reference to the English system of division of property by lot. This possible desire to construct equipossible events may explain a passage in the *Pardoner's Tale* from the *Canterbury Tales*. At one point in this particular tale, the pardoner says (Chaucer 1977, p. 227)

And *By the blood of Crist that is in Hayles,*
Sevene in [is] *my chaunce and thyn is cynk and treye*.

The use of the word *chaunce* has the following interpretation. If two people are playing at dice, the outcome (or outcomes) of the throw that leads to a player winning is known as his chance. Though Chaucer made no statement of probability, his two chances have equal probability: a sum of 7 on the faces of two dice has probability 1/6, likewise for the event 5 or 3.

At some point there was an intellectual transition from simple lots with equal chances to groups of lots or outcomes which as a group had equal chances. In the LLA Cardano was trying to take the process one step further. Using Aristotle's concept of justice, he tried to generalise gambling problems beyond equal stakes. His attempt was limited in that he did not go beyond the calculation of the number of outcomes of an event. This can be seen in Chapter 32 where Cardano counted the number of chances in the throw of the *tali* rather than calculating the probability of the throw. Within the framework of chance, it is not necessary to account for later concepts of probability such as long-term frequencies, degrees of belief, or even expectation that Cardano came close to hitting upon. Prior to his work it was only necessary to rely on justice to set up equitable initial conditions, in particular equal stakes and equal chances, or lots of equal size for all players. In the same context Cardano relied on justice to handle the situation of inequitable conditions.

Acknowledgements. I thank Professor Bill Acres of Huron University College for his comments and encouragement. I also thank the students (particularly Pat Giles, Sean Roche, Mike Snowdon and Lynne Thompson) of History 460 at the University of Western Ontario for many insightful questions and comments. I also thank the editor, Craig Fraser and the referees for many helpful comments.

Notes

1. *Abbreviation: LLA. I also abbreviate other works of Cardano:
De Subilitate = Sub; De rerum varietata = Rerum;
Practica arithmetice … = Practica; De vita propria … Vita;
Ars magna = Ars; De Ludis = Ludis; De Ludo Aleae = LA*

2. In § 7 Kendall (1956) suggested the period 1220 – 1250

3. Cardano was professor of mathematics at the Milan University and member of the college of physicians there. However, Zubov (2010, p. 6) noted that Cardano's attempt to enter the college had failed.

4. Hacking did not describe Aristotelian ideas.

5. According to the modern viewpoint, definitions can only be positive

6. Jacob Bernoulli *(Ars conj.,* pt. 3, problems 23 and 24) called such dice blind.

Information about some figures

Aretino P., 1492 – 1556, writer, playwright, satirist

Boethius A. M. S., 480 – 524, Roman senator, consul, philosopher

Castiglione B., 1478 – 1528, writer

Fibonacci L., ca. 1170 – after 1228, mathematician

Petrarca F., 1304 – 1374, great poet of pre-Renaissance period

Sacci B. (Platina), 1421 – 1481, humanist, writer, head librarian of Vatican Apostolic Library

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II

D. R. Bellhouse

Manuscript on chance written by John Arbuthnot

Intern. stat. rev., vol. 57, 1989, pp. 249 – 259

Summary

The Gregory manuscript collection held by the University of Edinburgh Library contains a treatise on chance written by John Arbuthnot (MS Dk.1.2Fol B(no. 191), probably in 1694. The manuscript consists of two theorems and four problems in which either generalizations of results in Arbuthnot (1692) or anticipations of results in Arbuthnot (1712) are given. Two types of significance tests are given as applications to the results derived. One of the tests is the same form that used in 1712; the other is a crude one-sample test of location. The mathematical content of the manuscript indicates that Arbuthnot was an able mathematician working on problems of current interest to probabilists.

1. Introduction

John Arbuthnot (1667 – 1735) was both a medical man and a man of science and letters. He was physician to Queen Anne and at the same time a learned wit, the creator of John Bull. He was elected Fellow of the Royal Society in 1704 and sat on the Royal Society committee, created in 1712 to investigate the dispute between Leibniz and John Keill over who had priority of discovery, Newton or Leibniz, of the calculus. Biographical details for Arbuthnot may be found in Aitken (1892) and Beattie (1935) as well as the *Dict. Nat. Biogr*., DNB).

Arbuthnot's major contributions to probability are his translation of and additions to Huygens (1657) *De Ratiociniis in Ludo Aleae* (Arbuthnot 1692 and later editions) and his paper on the sex ratio [at birth] of 1712 which contains one of the first tests of significance. Both these contributions are reviewed in Todhunter (1865); the paper of 1712 is also reviewed by Pearson (1978), Bartholomew (1984) and Stigler (1986).

The University of Edinburgh Library holds in its manuscript collection a manuscript on chance, unsigned but in the hand of John Arbuthnot. The manuscript is part of the Gregory Collection (MS Dk.1.2. Fol. B [no. 191]), and bears the title *A treatise of chance written by Dr. Arbuthnot in anno 1694* in the hand of David Gregory. He (1661 – 1708) was Savilian Professor of Astronomy at Oxford and an apologist of Sir Isaac Newton (see DNB for biographical details of Gregory).

Although the 1694 date on the manuscript has been questioned by Ross (1956), it appears to be the most reasonable date. A discussion of the dating of the manuscript is given in the *Appendix*. Arbuthnot first met Gregory when he entered University College, Oxford, in 1694. Based on the discussion in the *Appendix*, a reasonable conjecture is that the manuscript on chance was written shortly after his arrival at Oxford in October of 1694 to catch the attention of Gregory, another Aberdeen alumnus (DNB). This would explain why the manuscript remained in Gregory's possession and was never published.

The manuscript in the library collection comprises 11 pages of which the first ten, all in the hand of Arbuthnot, contain much original material. There are generalizations of some of the results on dicing games in Arbuthnot (1692) and anticipations of the results on the sex ratio in Arbuthnot (1712). The last page is a translation and condensation of a paper on chance by Leibniz (1690); a brief description of the problem solved by Leibniz and earlier by Jacob Bernoulli (1690), is given in Todhunter (1865, p. 47). Gregory references both these articles at the top right corner of the page by citing the journal name, *Acta Eruditorum*, the year 1690, and some page numbers. As a prefatory note to his published collection of Gregory's Memoranda. Hiscock (1937) states that the Memoranda in manuscript form contained abstracts, that Hiscock chose not to publish, of material that Gregory had read. This includes articles in the journal *Acta Eruditorum*. The final page of the Arbuthnot manuscript is then merely one more abstract made by Gregory. As discussed in the *Appendix*, the placement of this abstract with the Arbuthnot manuscript may be helpful in confirming the 1694 dating.

2. The mathematical content of the manuscript

The manuscript contains two theorems, the first of which is followed by a corollary and a scholium, and four problems, of which the second and fourth are each followed by a scholium. The formulas which Arbuthnot obtains throughout the manuscript are all correct; however, there are several arithmetical errors. Throughout the manuscript Arbuthnot uses as his model a die with n sides which is thrown p times, or equivalently p dice each with n sides thrown once. The faces of the die are numbered consecutively 1 through n. Modern notation is used to describe the results obtained.

In Theorem 1, Arbuthnot obtains the series

$$n^p - b^p - C_p^1 b^{p-1} - C_p^2 b^{p-2} + C_p^3 b^{p-3} - \ldots \tag{1}$$

which is an expansion of $n^p - (b + 1)^p$, where Arbuthnot sets $b = n - 1$. On dividing this series by n^p, the first $i + 1$ terms, $i = 1, \ldots p$, give the probability that a particular face shows at least i times in p throws of the n-sided die. An equivalent formula is in Montmort (1713, Proposition XIII, p. 40). In the scholium which follows his theorem, Arbuthnot shows how to manipulate this series so that the same set of probabilities can be calculated for two or more particular faces to show. For example, for three particular faces to show, in the series set $b = n - 3$ and multiply b^{p-1} by 3^i, $i = 1, 2, \ldots, p$. He also indicates in the corollary how one could make the probability calculations with dice with differing number of sides by reducing the problem to some simple algebraic manipulations. Later in this manuscript, Arbuthnot shows how this corollary may be applied to find the input probabilities to use the Halley's (1693) method for finding the present value of joint survivorship annuities and insurance. As a final note to this section, Arbuthnot gives an algorithm, through the construction of an arithmetical triangle, to obtain numerical values of the probabilities given by (1).

The sets of results from Theorem 1 and its corollary generalize Propositions X, XI and XII in Arbuthnot (1692). In Propositions X and XI, several laboriously obtained numerical calculations are made to calculate the probability of throwing a six at least once in *p* throws of a regular six-sided die ($p = 1, \ldots, 5$) and of throwing a twelve at least once in *p* throws of the two dice ($p = 1, 2, 4$). In Proposition XII the event is at least two sixes in *p* throws; the numerical value is given for $p = 3$ and it is stated that the probability is greater than 1/2 for $p = 10$.

Problem 1, which follows the first theorem, is another type of generalization of Propositions X and XI in 1692. The problem is to find *p*, the number of throws required of the *n*-sided die so that the probability of throwing a particular side at least once is some specified number $1/r$. On using the first two terms of (1), Arbuthnot obtains the solution

$$p = \log\frac{r}{r-1} \div \log\frac{n}{n-1}.$$

He provides two numerical examples. First, for a regular die and $r = 2$, he shows that it would be advantageous to bet on the event with four dice, but not with three. The second example is concerned with the Royal Oak Lottery, briefly described in Arbuthnot (1692, pp. 57 – 58). The determination of the lottery is equivalent to the throw of a 32-sided die. Again with $r = 2$ the throws that it would be advantageous to bet on is $p = 22$ but not 21. The solution to this problem was later given by Montmort (1708 p. 180). It appears as Proposition XXXIX in Montmort (1713, p. 228) and is also given by De Moivre (1711, Problem V).

In Theorem 2, Arbuthnot examines the problem of finding the probabilities of a particular sum showing on the faces in the throw of *p* *n*-sided dice, but does not provide a general solution. The theorem generalizes some earlier observations he made (Arbuthnot, 1692, pp. 59 – 62) on the average value of the throw of any number of regular dice. In the manuscript, Arbuthnot notes that the minimum and maximum for the sum must be *p* and *np* respectively, that for a given value of *i* the sums $p + i$ and $np - i$ are equiprobable, and that the sum with the greatest probability is $(np + p)/2$. From symmetry of the distribution the probability that the sum is at least $(np + p + 1)/2$ is the same as being at most $(np + p - 1)/2$. This result and others related to the distribution of the sum were obtained earlier by Strode (1678), see Stigler (1988) for a discussion of Strode's work. Since Strode is not mentioned in the manuscript, while Huygens, Halley and Leibniz are, Strode's work was probably unknown to Arbuthnot at that time (1694). The general solution for the distribution of the sum was obtained later by De Moivre (1711) in the Lemma on pp. 220 – 221 and by Montmort (1713, Proposition XVI, p. 46).

Arbuthnot's Problems 2 and 3, for which complete solutions are given involve very elementary probability calculations. Problem 2 is now known as the Birthday Problem. Arbuthnot states it as finding the probability that at least two faces will show the same in a throw of p n-sided dice. He notes in the scholium to this problem that the probability is one when $p > n$. Arbuthnot (1692, pp. 75 – 77) had earlier obtained the solution for $n = 6$. In Problem 3 and its scholium Arbuthnot shows essentially that the probability of a series of independent events will happen together is the product of probabilities[1].

Finally, in Problem 4, Arbuthnot begins by giving the formula for ${}_nC_r$ and then presents the general formula for the probability of an equal number of heads and tails showing, or crosses and piles as he calls them, in n tosses of a coin. He calculates the exact probabilities for two and four coins, and, using logarithms, attempts to approximate the probability for one million coins, but makes a major arithmetical error. He finds that the probability for one million coins is in the order of $1/1^{251030}$ rather than the correct value which is closer to 8 in 10 000.

3. Anticipation of results in the 1712 paper

In his 1712 paper, Arbuthnot provides two arguments for the presence of Divine providence in the determination of the sex ratio [at birth]. For both arguments he begins by assuming that male and female births occur by chance, i. e., that the two events are equiprobable. His first argument is that it is unlikely that an equal number of males and females will be born in any year. The probability, from the [yet unknown] binomial distribution[2], is

$$C_n^{n/2} \div 2^n$$

for n even, when n is the number of children born in a year. Arbuthnot calculates this probability for $n = 2, 4, 6, 8$ and notes that, for large n, the probability, which can be obtained using logarithm is small. His second argument is based on a significance test. Using the chance model leading to the binomial distribution, Arbuthnot finds that the probability that the number of male births in a year exceeds the number of female births is less than $n/2$. On setting the probability equal to 1/2 and noting that over an 82 year period, every year more males than females were christened in London, Arbuthnot calculates the probability of the observed event at $1/2^{82}$. He argues that this probability can be reduced even further since the inequality in the sex ratio has been observed in several other localities and at other times. He concludes that the observed inequality in the sex ratio cannot be attributed to chance and puts forward his view that the inequality may be attributed to Divine providence working to a good end.

The first argument that appears in the 1712 paper is completely anticipated in the manuscript. The appropriate probability calculations are given in Problem 4 of the manuscript with the addition of the incorrect numerical probability for $n = 1$ million. In a scholium following Problem 4, Arbuthnot ponders the smallness of the incorrect probability he has just calculated (punctuation has been added; Arbuthnot tends not to use any or even use upper case letters to begin sentences):

By this it is evident that the equilibrium which is kept between the sexes in mankind, and which is confirm'd by the bills of mortality of all places, cannot be the effect of mere chance. For if it were left to chance, the begetting of a male or a female would be cross and pile. And by the former calculation, it appears what a vast improbability that is to keep an equality in a considerable number for one, but to do it for a succession of ages together, is vastly more. Its true the equality of male and female is not precise, but it differs by so very small a pairt that in calculations which cannot consider these inequalities we might suppose it to be so.

The argument that is given in the last sentence of the quotation appears also in 1712. Arbuthnot notes both in the manuscript and in 1712, that the sex ratio is not exactly equal to one. In 1712 he states that the appropriate probability is not the middle term, but it

Will take in some of the Terms next to the middle one, and will lean to one side or the other.

The significance tests in the manuscript are given as applications of Theorem 2 and Problem 3 of the manuscript, both of which are described in § 2. Three significance tests are given. The first is a direct anticipation of the test in 1712 and is used to evaluate the chronology of the first seven kings of Rome. The second and third tests are used to evaluate the chronology of the kings of Scotland. The second test is again similar in spirit to the 1712 test. Arbuthnot crossed out the discussion surrounding this test and replaced it by the third significance test, a test which is much different in character from the first two in the manuscript and from the 1712 significance test.

Here is Arbuthnot's first significance test from the manuscript, again with punctuation added. There are two or three words for which Arbuthnot's handwriting, poor at the best of times, is indecipherable; they are marked […] in the transcription.

To make some application of this Theory to the mortality of mankind, supposing (till we know a better) the quantity of it determin'd by the bills of Breslau in the philosophical transactions Jan:92/3. By them it appears it is almost an eve'n wager that a man of 22 should live till 56. But that ther should be seven men succeeding one another in a office, or so who entering on it at 22 should live at least 56, it is, by Prob. 3, 47 to one which has sometimes made me suspect the chronology of the first seven Roman kings who made 238 years among them. They were men when they begunn to reign, and most of them much more than 22, except Romulus who I think was 18. Now the 238 years makes as a whole number 34 years appiece, which […] a little improbable supposing the casualties of kings the same as other men, or the quantity of mortality the same with that now[3].

There are two arithmetical errors in this text, one more obvious than the other. Arbuthnot uses Halley's (1693) life table to obtain 1/2 for either the survival probability, ${}_{34}p_{22}$ or the death probability ${}_{34}q_{22}$, using standard actuarial notation. The more obvious error is that $1/2^7$, the probability of seeing seven of these 34-year survival periods in succession is 1/128 rather than 1/48. The second error is in the calculation of ${}_{34}p_{22}$ itself; Arbuthnot appears to have found ${}_{33}p_{22}$ instead. On using Halley's table, the values ${}_{34}p_{22} = 282/586 = 0.482$ and ${}_{33}p_{22} = 292/586 = 0.498$ are obtained.

This significance test is not as *clean cut* as the test which appears in 1712, a number of simplifying assumptions have been made here. Every king ascends the throne at age 22 and reigns 34 years. There is also a bit of undisclosed data manipulation to get the average regnal length to be a whole number 34. Tradition places the reigns of the seven kings of Rome between 753 and 510 B. C., see, for example, Scullard (1980, p. 420) for a total of about 243 years instead of 238. Tradition also gives the individual regnal lengths. These range from about 26 to 45 years, data which Arbuthnot ignores.

The second significance test is difficult to decipher completely in that part of the discussion has been obliterated by the pasting of an extra flap of paper, which contains the third significance test, onto the manuscript page. The part which remains visible has been crossed out. In spite of this handicap, there is enough information in what remains and in the discussion on the extra flap of paper to piece the text together. Arbuthnot refers to *111 Scots kings* who reigned a total of about 2024 years. The average regnal length is between 18 and 19 years. Arbuthnot assumes that each king reigns 19 years from age 14 to age 33 and obtains, correctly, ${}_{19}q_{14} = 1/5$ (*odds of 1 to 4,* he says), again using Halley's life table. The odds of seeing a 19-year reign 111 times in succession he calculates, incorrectly, as to $(4^{111} - 1)$. His conclusion seems to be that there is a difference in the mortality rates between the Roman and Scots kings rather than doubting the Scottish regnal list. It was probably this conclusion that led Arbuthnot to cross out this test and to rewrite his analysis of the 111 Scots kings using another significance test which attempted to show that there was a higher rate of mortality in the Scots kings than that in the Breslau life table.

The third significance test follows directly after the first test, but on the extra piece of paper that has been pasted as a flap to the manuscript page. The edge of the page on which this is written is ragged so that some words or endings of words are missing. Any additions to what actually appears on the manuscript page are given in square brackets. Once again punctuation has been provided.

It appears by the bills of Breslau that all the persons born are reduced by mortality to one half by 33 years. Now the mass of mankind being suppose'd neither to increase nor decrease, we must suppose that to supplye this mortality there must be an equall number born before 33 years, that is, evry person before 33 years has a child to succeed him. So to reduce the succession of mankind to some regular hypothesis, it seems to be the same as if every person shou live to the age of 33 and then dye to make room for his child. Supposing at this rate a man to be like a dye of 66 sides, of which the mid number is 33 (indeed this is not exactly true unless the ca[sts] *for a mans dying at any age on this side of 33 were those precisely with those for his dying at an age equally distant on the* [other] *side). The whole number of 3 such dice by theor 2 or (np + p/2), is 101 so that a succession going from father* [to] *son in 3 generations should make as the middle* *number 101 years from the birth of the grandfather to the death of the grandchild. Th*[is] *is not to be understood either of the younger or older children but that is an ev'n wager it happens in some of them. I believe th*[at] *will pretty well agree with experience. So 111 generations shou make 3669 years. Therefore, it may appear a little strange that 111 Sc*[ots] *kings should make only about 2024 years. But first taking away the collatorall successions and making allowance for the succession going to the eldest son, what is deficient of that num-*[ber] *seems to be the casualties of the Scots kings beyond the rest of mankind. From this ther may a probable conjecture be made of the whole number of mankind that have been for a great number of ages, allowing the quantity of mortality to have been the same in all those ages and that we know the number then living. Not: that the case of this problem is very different from that of the former, of men living 34 years after they come to mens age*.

What Arbuthnot is trying to do is a one-sample test of location. He calculates 3669 as the theoretical total of the 111 regnal lengths, the theory based on Halley's life table, and then compares it to the observed length, 2024. He has no method of probabilistically quantifying the magnitude of the difference, but feels that the difference is great enough to conclude that there is difference in the mortality patterns of the Scots kings from the rest of mankind, at least as it is described in the Breslau mortality tables.

The *Not* at the very end of the quotation appears a bit obscure until it is put into context with the first and second significance tests. In the first test Arbuthnot doubts the chronology of the Roman kings; together they appeared to have lived longer than normal. This is summed up in his p-value 1/48 or, more correctly, 1/128. On the other hand, Arbuthnot is willing to accept mortality in excess of the normal rate as observed in the 111 Scots kings. What he has not got clear is the concept of one-sided alternate hypotheses with the appropriate tail area probability calculation. His second significance test is correct, but the probability calculation is for the opposite tail area to that of the first test. In the first test a survival probability (${}_{34}p_{22}$) is used while in the second test a death probability (${}_{19}q_{14}$) is used to obtain the p-value. Since on the surface both tests appear to give similar results, Arbuthnot abandons the second test in favour of a third which better illustrates the higher than normal mortality rate of the 111 Scots kings.

The differences between this test and the first test are readily apparent; but there are also some similarities. As in the first test, a number of simplifying, and perhaps questionable assumptions are made. He assumes a stable population and argues that the median generation length is 33 years. Then he simulates the length of an individual generation by the throw of a 66-sided die. A final point of similarity is that Arbuthnot makes another arithmetical error. On applying the result of Theorem 2, Arbuthnot finds that the probability that the total regnal length is at least 3669 years is 1/2. He slipped a digit and miscalculated $(np + p + 1)/2$ as $(66(111)+ 11+ 1)/2 = 3669$ rather than $(66(111) + 111 + 1)/2 = 3719$.

Shoesmith (1987) has hinted that Arbuthnot's (1712) argument for Divine providence may have been influenced by the Boyle lectures delivered in 1692 by the philosopher and theologian Richard Bentley, and printed in 1692-3, reprinted in Bentley (1976). Part of Bentley's argument for the existence of a supreme being, an argument by design, is similar in style to that used by Arbuthnot in his 1712 paper. Bentley remarks on the improbability of certain things happening fortuitously. For example, he quotes astronomical odds against all the planets fortuitously moving in the same direction around the sun and virtually in the same plane.

In view of the secular rather than sacred nature of the significance tests in the manuscript, Bentley's influence appears to have been minimal in Arbuthnot's development of significant tests. Moreover, Arbuthnot later satirized Bentley and his work in at least two publications, see Aitken (1892, pp. 121, 124) and Beattie (1935, pp. 282 – 284, 312 – 313). A more reasonable conjecture might be that the first significance tests, the ones in the manuscript, were motivated by Newton's interest in the regnal lengths, see Stigler (1977) [see Sheynin (1971] for a discussion of Newton's work in this area. The 1712 paper then becomes either a reply to Bentley's methods of argument by design, but using previously developed tools for statistical analysis that were motivated by other problems.

4. Postscript to the 1712 paper

Arbuthnot (1712) narrowly defined a chance event in the sex of new born as equipobable male and female births. Any deviation from this probability of 1/2 for a male or female birth was attributed to God's providence or, as we shall see here, some natural law. Todhunter (1865, pp. 130, 193, 197) described some of the discussion surrounding this narrow definition of chance and the probability calculation of $1/2^{82}$. A full account of the discussion is given in Shoesmith (1985, 1987). What has generally been ignored in any discussion on the paper is the technique given by Arbuthnot through which God's providence is made manifest. In 1712, Arbuthnot reasoned that

There seems no more probable Cause to be assigned in Physicks for this Equality of Births, than that in our first parents Seed there were at first formed an equal Number of both Sexes.

His position was questioned by John Chamberlayne, another fellow of the Royal Society and a scientist and writer in the royal court. With the addition of some punctuation, Arbuthnot wrote as follows to Chamberlayne in 1711, transcribed by Ross (1956) and mentioned by Shoesmith (1987):

There are a great many other inferences, that might be made of the observed equilibrium, but I had confynd myself to one Argument which was to prove that it was not the effect of chance, but of a regular Conduct. It is a pretty hard matter to guess at the physical Causes of the different sexes. The most probable is that they exist originally in Semine Masculo. There are some experiments that had never yet been made, that might give some light in the Matter.

What the experiments are he does not specify. However, in this quotation he does appear to be the precursor of the theories on the sex ratio that have culminated in the modern genetic theory of sex allocation. See Karlin & Lessard (1986) and Charnov (1982) for a treatment of these modern theories.

5. Arbuthnot as a mathematician

On reading Todhunter's (1865, pp. 48 – 53) analysis of the fourth edition *Of the laws of chance* (Arbuthnot 1738) one gets the impression that Arbuthnot was barely competent as a mathematician. Todhunter notes an incorrect solution to a problem, some problems that are not well-stated, and an approximate solution that is not very proximate. This view is not assuaged after reading the 1712 paper; the probability calculations appearing there are very elementary. The manuscript shows Arbuthnot in a much more positive mathematical light. In spite of his errors in arithmetical manipulations, he appears to be an able, though not brilliant, mathematician. He could tackle general unsolved probability problems of current interest and could apply the results to a wide variety of topics other than gambling. One good example of his mathematical abilities is the solution to dicing problems through formula (1) and the arithmetical triangle which accompanies this formula in the manuscript. The extension to the arithmetical triangle that Arbuthnot gives, a method which is straightforward but tedious, can be used to solve the dicing formula that Pepys posed to Newton in 1693, namely to find the probabilities that at least i sixes will show in the throw of $6i$ dice, $i = 1, 2, 3$. See David (1962, pp. 125 – 129) for a description of Newton's solution and the correspondence surrounding it. Arbuthnot's solution for this particular problem appears to be more elegant than Newton's.

This view of Arbuthnot as an able mathematician is shared by De Moivre, who described Arbuthnot, though not by name, in the preface to the *Doctrine of Chances* in 1756, as *a very ingenious Gentleman*. The preface was written in 1717. With reference to Arbuthnot's translation of Huygens's work, probably the second edition (Arbuthnot 1714), which is virtually identical to the first edition. De Moivre says that Arbuthnot was

Capable of carrying out the matter a great deal further.

What appears in the manuscript is probably the upper bound on Arbuthnot's mathematical capabilities. He may have been able to take his work in probability further; but, in view of his many official pressing duties and his devotion to other literary accomplishments, which limited the time he could devote to mathematical subjects, the full range of his mathematical abilities will never be known. Curiously, the general results he obtained in this manuscript did not appear in later editions of his book (Arbuthnot 1714, 1738).

Acknowledgements. The work was supported by a grant […]. I would like to thank Professors S. M. Stigler and D. A. Sprott, and a referee for their helpful com-ments, Dr. L. Lefkovitch for suggesting that I examine the watermarks in the papers, and my father, R. M. Bellhouse, for assisting in the transcription of the manuscript.

Appendix: the dating of the manuscript

Gregory's placement of the title *A treatise of chance written by Dr. Arbuthnot in anno 1694* on the manuscript must have occurred after

1696 since Arbuthnot did not receive his doctorate in medicine until that time. This retrospective dating on the manuscript by Gregory leads immediately to the suspicion that the 1694 date may be inaccurate. However, a careful examination of the manuscript, through both the handwriting and the subject material covered, provides evidence in favour of the 1694 date but leaves open the possibility for dating it approximately at 1705. The latest possible date for the manuscript is 1708, the year of Gregory's death.

The first negative response to the 1694 dating comes from Ross (1956), who has transcribed all of Arbuthnot's known correspondence. Based on this experience, Ross (1969) later commented that there is a distinct change in Arbuthnot's style of handwriting between his fifths surviving letter written in 1698 and the sixth in 1703. From the 1703 letter onwards Arbuthnot's handwriting is *more free, cursive and hurried* and he has developed an idiosyncratic style of punctuation. What is distinctive about Arbuthnot's punctuation in the 18^{th} century is that he rarely uses it. Sentences are not divided by periods, and new sentences do not necessarily begin with capital letters but with lower case letters of variably larger or smaller sizes. Ross' s observations on Arbuthnot's correspondence after 1698 exactly describe the style in which the manuscript was written. Moreover, some parts of the manuscript were not carefully worked upon; there are errors in several numerical calculations that Arbuthnot made. Ross (1956, p. 950), who saw the manuscript but did not publish a transcription of it, comments that the writing in the manuscript *appears* to be later than that of the earliest letters.

Secondly, in the body of the manuscript, Arbuthnot makes reference to *111 Scots kings* who reigned a total of about 2024 years. On examining the known Scottish regnal lists given by Anderson (1980), the only possible candidate for the list to which Arbuthnot refers is Regnal List F (Anderson 1980, pp. 269 – 278). The manuscript from which this list was taken was in possession of the priory of St. Andrew's. It was lost in 1660 and subsequently rediscovered in the early 18^{th} century by the Scottish antiquarian, Sir Robert Sibbald; see Anderson (1980, pp. 54 – 56). Sibbald showed the manuscript to at least two others, Sir James Dalrymple and Father Thomas Innes. Since Sibbald was also a physician, in particular a physician to King James II, there is the possibility that he knew Arbuthnot and told him of this regnal list. This possibility is unlikely since Sibbald lived in Edinburgh not London (see DNB) during the 18^{th} century and was also not a member of Queen Anne's court. A much better possibility is that Arbuthnot could have heard about the *111 Scots kings* from Dalrymple, another Scottish antiquarian. Dalrymple (1705, p. 131), in the first published reference to the manuscript in which the regnal list appears, mentions that he had seen the manuscript which was in Sibbald's possession. The only clue which might connect Dalrymple to Arbuthnot is that the book (Dalrymple 1705) is dedicated to Queen Anne. The interval evidence given by the 111 Scottish kings may not be as strong as it appears. Arbuthnot gives only the total number of kings and the total regnal length. This may have been well-known Scottish folklore at the time.

This concludes the evidence against the 1694 dating of the manuscript. The evidence in favour of the 1694 dating, in what follows, appears much stronger.

The paper on which the Arbuthnot manuscript and the Gregory abstract are both written is foolscap paper, originally about 12 inches high and 16 inches wide, folded in half to form a 12 by 8 booklet of 4 pages. When the complete foolscap is available (there are 2 complete sheets in the manuscript) the left side of the paper, or the first leaf of the booklet, contains the watermark of the seven provinces of Holland, a lion rampant holding a sword in the upper forepaw and seven darts of errows in the lower forepaw, all contained in a crowned shield. Watermarks of this type may be found in plates 109 – 119 of Churchill (1935) and plates 3137 – 3145 of Heawood (1950). The right side of the foolscap paper, the second leaf of the booklet, contains the initials ID as the watermark, probably the initials of the maker. The watermark which appears closest to the one in the manuscript is plate 117 of Churchill (1935). The major differences are in the detail in the crown on top of the shield and in the detail in the crown on top of the shield and in the detail in the head of the lion. Also the initials IV appear instead of ID. The present author has been unable to identify the paper-maker through the watermarks so that a range of dates for the manufacture of the paper cannot be given at the present time.

In spite of this inability to date the manuscript directly from the watermarks, these watermarks do provide some important information. The first 8 pages of the Arbuthnot manuscript appear on two complete foolscap sheets. The last two pages are written on each side of the left half of a sheet of a foolscap paper or the first leaf of a booklet. The right half of the foolscap page, which would have been blank, is missing. Undoubtedly the blank leaf of the booklet was removed since paper was expensive. Arbuthnot made two additions or corrections to his manuscript. These occur on pages 4 and 6, and were made by pasting a flap of paper on each of these pages. Both flaps are cut from the left side of the same piece of foolscap paper. One cut goes through the watermark and the partial watermarks match were the cut has been made. From the context of the material on the flaps, it appears that these changes were made after the first draft of the manuscript was completed. With the completion of the manuscript, there were two blank right halves of foolscap pages. The Gregory manuscript is written on one side of the right half of a foolscap sheet made by the same maker (ID). Again, the blank half of the foolscap, the left side, has been removed. Because of the raggedness of the edges of the paper of the Gregory abstract, it is impossible to tell if the final page of the Arbuthnot manuscript and the Gregory abstract are from the same foolscap sheet. They were, however, placed together in the Gregory papers with the abstract following the manuscript. The other contents of the manuscript collection in the same box are written on various sizes of paper with very few if any, of foolscap size. Finally, there exists no covering letter, or at least no letter has survived, from Arbuthnot to Gregory, or vice versa, about the manuscript.

The simplest explanation for the abstract and manuscript being written on paper from the same maker, for the placement of the papers together in the manuscript collection, and for the lack of correspondence about the manuscript, is that the manuscript and abstract were written in fairly close proximity, both in location and time. Since Gregory's abstract is taken from the 1690 volume of the *Acta Eroditorum*, the likely location is Oxford where Gregory would have kept his library. A reasonable date to pick is 1694, a year in which Arbuthnot resided in Oxford. There are, of course, other explanations for the evidence presented; this remains the simplest.

Within the manuscript the only published work that Arbuthnot references directly is Halley (1693) which was published in 1694; see Pearson (1978, p. 74). Two other individuals are mentioned, Huygens and Leibniz, but no direct reference to their published work is given. From the context in which these names appear in the manuscript, it appears that Arbuthnot had read Huygens (1657), which is obvious considering his own work (Arbuthnot 1692), and Leibniz (1666) on combinations. A 1690 edition of Leibniz's 1666 work published in Frankfurt, also exists; it is likely that this is the edition that Arbuthnot had read. When Arbuthnot mentions Leibniz, he refers to Leibniz's terminology as *complexions* and *com*^{p} *nation* for objects taken p at a time. See Todhunter (1865, p. 32) for a brief discussion of the use of this terminology in Leibniz (1666).

The internal evidence taken so far from the manuscript is also consistent with a 1694 dating for the manuscript. After the death of his father in 1691, Arbuthnot went to London (Aitken, 1892, pp. 6 – 11) where he supported himself by teaching mathematics as a private tutor. Other than the publication of his 1692 book, little is known of his life in the years 1691 to 1694, at which time he entered University College Oxford probably as a private tutor to another student. A a private tutor in London newly arrived from Scotland with a Master of Arts from Aberdeen, it is unlikely that in the period 1691 – 1694 he would have been personally acquainted with many leading mathematicians and [or] perhaps their work. This would explain why the results of Strode (1678), for example, were not used nor referenced by Arbuthnot in the manuscript.

In conclusion, the evidence in favour of the 1694 dating of the manuscript appears to be stronger than the evidence for a later dating.

Notes

1. This is almost the multiplication theorem. O. S.

2. Thus appeared the binomial distribution! It is also hinted at in the sequel, but never emphasised. O. S.

3. Breslau was a city with a closed population, and therefore could not be regarded as a standard. This circumstance should have been but was not allowed for in many instances. O. S.

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Huygens with important comment constitutes pt. 1 of Jacob Bernoulli *Ars conjectandi* of 1713.

The author quite unnecessarily uses many passive constructions. Just as many other authors, he never applies the more proper expression high (low) probability.

III

J. Bradley

Letter to Dr. Edmund Halley … giving an account of a new-discovered motion of the fixed stars

Phil. Trans. Roy. Soc., vol. 35, 1728, pp. 1 – 16

[1] Sir, You having been pleased to express your satisfaction with what I had an opportunity sometime ago of telling you in conversation, concerning some observations that were making by our late worthy and ingenious friend, the honourable Samuel Molyneux, esq. and which have since been continued and repeated by myself, to determine the parallax of the fixed stars; I shall now beg leave to lay before you a more particular account of them.

Before I proceed to give you the history of the observations themselves, it may be proper to let you know that they were at first begun in hopes of verifying and confirming those that Dr. Hooke formerly communicated to the public, which seemed to be attended with circumstances that promised greater exactness in them, than could be expected in any other that had been made and published on the same account. And as his attempt was what principally gave rise to this, so his method in making the observations was in some measure that which Molyneux followed. For he made choice of the same star, and his instrument was constructed upon almost the same principles. But if it had not greatly exceeded the doctor's in exactness, we might yet have remained in great uncertainty as to the parallax of the fixed stars; as you will perceive upon the comparison of the two experiments.

This indeed was chiefly owing to our curious member, George Graham, to whom the lovers of astronomy are also not a little indebted for several other exact and well-contrived instruments. The necessity of such will scarce be disputed by those that have had any experience in making astronomical observations. And the inconsistency which is to be met with among different authors in their attempts to determine small angles, particularly the annual parallax of the fixed stars, may be a sufficient proof of it to others. Their disagreement indeed in this article is not now so much to be wondered at, since I doubt not but it will appear very probable that the instruments commonly made use of by them, were liable to greater errors than many times that parallax will amount to.

The success then of this experiment evidently depending very much on the accurateness of the instrument, that was principally to be taken care of. In what manner this was done is not my present purpose to tell you, but if, from the result of the observations which I now send you, it shall be judged necessary to communicate to the curious the manner of making them, I may hereafter perhaps give them a particular description, not only of Molyneux's instrument, but also of my own, which hath since been erected for the same purpose and upon the like principles, though it is somewhat different in its construction for a reason you will meet with presently.

Molyneux's apparatus was completed and fitted for observing about the end of November 1725, and on the third day of December following, the bright star in the head of Draco (marked gamma]by Bayer) was for the first time observed as it passed near the zenith, and its situation carefully taken with the instrument. The like observations were made on the 5th, 11th and 12th days of the same month, and there appearing no material difference in the place of the star, a further repetition of them at this season seemed needless, it being a part of the year wherein no sensible alteration of parallax in this star could soon be expected.

[2] It was chiefly therefore curiosity that tempted me (being then at Kew, where the instrument was fixed) to prepare for observing the star on Dec. 17th, when having adjusted the instrument as usual, I perceived that it passed a little more southerly this day than when it was observed before. Not suspecting any other cause of this appearance, we first concluded that it was owing to the uncertainty of the observations, and that either this or the foregoing were not so exact as we had before supposed. For this reason we purposed to repeat the observation again to determine from whence this difference proceeded. And upon doing it on Dec. 20th, I found that the star passed still more southerly than in the former observations. This sensible alteration the more surprised us, in that it was the contrary way from what it would have been had it proceeded from an annual parallax of the star. But being now pretty well satisfied that it could not be entirely owing to the want of exactness in the observations, and having no notion of anything else that could cause such an apparent motion as this in the star, we began to think that some change in the materials etc. of the instrument itself might have occasioned it. Under these apprehensions we remained some time, but being at length fully convinced by several trials of the great exactness of the instrument, and finding by the gradual increase of the star's distance from the pole, that there must be some regular cause that produced it. We took care to examine nicely, at the time of each observation, how much it was, and about the beginning of March 1726 the star was found to be 20´´ more southerly than at the time of the first observation. It now indeed seemed to have arrived at its utmost limit southward, because in several trials made about this time, no sensible difference was observed in its situation. By the middle of April it appeared to be returning back again towards the north, and about the beginning of June it passed at the same distance from the zenith as it had done in December, when it was first observed.

From the quick alteration of the star's declination about this time (it increasing a second in three days) it was concluded that it would now proceed northward, as it before had gone southward of its present situation. And it happened as was conjectured for the star continued to move northward till September following, when it again became stationary being then near 20´´ more northerly than in June, and no less than 39´´ more northerly than it was in March. From September the star returned towards the south till it arrived in December to the same situation it was in at that time twelve months, allowing for the difference of declination on account of the precession of the equinox.

This was a sufficient proof that the instrument had not been the cause of this apparent motion of the star, and to find one adequate to such an effect seemed a difficulty. A nutation of the earth's axis was one of the first things that offered itself upon this occasion, but it was soon found to be insufficient. For though it might have accounted for the change of declination in gamma Draconis, yet it would not at the same time agree with the phenomena in other stars, particularly in a small one almost opposite in right ascension[1] to gamma Draconis, at about the same distance from the north pole of the equator. For though the star seemed to move the same way as a nutation of the earth's axis would have made it, yet, it changing its declination but about half as much as gamma Draconis in the same time (as appeared upon comparing the observations of both made upon the same days at different seasons of the year), this plainly proves that the apparent motion of the stars was not occasioned by a real nutation, since if that had been the cause the alteration in both stars would have been near equal.

The great regularity of the observations left no room to doubt but that there was some regular cause that produced this unexpected motion which did not depend on the uncertainty or variety of the seasons of the year. Upon comparing the observations with each other it was discovered that in both forementioned stars the apparent difference of declination from the maxima was always nearly proportional to the versed sine [to 1 – cosine] of the sun's distance from the equinoctial points.

[3] But not being able to frame any hypothesis at that time sufficient to solve all the phenomena and being very desirous to search a little farther into this matter I began to think of erecting an instrument for myself at Wansted, that, having it always at hand, I might with the more ease and certainty inquire into the laws of this new motion. The consideration likewise of being able by another instrument to confirm the truth of the observations hitherto made with Molyneux's was no small inducement to me. But the chief of all was the opportunity I should thereby have of trying in what manner other stars were affected by the same cause, whatever it was. For Moyneux's instrument being originally designed for observing gamma Draconis (as I said before, to try whether it had any sensible parallax) was so contrived as to be capable of but little alteration in its direction, not above seven or eight minutes of a degree. And there being few stars within half that distance from the zenith of Kew bright enough to be well observed, he could not with his instrument thoroughly examine how this cause affected stars differently situated with respect to the equinoctial and solstitial points of the ecliptic.

These considerations determined me, and by the contrivance and direction of the same ingenious person, Graham, my instrument was fixed up Aug. 19, 1727. As I had no convenient place where I could make use of so long a telescope as Molyneux's, I contended myself with one of but little more than half the length of his, (viz. about 121/2 feet, his being 241/4), judging from the experience which I had already had, that this radius would be long enough to adjust the instrument to a sufficient degree of exactness. And I have had no reason since to change my opinion for from all the trials I have yet made, I am very well satisfied that when it is carefully rectified, its situation may be securely depended to half a second. As the place where my instrument was to be hung in some measure determined its radius, so did it also the length of the arch, or limb, on which the divisions were made to adjust it. For the arch could not conveniently be extended farther than to reach in about 61/4° on each side of my zenith. This indeed was sufficient since it gave me an opportunity of making choice of several stars, very different both in magnitude and situation, there being more than 200 inserted in the British catalogue[2] that may be observed with it. I needed not to have extended the limb so far, but that I was willing to take in Capella, the only star of the first magnitude that comes so near my zenith.

[4] My instrument being fixed, I immediately began to observe such stars as I judged most proper to give me light into the cause of the motion already mentioned. There was variety enough of small ones and not less than twelve that I could observe through all the seasons of the year, they being bright enough to be seen in the daytime when nearest the sun. I had not been long observing before I perceived that the motion we had before entertained of the stars being farthest north and south when the sun was about the equinoxes was only true of those that were near the solstitial colure. And after I had continued my observations a few months I discovered what I then apprehended to be a general law observed by all the stars, viz. that each of them became stationary or was farthest north or south when they passed over my zenith at six of the clock, either in the morning or evening. I perceived likewise that whatever situation the stars were in with respect to the cardinal points of the ecliptic, the apparent motion of every one tended the same way when they passed my instrument about the same hour of the day or night. For they all moved southward while they passed in the day, and northward in the night so that each was farthest north when it came about six of the clock in the evening and farthest south when it came about six in the morning

Though I have since discovered that the maxima in most of these stars do not happen exactly when they come to my instrument at those hours, yet not being able at that time to prove the contrary and supposing that they did, I endeavoured to find out what proportion the greatest alterations of declination in different stars bore to each other, it being evident that they did not all change their declination equally. I have before taken notice that it appeared from Molyneux's observations that gamma Draconis altered its declination about twice as much as the forementioned small star almost opposite to it. But examining the matter more particularly I found that the greatest alteration in these stars was as the sine of the latitude of each respectively. This made me suspect that there might be the like proportion between the maxima of other stars, but finding that the observation of some of them would not perfectly correspond with such an hypothesis, and not knowing whether the small difference I met with might not be owing to the uncertainty and error of the observations, I deferred the farther examination into the truth of this hypothesis till I should be furnished with a series of observations made in all parts of the year which might enable me not only to determine what errors are the observations liable to or how far they may be safely depended upon but also to judge whether there had been any sensible change in the parts of the instrument itself.

Upon these considerations I laid aside all thoughts at that time about the cause of the forementioned phenomena hoping that I should the easier discover it when I was better provided with proper means to determine more precisely what they were.

[5] When the year was completed I began to examine and compare my observations, and having pretty well satisfied myself as to the general laws of the phenomena I then endeavoured to find out the cause of them. I was already convinced that the apparent motion of the stars was not owing to the nutation of the earth's axis. The next thing that offered itself was an alteration in the direction of the plumb-line with which the instrument was constantly rectified, but this upon trial proved insufficient. Then I considered what refraction might do but here also nothing satisfactory occurred. At last I conjectured that all the phenomena hitherto mentioned proceeded from the progressive motion of light and the earth's annual motion in its orbit[1]. For I perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object and that when the eye is moving in different directions, the apparent place of the object would be different.

I considered this matter in the following manner. [I omit a lengthy discussion about the influence of the mutual motion of light and eye.]

These particulars being sufficient for my present purpose, I shall not detain you with the recital of any more, or with any farther explication of these. It may be time enough to enlarge more upon this head when I give a description of the instruments etc. if that be judged necessary to be done and when I shall find what I now advance to be allowed of (as I flatter myself it will) as something more than a bare hypothesis. I have purposely omitted some matters of no great moment and considered the earth as moving in a circle and not an ellipse, to avoid too perplexed a calculus, which, after all the trouble of it, would not sensibly differ from that which I make use of, especially in those consequences which I shall at present draw from the foregoing hypothesis.

[6] This being premised, I shall now proceed to determine from the observations what the real proportion is between the velocity of light and the velocity of the earth's annual motion in its orbit upon supposing that the phenomena before mentioned do depend upon the causes I have here assigned. But I must first let you know that in all observations hereafter mentioned I have made an allowance for the change of the star's declination on the account of the precession of the equinox upon supposition that the alteration from this cause is proportional to the time and regular through all parts of the year. I have deduced the real annual alteration of declination of each star from the observations themselves and I the rather choose to depend upon them because all which I have yet made concur to prove that the stars near the equinoctial colure change their declination at this time 11/2 or 2´´ in a year more than they would do if the precession was only 50´´ as it is now generally supposed[3]. I have likewise met with some small varieties in the declination of other stars in different years which do not seem to proceed from the same the same cause, particularly in those that are near the solstitial colure, which on the contrary have altered their declination less than they ought if the precession was 50´´. But whether these small alterations proceed from a regular cause or are occasioned by any change in the materials etc. of my instrument, I am not yet able to fully to determine. However, I thought it might not be amiss just to mention to you how I have endeavoured to allow for them though the result would have been nearly the same if I had not considered them at all. What that is I will show, first, from the observations of gamma Draconis which was found to be 39´´ more southerly in the beginning of March than in September.

From what has been premised it will appear that the greatest alteration of the apparent declination of gamma Draconis on account of the successive propagation of light would be to the diameter of the little circle which a star (as was before remarked) would seem to describe about the pole of the ecliptic as 39 to 40.4. […] Whence it would follow that light moves or is propagated as far as from the sun to the earth in 8´12´´[4].

It is well known that Roemer who first attempted to account for an apparent inequality in the times of the eclipses of Jupiter's satellites by the hypothesis of the progressive motion of light supposed that it spent about 11 minutes of time in its passage from the sun to us. But it has since been concluded by others from the like eclipses that it is propagated as far in about 7 minutes. The velocity of light therefore deduced from the foregoing hypothesis is at it were a mean betwixt what had at different times been determined from the eclipses of Jupiter's satellites.

These different methods of finding the velocity of light thus agreeing in the result, we may reasonably conclude not only that these phenomena are owing to the causes to which they were ascribed but also that light is propagated (in the same medium) with the same velocity after it has been reflected as before, for this will be the consequence if we allow that the light of the sun is propagated with the same velocity before it is reflected as the light of the fixed stars. And I imagine this will scarce be questioned if it can be made appear that the velocity of light of all the fixed stars is equal, and that their light moves or is propagated through equal spaces in equal times, at all distances from them. Both which points (as I apprehend) are sufficiently proved from the apparent alteration of the declination of stars of different lustre. For that is not sensibly different in such stars as seem near together though they appear of very different magnitudes. And whatever their situations are (if I proceed according to the foregoing hypothesis) I find the same velocity of light from my observations of small stars of the fifth or sixth as from those of the second and third magnitude which in all probability are placed at very different distances from us. The small star, for example, before spoken of, that is almost opposite to gamma Draconis (being the 35th Camelopard. Hevelii in Flamsteed's Catalogue) was 19´´ more northerly about the beginning of March than in September. Whence I conclude, according to my hypothesis, that the diameter of the little circle described by a star in the pole of the ecliptic would be 40´´.2

The last star of the Great Bear's tail of the second magnitude (marked ita by Bayer) was 36´´ more southerly about the middle of January than in July. Hence the maximum or greatest alteration of declination of a star in the pole of the ecliptic would be 40´´.4, exactly the same as was before found from the observations of gamma Draconis.

The star of the fifth magnitude in the head of Perseus marked tau by Bayer was 25´´ more northerly about the end of December than on the 29th of July following. Hence the maximum would be 41´´. This star is not bright enough to be seen as it passes over my zenith about the end of June when it should be, according to the hypothesis, farthest south. But because I can more certainly depend upon the greatest alteration of declination of those stars, which I have frequently observed about the times when they become stationary with respect to the motion I am now considering, I will set down a few more instances of such from which you may be able to judge how near it may be possible from these observations to determine with what velocity light is propagated.

Alpha Persei Bayeri was 23´´ more northerly at the beginning of January than in July, hence the maximum would be 40´´.2. Alpha Cassiopeae was 34´´ more northerly about the end of December than in June, hence the maximum would be 40´´.8. Betta Draconis was 39´´ more northerly in the beginning of September than in March, hence the maximum would be 40´´.2. Capella was about 16´´ more southerly in August than in February, hence the maximum would be about 40´´. But this star being farther from my zenith than those I have before made use of, I cannot so well depend upon my observations of it as of the others because I meet with some small alterations of its declination that do not seem to proceed from the cause I am now considering.

I have compared the observations of several other stars and they all conspire to prove that the maximum is about 40 or 41´´. I will therefore suppose that it is 40´´1/2, or (which amounts to the same) that light moves or is propagated as far as from the sun to us in 8´13´´. The near agreement which I met with among my observations induces me to think that the maximum (as I have here fixed) cannot differ so much as a second from the truth, and therefore it is probable that the time which light spends in passing from the sun to us may be determined by these observations within 5 or 10 seconds which is such a degree of exactness as we can never hope to attain from the eclipses of Jupiter's satellites.

[7] Having thus found the maximum, or what the greatest alteration of declination would be in a star placed in the pole of the ecliptic, I will now deduce from it (according to the foregoing hypothesis) the alteration in one or two stars at such times as they were actually observed to see how the hypothesis will correspond with the phenomena through all the parts of the year.

It would be too tedious to set down the whole series of my observations, I will therefore make choice only of such as are most proper for my present purpose and will begin with those of gamma Draconis.

This star appeared farthest north about September 7th 1727 as it ought to have done according to my hypothesis. The following table shows how much more southerly the star was found to be by observation in several parts of the year and likewise how much more southerly it ought to be according to the hypothesis[5].

Hence it appears that the hypothesis corresponds with the observations of this star through all parts of the year for the small differences between them seem to arise from the uncertainty of the observations which is occasioned (as I imagine) chiefly by the tremulous or undulating motion of the air, and of the vapours in it which causes the stars sometimes to dance to and fro so much that it is difficult to judge when they are exactly on the middle of the wire that is fixed in the common focus of the glasses of the telescope.

I must confess to you that the agreement of the observations with each other as well as with the hypothesis is much greater than I expected to find before I had compared them and it may possibly be thought to be too great by those who have been used to astronomical observations and know how difficult it is to make such as are in all respects exact. But if it would be any satisfaction to such persons (till I have an opportunity of describing my instrument and the manner of using it) I could assure them that in above 70 observations which I made of this star in a year there is but one (and that is noted as very dubious on account of clouds) which differs from the foregoing hypothesis more than 2´´ and this does not differ 3´´.

This therefore being the fact I cannot but think it very probable that the phenomena proceed from the cause I have assigned since the foregoing observations make it sufficiently evident that the effect of the real cause whatever it is varies in this star in the same proportion that it ought according to the hypothesis.

But lest gamma Draconis may be thought not so proper to show the proportion in which the apparent alteration of declination is increased or diminished as those stars which lie near the equinoctial colure, I will give you also the comparison between the hypothesis and the observations of eta *Ursae majoris*, that which was farthest south about the 17th day of Jan. 1728, agreeable to the hypothesis. The following table shows how much more northerly it was found by observation in several parts of the year and also what the difference should have been according to the hypothesis.

[8] I find upon examination that the hypothesis agrees altogether as exactly with the observations of this star as the former. For in about 50 that were made of it in a year I do not meet with a difference of as much as 2´´ except in one which is marked as doubtful on account of the undulation of the air etc. and this does not differ 3´´ from the hypothesis.

The agreement between the hypothesis and the observations of this star is the more to be regarded since it proves that the alteration of the declination on account of the precession pf the equinox is (as I before supposed) regular through all parts of the year, so far at least as not to occasion a difference great enough to be discovered with this instrument. It likewise proves the other part of my former supposition, viz. that the annual alteration of the declination in stars near the equinoctial colure is at this time greater than a precession of 50´´ would occasion for this star was 20´´ more southerly in Sept. 1728 than in Sept, 1727, that is, about 2´´ more than it would have been if the precession was but 50´´. But I may hereafter perhaps be better able to determine this point from my observations of those stars that lie near the equinoctial colure at about the same distance from the north pole of the equator and nearly opposite in right ascension.

[9] I think it is needless to give you the comparison between the hypothesis and the observations of any more stars since the agreement in the foregoing is a kind of demonstration (whether it be allowed that I have discovered the real cause of the phenomena or not) that the hypothesis gives at least the true law of the variation of declination in different stars with respect to their different situations and aspects with the sun. And if this is the case, it must be granted that the parallax of the fixed stars is much smaller than has been hitherto supposed by those who have pretended to deduce it from their observations. I believe that I may venture to say that in either of the two stars last mentioned it does not amount to 2´´. I am of opinion that if it were 1´´ I should have perceived it in the great number of observations that I made, especially of gamma Draconis which agreeing with the hypothesis (without allowing anything for parallax) nearly as well when the sun was in conjunction with, as in opposition to this star. It seems very probable that the parallax of it is not so great as one single second. And consequently that it is above 400,000 times farther from us than the sun[6].

There appearing therefore after all no sensible parallax in the fixed stars the Anti-Copernicans have still room on that account to object against the motion of the earth and they may have (if they please) a much greater objection against the hypothesis by which I have endeavoured to solve the forementioned phenomena by denying the progressive motion of light, as well as that of the earth.

But as I do not apprehend that either of these postulates will be denied me by the generality of the astronomers and philosophers of the present age so I shall not doubt of obtaining their ascent to the consequences which I have deduced from them if they are such as have the approbation of so great a judge of them as yourself.

Notes

1. Right ascension changes from 0 to 24 hours so that opposite apparently meant a difference of 12 hours or 180 °.

2. Apparently the Flamsteed catalogue of 1712 and 1725.

3. This constant is now assumed as 50´´.3.

…**4.** A distance 400,000 times greater than the astronomical unit corresponds to parallax 0´´.5.

5. I do not reproduce either this or the following table since their data are sufficiently described in the main text.

6. The distance of that star is 6.3 lightyears.

Bayer published his catalogue in 1603 and Flamsteed, in 1712 and 1725. Hippocrates, Tycho and Bradley are justly considered the best observers before Gauss and Bessel. Bradley approached observations extremely cautiously and paid due attention to other extremely important topics, velocity of light and distances between celestial objects. A special point is his repeated mention of uncertainty of observations. In § 7 it becomes clear that he meant systematic errors. Another point is Bradley's apparent ignorance of the notion of expectation which was formally introduced by Huygens.

Postscript

As to observations of Dr. Hooke, I must own to you that before Molyneux's instrument was erected I had no small opinion of their corrcctness. The length of his telescope and the care that he pretends to have taken in making them exact, having been strong inducements with me to think them so. And since I have been convinced both from Molyneux's observations and my own that the Doctor's are really very far from being exact or agreeable to the phenomena. I am greatly at a loss how to account for it. I cannot well conceive that an instrument of the length of 36 feet, constructed in the manner he describes his, could have been liable to an error of near 30´´ (which was doubtless the case) if rectified with so much care as he represents.

The observations of Flamsteed of the different distances of the pole star from the pole at different times of the year which were through mistake looked upon by some as a proof of the annual parallax of it, seem to have been made with much greater care than those of Dr. Hooke. For though they do not all exactly correspond with each other, yet from the whole Flamsteed concluded that the star was 35, 40 or 45´´ nearer the pole in December than in May or July. And according to my hypothesis it ought to appear 40´´ nearer in December than in June. The agreement therefore of the observations with the hypothesis is greater than could reasonably be expected, considering the radius of the instrument and the manner in which it was constructed.

IV

J. Bradley

Letter to Rt. Hon. George Earl of Macclesfield Concerning an apparent motion observed in some of the fixed stars

Phil. Trans. Roy. Soc., vol. 45, 1750
Reprint: S, P. Rigaud, J. Bradley, *Misc. works and correspondence*. London, 1832; New York – London, 1972, pp. 17 – 41

[1] My Lord, The great exactness with which instruments are now constructed has enabled the astronomers of the present age to discover several changes in the positions of the heavenly bodies, which, by reason of their smallness had escaped the notice of their predecessors. And although the causes of such motions have always subsisted, yet philosophers had not so fully considered what the effects of those known causes would be, as to demonstrate *a priori* the phenomena they might produce, so that theory itself is here, as well as in many other cases, indebted to practice, for the discovery of some of its most elegant deductions. This points to us the great advantage of cultivating this, as every other branch of natural knowledge by a regular series of observations and experiments.

The progress of astronomy indeed has always been found to have so great a dependence upon accurate observations that till such were made, it advanced but slowly: for the first considerable improvements that it received in point of theory were owing to the renowned Tycho Brahe; who, far exceeding those that had gone before him in the exactness of his observations, enabled the sagacious Kepler to find out some of the principal laws relating to the motion of the heavenly bodies[1]. The invention of telescopes and pendulum clocks affording proper means of still farther improving the praxis of astronomy, and these being also soon succeeded by the wonderful discoveries made by our great Newton as to its theory. The science in both respects had acquired such extraordinary advancement, that future ages seemed to have little room left for making any great improvements. But in fact we find the case to be very different for, as we advance in the means of making more nice inquiries, new points generally offer themselves that demand our attention. The subject of my present letter to your lordship is a proof of the truth of this remark. For as soon as I had discovered the cause and settled the laws of the aberrations of the fixed stars, arising from the motion of light etc., whereof I gave an account [in this collection], my attention was again excited by another phenomenon, viz., an annual change of declination in some of the fixed stars which appeared to be sensibly greater about that time than a precession of the equinoctial points of 50´´ in a year would have occasioned. The quantity of the difference, although small in itself, was rendered perceptible through the exactness of my instrument even in the first year of my observations. But being then at a loss to guess from what cause that greater change of declination proceeded, I endeavoured to allow for it in my computations by making use of the observed annual difference as mentioned [author's paper in same collection, end of § 4 and § 6].

[2] From that time to the present I have continued to make observations at Wansted as opportunity offered with a view of discovering the laws and cause of the phenomenon. For by the favour of my very kind and worthy friend Matthew Wymondesold, esq my instrument has remained where it was first erected so that I have been able without any interruption, which the removal of it to another place would have occasioned, to proceed on with my intended series of observations for the space of 20 years, a term somewhat exceeding the whole period of the changes that happen in this phenomenon.

When I shall mention the small quantity of the deviation which the stars are subject to from the cause that I have been so long searching after, I am apprehensive that I may incur the censure of some persons for having spent so much time in the pursuit of such a seeming trifle. But the candid lovers of science will, I hope, make the allowance for that natural ardour with which the mind is urged on towards the discovery of truths, in themselves perhaps of small moment, were it not that they tend to illustrate others of greater use.

The apparent motions of the heavenly bodies are so complicated and affected by such a variety of causes, that in many cases it is extremely difficult to assign to each its due share of influence. Or distinctly to point out what part of the motion is the effect of one cause and what of another. And whilst the joint effects of all are only attended to great irregularities and seeming inconsistencies frequently occur, whereas when we are able to allot to each particular cause its proper effect, harmony and uniformity usually ensue.

Such seeming irregularities being also blended with the unavoidable errors which astronomical observations must be always liable to, as well from the imperfection of our senses as of the instruments that we make use of, have often very much perplexed those who have attempted to solve the phenomena. And till means are discovered whereby we can separate and distinguish the particular part of the whole motion that is owing to each respective cause, it will be impossible to be well assured of the truth of any solution[2]. For these reasons we generally find that the more exact the instruments are that we make use of and the more regular the series of observations is that we take, the sooner we are enabled to discover of any new phenomenon. For when we can be well assured o f the limits wherein the errors of observation are contained and have reduced them within as narrow bounds as possible by the perfection of the instruments which we employ we need not hesitate to ascribe such apparent changes, as manifestly exceed those limits to some other causes. Upon these accounts it is incumbent upon the practical astronomer to set out at first with the examination of the correctness of his instruments and to be assured that they are sufficiently exact for the use he intends to make of them. Or at least he should know within what limits their errors are confined.

[3] This practice has in an eminent manner been lately recommended by your lordships noble example who having out of a singular regard for the science of astronomy, erected an observatory and furnished it with as complete an apparatus of instruments as our best artists could contrive, would not fully rely on their exactness till their divisions had undergone the strictness re-examination. Whereby they are probably now rendered as perfect in their kind as any extant or as human skill can at present produce.

The lovers of this science in general cannot but acknowledge their obligations to your lordship on this account. But I find myself bound to do it since by means of your lordship's most accurate observations I have been enabled to settle some principle elements which I could not at present otherwise have done for want of an instrument at the Royal Observatory proper for that purpose: for the large mural quadrant which is there fixed to observe objects lying southward of the zenith, however perfect an instrument it may be in itself, is not alone sufficient to determine with proper exactness either the latitude of the Observatory or the quantity of refraction corresponding to different altitudes: for it being too heavy to be conveniently removed and the room wherein it is placed being too small to admit of its being turned to the opposite side of the wall whereon it now hangs. I cannot by actual observations of the circumpolar stars settle those necessary points and therefore have endeavoured to do it by comparing my own with your lordship's observations. And until this defect in the apparatus belonging to the Royal Observatory be removed, we must be indebted to your lordship for the knowledge of its true situation.

[4] A mind, intent upon the pursuit of any kind of knowledge will always be agreeably entertained with what can supply the most proper means of attaining it. Such to the practical astronomer are exact and well-contrived instruments, and I reflect with pleasure on the opportunities I have enjoyed of cultivating an acquaintance and friendship with the person that, of all others, has most contributed to their improvement. For I am sensible that, if my own endeavours have, in any respect, been effectual to the advancement of astronomy, it has principally been owing to the advice and assistance given me by our worthy member George Graham whose great skill and judgement in mechanics joined with a complete and practical knowledge of the uses of astronomical instruments enable him to contrive and execute them in the most possible manner.

The gentlemen of the [Paris] Royal Academy of Sciences to whom we are so highly obliged for their exact admeasurement of the quantity of a degree under the arctic circle[3], have already given the world very convincing proofs of his care and abilities in those respects. And the particular delineation which they have lately published of the several parts of the sector which he made for them has now rendered it useless to enter upon any minute description of mine at Wansted, both being constructed upon the same principle and differing in their component parts chiefly on account of the different purposes for which they were intended.

As mine was originally designed to take only the differences of the zenith distances of stars in the various seasons of the year without any view of discovering their real places, I had no occasion to know exactly what point on the limb corresponded to the true zenith. Therefore no provision was made in my sector for the changing of its situation for that purpose. Neither was it necessary that the divisions or points on the arc should be set off with the utmost accuracy, equidistant from each other because when I observe any particular star the same spot or point being first bisected by the plumb-line and then the screw of the micrometer turned until the star appears upon the middle of the wire that is fixed in the common focus of the glasses of the telescope. I can thereby collect how far the star is from that given point at the time of observation. And afterwards by comparing together the several observations that are made of it I am able to discover what apparent change has happened. The quantity of the visible alteration in the position in the position of the stars being expressed by revolutions and parts of a revolution of the screw of the micrometer, I endeavoured to determine with great care the true angle answering thereto. And after various trials I thoroughly satisfied myself both of the equality of the threads of the screw and of the precise number of seconds corresponding to them[4].

But although these points could be settled with great certainty, I was nevertheless obliged to make one supposition which perhaps to some may seem of too great moment in the present inquiry to be admitted without an evident proof from facts and experiments. For I suppose that the line of collimation of my telescope has invariably preserved the same direction with respect to the divisions upon the arc during the whole course of my observations. And indeed it was on account of the objections which might have been raised against such a postulate that I thought it necessary to continue my series of observations for so many years before I published my conclusions which I shall at present endeavour to draw from them.

Whoever compares the result of the several trials that have been made by the gentlemen of the [Paris] Academy of Sciences for determining the zenith point of their sector since their return from the north, will, I presume, allow that mine is not an unreasonable or precarious supposition since it is evident from their observations that the line of collimation of that instrument of that instrument underwent no sensible change in its direction during the space of more than a whole year, although it was several times taken down and set up again in different and remote places, whereas mine has always remained suspended in the same place.

But besides such a strong argument for the probability of the truth of my supposition, I have the satisfaction of finding it actually verified by the observations themselves; which plainly prove that at the end of the full period of the deviations which I am going to mention the stars are found to have the same positions by the instrument as they ought to have supposing the line of collimation to have continued unaltered from the time when I first began to observe.

[5] I have already taken notice in what manner this phenomenon discovered itself to me at the end of my first year's observations, viz. by a greater apparent change of declination in the stars near the equinoctial colure than could arise from a precession of 50´´ in a year; the mean quantity now usually allowed by astronomers. But there appearing at the same time an effect of a quite contrary nature in some stars near the solstitial colure which seemed to alter their declination less than a precession of 50´´ required. I was thereby convinced that all the phenomena in the different stars could not be accounted for merely by supposing that I had assumed a wrong quantity for the precession of the equinoctial points.

At first I had a suspicion that some of these small apparent alterations in the places of the stars might possibly be occasioned by a change in the materials, or in the position of the parts of my sector, but upon considering how firmly the arc on which the divisions or points are made, is fastened to the plate wherein the wire is fixed that lies in the focus of the object-glass, I saw no reason to apprehend that any change could have happened in the position of that wire and those points. The suspension therefore of the plummet being the most likely cause from whence I conceived any uncertainty could arise, and the wire of which had been broken three or four times in the first year of my observations, I attempted to examine whether part of the forementioned apparent motions might not have been owing to the different plumb-lines that had been made use of. To determine this I adjusted a particular point of the arc to the plumb-line with all the exactness I could. And then, taking off the old wire I immediately hung on another, with which the same spot was again compared. I repeated the experiment three or four times and thereby fully satisfied myself that no sensible error could arise rom the use of different plumb-lines since the various adjustments of the same point agreed with each other within less than half a second.

[6] Having then from each trials sufficient reason to conclude that these second unexpected deviations of the stars were not owing to any imperfection of my instrument, after I had settled the laws of abberations arising from the motion of light etc. I judged it proper to continue my observations of the same stars, hoping that by a regular and longer series of them carried on through several succeeding years I might at length be enabled to discover the real cause of each apparent inconsistencies.

As I resided chiefly at Wansted, after my sector was erected there in the year 1727, till the beginning of May 1732, when I removed from thence to Oxford, I had during my abode at Wansted frequent opportunities of repeating my observations and thereby discovered so many particulars relating to these phenomena that I began to guess what was the real cause of them.

It appeared from my observations that, during this interval of time some of the stars near the solstitial colure had changed their declination 9 or 10´´ less than a precession of 50´´ would have produced. And at the same time that others near the equinoctial colure had altered theirs about the same quantity more than a like precession would have occasioned. The north pole of the equator seeming to have approached the stars which come to the meridian with the sun about the vernal equinox and the winter solstice and to have receded from those which come to the meridian with the sun about the autumnal equinox and the summer solstice.

When I considered these circumstances and the situation of the ascending node of the moon's orbit at the time when I first began my observations I suspected that the moon's action upon the equatorial parts of the earth might produce these effects: for if the precession of the equinox be, according to Sir Isaac Newton's principles caused by the actions of the sun and the moon upon these parts, the plane of the moon's orbit being at one time above ten degrees more inclined to the plane of the equator than at another, it was reasonable to conclude that the part of the whole annual precession which arises from her action would in different years be varied in its quantity whereas the plane of the ecliptic wherein the sun appears keeping always nearly the same inclination to the equator that part of the precession which is owing to the sun's action may be the same every year. Hence it would follow that although the mean annual precession proceeding from the joint actions of the sun and the moon were 50´´ yet the apparent annual precession might sometimes exceed and sometimes and sometimes fall short of that mean quantity according to the various situations of the nodes of the moon's orbit.

[7] In the year 1727 when my instrument was first set up, the moon's ascending node was near the beginning of Aries; and consequently her orbit was as much inclined to the equator as it can at any time be; and then the apparent annual precession was found by my first year's observations to be greater than the mean: which proved that the stars near the equinoctial colure, whose declinations are most of all affected by the precession, had changed theirs above a tenth part more than a precession of 50´´ would have caused. The succeeding year's observations proved the same thing; and in three or four years' time the difference became so considerable as to leave no room to suspect that it was owing to any imperfection either of the instrument or observations.

But some of the stars which I had observed that were near the solstitial colure, having appeared to move during the same time in a manner contrary to what they ought to have done by an increase in the precession; and the deviations in them being as remarkable as in the others, I perceived that something more than a mere change in the quantity of precession would be requisite to solve this part of the phenomenon. Upon comparing my observations of stars near the solstitial colure that were almost opposite to each other in right ascension, I found that they were equally affected by this cause; for whilst gamma Draconis appeared to have moved northward, the small star which is the 35th Camelopardali Hevel, in the British Catalogue, seemed to have gone as much toward the south: which showed that this apparent motion in both these stars might proceed from a nutation in the earth's axis; whereas the comparison of my observations of the same stars formerly enabled me to draw a different conclusion with respect to the cause of the annual aberrations arising from the motion of light. For the apparent alteration in gamma Draconis from that cause being as great again as in the other small star proved that the phenomenon did not proceed from a nutation of the earth's axis; as, on the contrary, this may. Upon making the like comparison between the observations of other stars that lie nearly opposite in right ascension, whatever their situations were with respect to the cardinal points of the equator, it appeared that their change of declination was nearly equal but contrary and such as a nutation or motion of the earth's axis would affect,

[8] The moon's ascending node being got back towards the beginning of Capricorn in the year 1732, the stars near the equinoctial colure appeared about that time to change their declination no more than a precession of 50´´ required; whilst some of those near the solstitial altered theirs above 2´´ in a year less than they ought. Soon after, I perceived the annual change of declination of the former to be diminished, so as to become less than 50´´ of precession would cause. And it continued to diminish till the year 1736, when the moon's ascending node was about the beginning of Libra, and her orbit had the least inclination to the equator. But by this time some of the stars near the solstitial colure had altered their declinations 18´´ less since the year 1727, than they ought to have done from a precession of 50´´. For gamma Draconis, which in those nine years should have gone about 8´´ more southerly, was observed in 1736 to appear 10´´ more northerly than it did in the year 1727.

As this appearance in gamma Draconis indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic; and as several astronomers have supposed that inclination to diminish regularly; if this phenomenon depended upon such a cause, and amounted to 18´´ in nine years, the obliquity of the ecliptic would at that rate alter a whole minute in 30 years: which is much faster than any observations before made would allow. I had reason therefore to think that some part of this motion at the least, if not the whole, was owing to the moon's action upon the equatorial parts of the earth; which I conceived might cause a libratory motion of the earth's axis. But as I was unable to judge from only nine years' observations, whether the axis would entirely recover the same position that it had in the year 1727, I found it necessary to continue my observations through a whole period of the moon's nodes[5]; at the end of which I had the satisfaction to see that the stars returned to the same positions again, as if there had been no alteration at all in the inclination of the earth's axis; which fully convinced me that I had guessed rightly as to the cause of the phenomena. This circumstance proves likewise, that if there be a gradual diminution of the obliquity of the ecliptic, it does not arise only from an alteration in the position of the earth's axis, but rather from some change in the plane of the ecliptic itself; because the stars, at the end of the period of the moon's nodes, appeared in the same places, with respect to the equator, as they ought to have done, if the earth's axis had retained the same inclination to an invariable plane.

During the course of my observations our ingenious secretary of the Royal Society. John Machin, being employed in considering the theory of gravity, and its consequences with regard to the celestial motions, I acquainted him with the phenomena that I had observed, and at the same time mentioned what I suspected to be the cause of them. He soon after sent me a table, containing the quantity of the annual precession in the various positions of the moon's nodes, as also the corresponding nutations of the earth's axis; which was computed upon the supposition that the mean annual precession is 50´´, and that the whole is governed by the pole of the moon's orbit only: and therefore he imagined that the numbers in the table would be too large, as in fact they were found to be. But it appeared that the changes which I had observed, both in the annual precession and nutation, kept to the same law, as to increasing and decreasing, with the numbers of his table. These were calculated upon the supposition that the pole of the equator, during a period of the moon‘s nodes, moved round in the periphery of a little circle whose centre was 23°29´ distant from the pole of the ecliptic, having itself also an angular motion of 50´´ in a year about the same pole: the north pole of the equator was conceived to be in that part of the small circle which is farthest from the north pole of the ecliptic at the time when the moon's ascending note is in the beginning of Aries, and in the opposite point of it when the same node is in Libra.

Such a hypothesis will account for an acceleration and retardation of the annual precession, as also for a nutation of the earth's axis, and if the diameter of the little circle be supposed equal to 18´´, which is the whole quantity of the nutation, as collected from my observation of gamma Draconis, then all the phenomena in the several stars which I observed will be very nearly solved by it. [Here follows a lengthy explanation of precession and nutation as well as a summary of the results of Bradley's observations.]

[9] I have endeavoured to find the exact quantity of the mean precession of the equinoctial points by comparing my own observations made at Greenwich with those of Tycho Brahe and others, which I judged to be most proper for that purpose. But as many of the stars which I compared gave a different quantity, I shall assume the mean result, which gives a precession of one degree in 711/2 years [50´´.3 annualy]; this agreeing very well likewise with my observations that were taken at Wansted. The numbers in the following tables, which express the change of declination in each star are computed upon the supposition that the mean obliquity of the ecliptic was 23°28´30´´, and that it con-tinued the same during the whole course of my observations. And as the moon's ascending node was in the beginning of Aries about the 27th day of March 1727, I have reduced the place of each star to that time; by allowing the proper change of declination from that day to the day of each respective observation.

It being also necessary to make an allowance for the aberrations of light, I have again examined my observations that were most proper to determine the transverse axis of the ellipsis which each star seems to describe, and have found it to be nearest to 40´´; this number I therefore make use of in the following computations.

The divisions or points upon the limb of my sector are placed 5 minutes of a degree from each other, and are numbered to show the polar distances nearly, the true polar distance exceeding that which is shown by the instrument about 1´35´´. When I first began to observe, I generally made use of that point on the limb which was nearest to the star's polar distance, without regarding whether it was more northerly or more southerly than the star; but as it sometimes happened that the original point, with which I at first compared the star, became in process of time pretty remote from it; I afterwards brought the plummet to another point that was nearer to it, and carefully examined what number what number of revolutions of the screw of the micrometer etc. corresponded to the distance between the different points that I had made of: by which means I was able to reduce all the observations of the same star to the same point without supposing the several divisions to be accurately 5´´ asunder.

I have expressed the distance of each star from the point of the arc with which it was compared in seconds of a degree and tenth parts of a second, exactly as it was collected from the observations; although I am sensible that the observations themselves are liable to an error of more than a whole second; because I meet with some that have been made within 2 or 3 days of each other that differ 2´´ even when they are not marked as defective in any respect.

It would be too tedious to set down the whole number of the observations that I have made and therefore I shall give only enough of them to show their correspondency with the forementioned hypothesis in the several years wherein any were made of the stars here recited. When several observations have been taken of the same star within a few days of each other, I have either set down the mean result, or that observation which best agreed with it. I have likewise commonly chosen those that were made near the same season of the year, in such stars as gave me the opportunity of making that choice, particularly in gamma Draconis, which was generally observed about the end of August or the beginning of September; that being the usual time when I went to Wansted on purpose to observe both that and also some of the stars in the Great Bear. But the weather proving cloudy at that season, in the year 1744, prevented my making a single observation either of gamma Draconis or any other star while I was there, which is the cause of one vacancy in a series of 20 succeeding years, wherein that particular star had been observed. Such stars as were either not visible in the daytime towards the beginning of September, or came at such hours of the night as would have incommoded the family of the house wherein the instrument is fixed, were but seldom observed after I went to reside at Oxford; which is the reason why the series of observations of those is so imperfect, as sometimes to leave a chasm for several years together. But notwithstanding this, I doubt not but upon the whole they will be found sufficient to satisfy your lordship of the general correspondency between the hypothesis and the phenomena in the several stars, however different their situations are with respect to the cardinal points of the equator.

As I made more observations of gamma Draconis than of any other star, and it being likewise very near the zenith of Wansted, I will begin with the recital of some of them. The point upon the limb with which this star was compared was 38°25´ from the north pole of the equator, according to the numbers of the arc of my sector. The first column in the following table [many tables are inserted; I do not reproduce any of them] shows the year and the day of [and] the month when the observations were made; the next gives the number of seconds that the star was found to be south of 38°25´; the third contains the alterations of the polar distance which the mean precession, at the rate of one degree in 711/2 years would cause in this star from the 27th day of March 1727 to the day of observation; the fourth shows the aberrations of light; the fifth, the equation arising; and the sixth gives the mean distance of the star from the point with which it was compared, found by collecting the several numbers, according to their signs, in the third, fourth anf fifth columns, and applying them to the observed distances contained in the second.

If the observations had been perfectly exact, and the several equations of their due quantity, then all the numbers in the last column would have been equal. But since they differ a little from one another, if the mean of all be taken, and the extremes are compared with it, we shall find no greater difference than what may be supposed to arise from the uncertainty of the observations themselves; it nowhere amounting to more than 1´´1/2. The hypothesis therefore seems in this star to agree extremely well with the observations here set down. But as I had made above 300 of it, I took the trouble of comparing each of them with the hypothesis; and although it might have been expected that in so large a number some great errors would have occurred, yet there are very few, viz. only 11 that differ from the mean of these so much as 2´´, and not one that differs so much as 3´´. This surprising agreement, therefore, in so long a series of observations taken in all the various seasons of the year, as well as in the different positions of the moon's nodes, seems to be a sufficient proof both of the hypothesis and also of that which I formerly advanced, relating to the aberration of light; since the polar distance in this star may differ in certain circumstances almost a minute, viz. 56´´1/2, if the correction resulting from both these hypotheses are neglected; whereas, when those equations are rightly applied, the mean place of the star comes out the same, as nearly as can be reasonably expected.

I made about 250 observations of beta Draconis which I find correspond as well with the hypothesis as those of gamma. But since the positions of both these stars in respect to the solstitial colure differ but little from each other, it will be needless to set down the observations of beta. I shall therefore proceed to lay before your lordship some observations of a small star that is almost opposite to gamma Draconis in right ascension being the 35th Camelopardali Hevel in the British [apparently Flamsteed] Catalogue. Flamsteed indeed has not given the right ascension of this star but that being necessary to be known to compute the change of its declination arising from the precession of the equinox, I compared the time of its transit over the meridian with that of some other stars near the same parallel, whereby I found that its right ascension was 85°54´1/2 at the beginning of the year 1737.

This small star was compared with the same point of the limb of my sector as gamma Draconis; and the second column in the following table shows how many seconds it was found to be south of that point at the time of each respective observation. The other columns contain, as in the foregoing table, the equations that are necessary to find what its mean distance from the same point would have been on the 27th day of March 1727, which is exhibited in the last column. The whole number of my observations of this star did not much exceed 40, the greatest part of which were made before the year 1730. In some of the following years none were taken, and only a single one in any other, except in 1739. However, their correspondency seems sufficient to evince the truth of the hypothesis for if the mean of these contained in the table be taken, not one among the rest of the observations will differ from it more than 2´´.

The observations of the foregoing stars are most proper to prove the change of the inclination of the earth's axis to the plane of the ecliptic. Those which follow will show in what manner the stars that lie near the equinoctial colure are affected as well as others that are differently situated with respect to the cardinal points of the equator. Some of these stars are indeed more remote from the zenith than I would have chosen, if there had been others, of equal lustre, in more proper positions; because experience has long since taught me that the observations of such stars as lie near the zenith do generally agree best with one another and are therefore the fittest to prove the truth of any hypothesis. I shall begin with those near the vernal equinox. Alpha Cassiopeae was compared with the point marked 34°55´; and at first was found to be more southerly, but afterwards became more northerly than that point, as in the following table; the last column of which shows its mean distance south of that point on the 27th of March 1727. The observation on the 23rd day of December in the year 1738 differs 3´´ from the mean of the others, as does also another that was taken five days after this, neither of which being marked as uncertain. I judged it proper to insert one of them; although they give the mean place of the star near 2 seconds more northerly than any other in a series of above 100; all of which correspond with the mean of these here recited within less than 2´´ excepting two that give the star's mean distance almost 3´´ more southerly. But these last mentioned are marked as dubious and in deed they appear to have been bad, by comparing them with several others that were made near the same time, from which they differ almost 2´´.

Although I have taken no observation of tau Persei since the 22nd day of Jan. 1740, yet, as this star is very near the zenith, and a sufficient number were made about the times when the equation resulting from the hypothesis was at its maximum, I judged it proper to insert some of them in the next table; the last column of which shows how much the star's mean distance was south of 38°20´ on the 27th day of March 1727. Among near 60 observations I meet with two only that differ from the mean of these so much as 2´´, and those differ almost as much from the mean of others that were taken near the same time; so that the hypothesis seems to correspond in general with the observations of this star as well as with either of the foregoing.

After the last recited observations it may perhaps seem needless to add those of alpha Persei which is farther from the zenith, but, however, as this star lies very nearly at an equal distance from the equinoctial and solstitial colures, and the series of observations of it is somewhat more complete than that of tau Persei, I shall insert one at least for each year wherein it has been observed, whereby it may appear, that the hypothesis solves the phenomena of stars in this situation as exactly as in others; for if a mean be taken of the numbers in the last column of the following table which expresses the mean distance of the star south of 41°5´ on March 27th, 1727 [this date is many times mentioned below and I will abbreviate it], it will agree within two seconds with everyone of 80 observations that have been made of this star.

Having already given examples of stars lying near both the solstices and the vernal equinox, I shall now add the observations of one that is not far from the autumnal equinox, viz. eta Ursae Majoris, the brightest star in that part of the heavens which approaches the zenith of Wansted within a degree; and which by reason of its lustre and position gave me the opportunity of making my series of observations of it more complete than of many others. This star was compared with the point marked 39°15´ and was south of it, as in the following table; wherein your lordship will see that the observations of the years 1740 and 1741 give the polar distances 3´´ greater than the mean of the other years. Had there been only a single observation taken in either of those years, part of this apparent difference might have been supposed to arise from their uncertainty; but as there were eight observations taken within a week, either before or after the 3rd day of June 1740, which agree well with each other; and three were made within 20 days in Sept. 1741, which likewise correspond with each other, I am inclined to think that the forementioned differences must be owing to something else besides the error of the observations. This phenomenon therefore may deserve the consideration of those gentlemen who have employed their time in making computations relating to the quantity of the effects which the power of gravity may on various occasions produce. For I suspect that the position of the moon's apogee, as well as of her nodes, has some relation to the apparent motions of the stars that I am now speaking of.

My series of observations of several stars abound, of late years, with so many and long interruptions, that I cannot pretend to determine this point; but probably the differences before taken notice of in the observations of alpha Cassiopeae, and some others that I have found likewise among the observations of other stars that are not here recited, may be owing to such a cause; which although it should have any large share of influence, may yet, in certain circumstances, discover a defect in a hypothesis that pays no regard at all to it. But whether these differences do arise from the cause already hinted at, or whether they proceed from any defect of the hypothesis itself in any other respect, it will not be very material in point of practice; since that hypothesis, as it was before laid down, appears to be sufficient to solve all the phenomena to a great degree of exactness as we can in general hope or expect to make observations. For if I take the mean of all the numbers of the last column of the following table for eta Ursea Majoris, and compare it with any one of 164 observations that were taken of it, the difference will not exceed three seconds.

You may perceive, my lord, by inspecting the tables which contain the observations of alpha Cassiopeae and eta Ursae Majoris, that the greatest differences that occur therein may be diminished by supposing the true pole of the equator to move round [a explanation accompanied by a picture follows]. But since this would not entirely remove the inequalities in all the positions of the moon's nodes, I shall refer the more accurate determination of the locus of the true pole to theory;

and at present only give the equations for the precession of the equinoctial points, and the obliquity of the ecliptic, as also the real quantity of the annual precession, to every fifth degree of the place of the moon's ascending node in the following tables, just as they result from the hypothesis as at first laid down; it appearing from what has already been remarked, that these will be sufficiently exact for practice in all cases.
Sir Isaac Newton, in determining the quantity of the annual precession from the theory of gravity, upon supposition that the equatorial is to the polar diameter of the earth as 230 to 229, finds the sun's action sufficient to produce a precession of 9´´1/8 only; and collecting from the tides the proportion between the sun's force and the moon's to be as 1 to 41/2, he settles the mean precession resulting from their joint actions at 50´´. But since the difference between the polar and equatorial diameter is found by the late observations of the gentlemen of the [Paris] Academy of Sciences, to be greater than what sir [!] Isaac had computed it to be; the precession arising from the sun's action must likewise be greater than what he stated it at, nearly in the same proportion. From whence it will follow, that the moon's force must bear a less proportion to the sun's than 41/2 to 1; and perhaps the phenomena which I have now been giving an account of will supply the best data from settling the matter[6].

As I apprehend that the observations already set down will be judged sufficient to prove in general the truth of the hypothesis before advanced, I shall not trouble your lordship with the recital of more than I made of stars lying at greater distances from the zenith; those not being so proper, for the reason before mentioned, to establish the point that I had chiefly in view. But as it may perhaps be of some use to future astronomers to know what were the mean differences of declination at a given time between some stars that lie nearly opposite to one another in right ascension, and not far from either of the colures, I shall set down the result of the comparison of a few that differ so little in declination, that I could determine the quantity of that difference with great certainty.

By the mean of 64 observations that were made of alpha Cassiopeae before the end of the year 1728, I collect, after allowing for the precession, and nutation, as in the forgoing tables, that the mean distance of this star was 68´´.7 south of 34°55´ on the 27th day of March. By a like comparison of 40 observations taken of gamma Ursae Majoris during the same interval of time, I find this star was at the same time 39´´.6 south of 34°45´. I carefully measured with the screw of the micrometer the distance between the points with which these stars were compared and found them to be 9´59´´ from each other, or one second less than they ought to have been. Hence the mean difference of declination between these two stars was 10´28´´.1 on the 27th day of March.

By the mean of 65 observations that were taken of beta Cassiopeae before the end of the year 1728, this star was 25´´.8 north of 32°20´ on the 27th day of March; and by the mean of 52 observations, epsilon Ursae Majoris was 87´´.6 south of 32°30´ at the same time. The distance between these points was found to be 9´59´´.3; from whence it follows that the mean difference of declination between these two stars was 11´52´´.7 on March 27th.

By the mean of 100 observations taken before the end of the year 1728, the mean distance of gamma Draconis was 79´´.8 south of 38°25´ on March 27th 1727; and by the mean of 35 observations, the 35th Camelopard. Hevel was south of the same spot 76´´.4. So that the mean polar distance of gamma Draconis was only 3´´.4 greater than that of the 35th Camelopard. Hevel; but as the equation for the nutation in both these stars was then near the maximum, and to be applied with contrary signs, the apparent polar distance of gamma Draconis was 21´´.4 greater on the 27th day of March.

The differences of the polar distances of the stars, as here set down, may be presumed, both on account of the radius of the instrument and the number of observations, to be very exactly determined, to the time when the moon's ascending node was at the beginning of Aries; and if a like comparison be hereafter made, of observations taken of the same stars, near the same position of the moon's nodes, future astronomers may be enabled to settle the quantity of the mean precession of the equinox, so far as it affects the declination of these stars with great certainty; and they may likewise discover by means of the stars near the solstitial colure, from what cause the apparent change in the obliquity of the ecliptic really proceeds, if the mean obliquity be found to diminish gradually.

The forementioned points indeed can be settled only on the supposition that the angular distances of these stars do continue always the same, or that they have no real motion in themselves, but are at rest in absolute space. A supposition which, though usually made by astronomers, nevertheless seems to be founded on too uncertain principles to be admitted in all cases. For if a judgement may be formed with regard to this matter from the result of the comparison of our best modern observations with such as were formerly made, with any tolerable degree of exactness; there appears to have been a real change in the position of some of the fixed stars with respect to each other; and such as seems independent of any motion in our own system, and can only be referred to some motion in the stars themselves. Arcturus affords a strong proof of this; for if its present declination be compared with its place as determined either by Tycho or Flamsteed, the difference will be found to be much greater than what can be suspected to arise from the uncertainty of their observations.

It is reasonable to expect that other instances of the like kind must also occur among the great number of the visible stars because their relative positions may be altered by various means. For if our own solar system be conceived to change its place with respect to absolute space, this might in process of time occasion an apparent change in the angular distances of the fixed stars, and in such a case the places of the nearest stars being more affected than of those that are very remote, their relative positions might seem to alter, though the stars themselves were really immoveable. And on the other hand, if our own be at rest, and any of the stars really in motion, this might likewise vary their apparent positions; and the more so, the nearer they are to us, or the swifter their motions are, or the more proper the direction of of the motion is, to be rendered perceptible by us. Since then the relative places of the stars may be changed from such a variety of causes, considering that amazing distance at which it is certain some of them are placed, it may require the observations of many ages to determine the laws of the apparent changes even of a single star: much more difficult must it be to settle the laws relating to all the most remarkable stars.

When the causes which affect the places of all the stars in general are known, such as the precession, aberration, and nutation, it may be of singular use to examine nicely the relative situations of particular stars; and especially of those of the greatest lustre, which it may be presumed lie nearest to us, and may therefore subject to more sensible changes, either from their own motion, or from that of our system. And if at the same time that the brighter stars are compared with each other, we likewise determine the relative positions of some of the smallest that appear near them, whose places can be ascertained with sufficient exactness, we may perhaps be able to judge to what cause the change, if any be observable, is owing. The uncertainty that we are at present under, with respect to the degree of accuracy wherewith former astronomers could observe, makes us unable to determine several things relating to the subject that I am now speaking of; but the improvements which have of late years been made in the methods of taking the places of the heavenly bodies, are so great, that in a few years may hereafter be sufficient to settle some points, which cannot now be settled by comparing even the earliest observations with those of the present age.

It were to be wished therefore, that such persons as are provided with proper instruments would attempt to determine with great care the present relative positions of several of the principal stars in various parts of the heavens, especially of those that are least affected by refraction: that cause having many times so uncertain an influence on the places of objects that are very remote from the zenith, that, wherever it is concerned, the conclusions deduced from observations that are much affected by it will always remain doubtful, and too precarious in many cases to be relied upon.

The advantages arising from different persons attempting to settle the same points of astronomy near the same time are so much the greater, as a concurrence in the result would remove all suspicion of incorrectness in the instruments made use of. For this reason I esteem the curious apparatus at Shirburn Castle[7] and the observations there taken, as a most valuable criterion whereby I may judge of the accuracy of those that are made at the Royal Observatory; and as a lover of science, I cannot but wish that our nation abounded with more examples of persons of like rank and ability with your lordship, equally desirous of promoting this, as well as every other branch of natural knowledge, that tends to the honour and benefit of our country.

But were the patrons of arts and sciences ever so numerous, the subject of my present letter is of such a nature, as most direct me to beg leave to address it to the Earl of Macclesfield; not only as a most competent judge of it, but as the sole person in this nation that has instruments proper to examine into the truth of the facts here related.

And it is a particular satisfaction to me, that, after so long an attendance upon these phenomena, I am allowed the honour of transmitting the account of them to the public through your lordship's hand: as it gives me at the same time an opportunity of professing the grateful sense I shall ever retain, both of the signal favours which I formerly received from the noble earl your father[8], and of the many recent obligations conferred upon [me].

Notes

1. Bradley apparently bore in mind Newton's later explanation of Keplerian laws.

2. The study of the action of each cause (factor) became the aim of the analysis of variance.

3. Two meridian arc measurements were needed for that aim. The other arc was measured in Peru.

5. That period covered 18.6 years, and Bradley's observations lasted somewhat longer.

6. According to Newton, precession amounted to 9´´x125x5.5 = 50´´.2. The modern estimate is 50´´.3. Bradley did not insert decimal points!

7. The place of the Macclesfield observatory. Thomas Simpson, in his paper of 1756 on the advantage of taking the mean, also very favourable mentioned that observatory

8. George Macclesfield, a most eminent English lawyer of his time.

Information about some persons

Bayer Johann, 1572 – 1625. Astronomer. His star catalogue appeared in 1603.

Graham George, 1673 – 1751. Watchmaker, manufacturer of astronomical instruments. Fellow of Royal Society.

Macclesfield George, 1697 – 1764. De Moivre's student, astronomer. Bradley's close friend.

Molineux Samuel, 1689 – 1728, Amateur astronomer.

Roemer Ole, 1644 – 1710. Dutch astronomer. In 1676 estimated the velocity of light.

Here, James Bradley describes his discovery of the nutation of the earth's axis a s the result of his painstaking observations which covered about 20 years and, what is usually overlooked, most certainly had to make a great amount of calculations. He also studied precession and was apparently the first to admit the possibility of the movement of our solar system.

His style is extremely bad and in § 9 the calculations of the places of stars should have been presented in a table rather than dolefully described in the text itself. But then, his contribution is difficult to read. There is no summary and the reader is compelled to extract his finding from many other matters. Some points are hard to understand, especially at the end of § 9. There, Bradley discusses the proper motion of stars but does not mention that Halley, in 1718, discovered this phenomenon.

V

K.-R. Biermann

Die Briefe von Martin Bartels an C. F. Gauss

Schriftreihe f. Geschichte der Naturwissenschaften, Technik und Med., Bd. 10, 1973, pp. 5 – 22

1

Als sein *erster Lehrer in der Mathematik*[1] sein vieljäriger[2], sein unvergeßlicher Freund[3] ist Martin Bartels (1769 – 1836) von Carl Friedrich Gauss (1777 – 1855) bezeichnet worden. Der *Princeps mathematicorum* hat Bartels *dankbar verehrt* und *als Mathematiker geachtet*[4]. Bartels wird noch heute in der Sowjetunion als verdienter Universitätsprofessor der Mathematik, vor allem aber als Lehrer des genialen Nikolaj Ivanovic c (1792 – 1856) hochgeschätzt[5]. Schon aus diesen Gründen kann Interesse an dem Briefwechsel zwischen Bartels und Gauss vorausgesetzt werden. Es kommt aber noch mehr hinzu.

Obwohl Friedrich Engel (1861 – 1941) schon 1899 nachgewiesen hat[6], daß Lobacevskij in seiner Entdeckung der nichteuklidischen Geometrie ebenso von Gauss unabhängig gewesen ist wie der hervorragende ungarische Mathematiker Johann (János) Bolyai (1802 – 1860) mit seiner absoluten Ge-ometrie, wurde, wie schon vor Engels Untersuchungen seit 1860[7], so auch danach bis zur Gegenwart[8] immer erneut eine Beeinflussung Lobacevskijs durch Gauss über Bartels vermutet oder behauptet[9], und die Darlegungen von Engel vermochten nicht, die unbegründete Göttinger Tradition[10] einer solchen Einflußnahme aus der Literatur zu verbannen. In diesem Zusammenhang nun kommt den Briefen von Bartels an Gauss Bedeutung zu, und zwar in erster Linie, so paradox es klingt, weil in ihnen mathematische Themen fehlen[11], ist doch daraus zu schließen, das Gauss in Bartels keinen Partner für fachlichen Gedankenaustausch erblickt hat. Hätte Gauss in seinen Briefen mathematische Fragen berührt, so wäre ohne Zweifel Bartels auf sie eingegangen. Insofern ist das Fehlen der Briefe von Gauss an Bartels (vor 1808, ein Brief aus dem Jahre 1808, nach dem 18. Juli, und ein um die Jahreswende 1821/1822 geschriebener Brief) zu verschmerzen. Nach ihnen ist schon frühzeitig gesucht worden, aber sogar öffentliche Suchaktionen[12] sind ohne Erfolg geblieben. Dies mag seinen Grund darin haben, daß Bartels *seine Korrespondenz nicht aufzuheben pflegte,* wie der Astronom Otto Struve (1819 – 1905), der Bartels noch persönlich gekannt hat, bezeugte[13]. Jedenfalls waren auch jetzt wiederholte Versuche diese aufzuspuren[14], erfolglos.

Bedeutungsvoll ist in diesem Zusammenhang das Gauss nach 1808 bis 1821, also gerade in den für das Reifen seiner Gedanken über dir Grundlagen der Geometrie entscheidenten Jahren um 1815, keinen einzigen Brief an Bartels geschrieben hat, wie wir aus den Bartelsschen, an Gauss gerichteten Briefen ersehen.

Wenn aber Gauss in seinen Briefen an Bartels keine fachlichen Probleme angeschnitten hat, so muß es als sehr fraglich gelten, ob er in seiner bekannten *das Geschrei der Boeoter* scheuenden Zurückhaltung[15] sich jemals Bartels gegenüber im Gespräch zu seinen geometrischen Ansichten geäußert hat, zumal er ausdrücklich gesagt haben soll, allein der junge Wolfgang (Farkas) Bolyai (1775 – 1856), der Vater des genannten Johann, sei es gewesen, *der in seine metaphysischen Ansichten über Mathematik einzugehen verstanden habe*[16]. Natürlich ist das kein schlüssiger Beweis, daß Gauss nicht doch gelegentlich zu Bartels über die Grundlage einer nichteuklidischen Geometrie gesprochen hat, sollte das aber geschehen sein, so können wir nach Kenntnisnahme der Briefe von Bartels an Gauss sagen, daß die Gaussschen Ausführungen keinerlei nachhaltigen Eindruck auf Bartels gemacht haben. Andernfalls hätten die Lobacevskijschen Untersuchungen für Bartels gemacht haben. Andernfalls hätten die Lobacevskijschen Untersuchungen für Bartels einen willkommenen Anlaß zu Reminiszenzen zu frühere Unterhaltungen mit Gauss gegeben. Im Gegenteil: Nach einer Mitteilung von Otto Struve an Engel hat Bartels den wahren Wert der einschlägigen Lobacevskijschen Arbeiten verkannt, und Struve hat Bartels nie von anklingenden Gaussschen Ideen sprechen hören[17].

Nein, Bartels hat revolutionierende Ideen in der Mathematik weder hervorgebracht noch aufgegriffen und weitergegeben. So sollte den die Hypothese einer Beeinflussung (im Sinne einer Übermittlung von Überlegungen, Resultaten oder auch nur von bewusstem Anstoß) Lobacevskijs durch Gauss via Bartels endgültig zu den Akten gelegt werden, zumal Gauss selber nie die Selbstständigkeit Lobacevskijs (wie auch die des jüngeren Bolyai) ange-zweifelt hat![18]. Bartels war ausgezeichneter Pädagoge mit gründlichem und ausgebreitetem Wissen, seine wenigen Arbeiten zeichnen sich durch Gediegenheit und strenge aus und beweisen eine große mathematische Allgemeinbildung. Auch war er kein ausschließlich reproduktiver Mathematiker – erinnert sei an seine erst in der rezenten Vergangenheit hervorgehobenen bzw. aufgedeckten Prioritätsansprüche in der Theorie de Raumkurven[19] aber, und auch das lehren seiner Briefe an Gauss, er hat nie die Distanz zu letz-terem verkleinern können. Zunächst mag der Altersunterschied und das Leh-rer-Schüler-Verhältnis hierfür die Ursache gewesen sein, später hat an der Stelle dieser Schranke der geistige Abstand der der Annäherung Grenze setzte, wie sie nicht nur in dem Ton der Briefe, sondern auch in den Briefe, sondern auch in dem erheblich zeitlichen Zwischenraum zwischen den Briefen und im gänzlichen Versiegen der Korrespondenz 13 Jahre vor Bartels' Tod zum Ausdruck kommen.

Neben ihrer Bedeutung in der *Beeinflussungsfrage* sprechen für die Publikation der Bartelsschen Briefe an Gauss die aus ihnen zu gewinnenden Kenntnisse des Lebensablaufes von Bartels und deutsch-russischer Wissenschaftsbeziehungen; auch als Beitrag zur Biographie von Gauss sind sie schätzbar. Nachdem der Briefwechsel zwischen Gauss und Wolfgang Bolyai vor über 70 Jahren ediert worden ist[20], sollen daher nunmehr auch die Briefe von Bartels aus dem Gauss-Archiv[21] der Öffentlichkeit zugänglich.

Die ergiebigste Quelle für das Lebenslauf von Bartels ist seine Autobiographie[22], jedoch läßt sie manche interessierenden Fragen offen. Die Sekundärliteratur enthält zwar einige zusätzliche Fakten, andererseits aber auch eine Reihe von Irrtumern und Versehen[23]. Für das Verständnis der Biefe genügt es, hier die wichtigsten Lebensdaten zusammenzustellen. Weitere Einzelheiten sind den Briefen an Gauss bzw. den Anmerkungen zu entnehmen.

1769.8.12. Johann Martin Christian (Martin Fedorovic) Bartels wird in Braunschweig geboren.

1783 – 1788. Bartels ist als Helfer des Lehrers an der Katharinen-Volks-schule in Braunschweig tätig. Er studiert mit dem 1784 neu hinzugekommen, acht Jahre jüngeren Nachbarssohn Gauss mathematische Literatur.[24]

1788 – 1791. Besuch des Collegium Carolinum (Vorläufer der Technischen Hochschule) in Braunschweig

1791 – 1795. Studium an den Universitäten Helmstedt und (ab 25.10.1793) Göttingen.

Ende 1795 – 1798. Tätigkeit als Mathematiklehrer am Seminar im Schloss Reichenau bei Chur (Graubünden)[25].

1799. Vorübergehender Aufenthalt in der Heimat. Bartels wird am 18.7.1799 durch die Universität Jena in Abwesenheit promoviert[26].

1800 – 1804. Zweite Tätigkeit in der Schweiz als Mathematiklehrer in Aarau (Aargau), anfangs an der Realschule, dann an der neu gegründeten Kantonsschule.

1805 – 1807. Bartels hält sich gleichzeitig mit Gauss als herzoglicher Stipendiat in Braunschweig auf.

1808 – 1820. Ordentlicher Professor der Mathematik an der Universität in Kazan.

1821 – 1836. Ordentlicher Professor der Mathematik an der Universität Dorpat (Tartu).

1836.12.19. Bartels stirbt in Dorpat.

3

Für die Wiedergabe der Brieftexte gelten folgende Regeln: Die Schreibweise von Bartels wurde beibehalten, nur in die Zeichensetzung wurde stillschweigend dort eingegriffen, wo dies dem leichteren Verständnis dienlich schien. Ergänzungen wurden in [ ], von Engel (nicht immer ganz korrekt) zitierte Briefpassagen (a. a. o. pp. 353 – 354) in [[ ]] eingeschlossen. Nur wenige Losungen blieben fraglich; sie wurden kenntlich gemacht. Die jeweils erste Seite jedes Brieforiginals ist nicht als solche besonders ausgezeichnet, wohl aber jede folgende Briefseite durch d die betreffende Zahl in [ ].

Biographische Daten der in den Briefen genannten Personen gibt ein Personenverzeichnis im § 4.

Brief No. 1 [eigenhändige Anmerkung von Gauss: Martin Bartels, geboren in Braunschweig 1769 August 12, starb in Dorpat 1836. Decemb 19

Bremen, den 22. Sept. 1799

Sie haben mir, theuerster Freund, durch Ihre übersandten Disputationes[27] ein sehr angenehmes Geschenk gemacht. Ich sage Ihnen meinen herzlichen Dank dafür. Meine Arbeiten[28] haben mir dieser Tage noch nicht erlaubt, diese Schrift so, wie sie es verdient, sorgfältig durchstudieren, und ich habe mich damit begnügen müssen, sie vorerst nur flüchtig durchzulaufen. Allein auch diese flüchtige Durchsicht war hinlänglich, mich in der schon längst gehabten Üverzeugnung zu bestärken, daß durch Ihre Arbeiten das Feld der Mathematik nicht nur sehr wird erweitert werden, sondern auch das schon Bearbeitete die ihr fehlende Gründlichkeit erlangen wird. Dem Dr. Olbers habe ich das für ihn bestimmte Exemplar zugestellt[29]. Er läßt Ihnen seinen verbindlichsten Dank sagen und sich Ihrer Freundschaft empfehlen.

Es thut mir sehr leid, daß Sie die erwünschte Antwort von Zach noch nicht erhalten haben[30]. Leben Sie recht wohl und vergnügt uns lassen Sie mich Ihren gütigen Andenken empfohlen sein. Der Ihrige Bartels.

P. S. Sollten Sie mir mal wieder das Vergnügen machen zu schreiben, so haben Sie doch die Güte, die in Ihrer Abhandlung p. 12 in der Anmerk vorkommenden Seitenzahlen 441 – 474 von Eul[ers] Inst Calc Diff[31], Cap. VI, in die § Zahl zu verwandeln, weil ich nicht das Original, sondern nur die Übersetzung[32] besitze.

Brief No. 2. Aarau, den 10. Jun. 1804

Theuerster Freund, Mit Vergnügen bediene ich mich dieser Gelegenheit, mich bei Ihnen ins Andenken zurückzurufen. Wenn ich Ihnen während der langen Zeit meines hiesigen Aufenthaltes nicht schrieb, so dachte ich nichts destoweniger sehr oft an Sie und vernahm immer mit der innigsten Freude alle Sie betreffende Nachrichten. Auch Sie haben, wie ich mir schmeichle, sich zuweilen meiner erinnert und werden es nicht ungern sehen, durch mich selbst etwas von mir zu erfahren.

Während meines Hierseins habe ich manches angenehme und unangenehme erfahren. Im ganzen habe ich mich in meinen Erwartungen, so gemäßigt sie auch waren, getäuscht. Der beständige Wechsel der Dinge in der Schweiz[33] vereitelt oft plötzlich die schönsten Aussichten. Sinn für wissenschaftliche Kultur herrscht bei näherer Untersuchungen hier gar nicht. Alles hat eine merkantilische oder politische Tendenz. Dies zeigt sich auch bei unserer hiesigen Lehranstalt[34], an deren Entstehen und Gedeihen ich einen beträchtlichen Antheil habe. [2] Nach einer kurzen Existenz von etwa 3 Jahren hat sich der Geist, der in Anfang diese Anstalt zu beseelen schien, so ganz verändert das man sie kaum wieder erkennen wurde. Es würde Ihnen vielleicht nicht uninteressant sein, wenn ich ihnen umständlichere Berichte über diese Anstalt, die in ihren Folgen einen wichtigen Einfluss auf die Schweiz haben zu sollen schien, mitteilte; allein, theils wurde ich mich dies zu weit führen, theils beschäftige ich mich auch jetzt mit diesem Gegenstande nicht gern mehr. Oft bedaure ich die Zeit und Arbeit die ich einem Geschäft gewidmet habe, das leider ebenso fruchtlos fürs Ganze, als für mein Individuum ist. Meine müßige Zeit, deren es freilich einige gab, habe ich noch immer mathematischen Spekulationen gewidmet. Etwas davon habe ich Herrn Pfaff und Klügel mitgeteilt, was ich unter günstigeren Umständen wahrscheinlich sucht nicht wurde gethan haben, weil ich keinen andern Werth darauf lege, als den sie für mich hatten, mir einige angenehme Augenbicke zu verschaffen. Immerhin mögen diese Arbeiten, ausgeführt[35], einer Platz neben dem, was so *gewöhnlich* in Deutschland herauskommt, verdienen und konnten mir daher noch von einigem Nutzen sein. [3]

Der Besuch meiner Eltern, Briefe von Herrn v. Z[immermann], von Pfaff, Nachrichten von Ihnen etc., hat den Wunsch, in Braunschweig zu leben, aufs lebhafteste wieder bei mir erregt[36]. Doch dies nur unter uns gesagt. Ich wurde gern bei nur einiger Sicherheit für meinen Finanzzustand meinen Wohnort verändern, um, aufgemüntert durch Sie und meine übrigen mathem Freunde, die Trümmer meiner Kenntnisse zu sammeln und das Versäumte einigermaßen wieder nachzuhohlen. Konnten Sie dazu etwas beitragen, so werden Sie es gewiss thun[37], Das ich verheiratet bin, wissen Sie wahrscheinlich, und das ich in jeder Rücksicht sehr glücklich verheiratet bin, daran nehmen Sie gewiss herzlichen Antheil. Ich besitze ein liebes gutes Weib[38] ganz so, wie ich es mir wünschte, und bin Vater eines munteren Knaben[39].

Leben Sie wohl und vergnügt und erinnern sich zuweilen meiner; auch theilen Sie mir, wenn Sie mal einige müßige Augenblicke übrig haben sollten, Nachricht von sich mit. Sollte etwas meinem Wunsche entsprechendes in Braunsch vorfallen, so theilen Sie mir es gewiss gütigst mit. Von Hof-r[ath] Pfaff könnten Sie, wenn es nöthig wäre, vorerst vielleicht Auskunft[40] erhalten. Ihr ergebener Bartels

Brief No. 3[41]. Kasan, den 6. (18,) Jul 1808

Theurer lieber Freund, Mit einer Art bon Verlegenheit setze ich mich nieder, um Ihnen zu schreiben. Ich bin bereits über 4 Monate hier in Kasan und habe Ihnen auch noch nicht ein Lebenszeichen von mir gegeben. Das ist nicht recht und ich wage es nicht zu entschuldigen. Also stille davon! Billig sollte ich Ihnen einige Details von meiner Reise nach hier mittheilen, allein, wenn Sie auch diesmal nicht wieder ohne Nachricht von mir bleiben sollen, so muss ich mich damit begnügen, Ihnen ganz kurz zu erzählen, dass meine Reise [von] dem gezwungenen Aufenthalt von 4 Wochen in dem elenden Sammelplatz so vieler Unglücklicher[42], Memel [Kleipeda], wegen der Verspätung der Ankunft eines Passes und einigen von solcher Reise durchaus unzertrennlichen Beschwerden [abgesen], mir ungemein viel Genus gewährt hat. Meine Reise bis Petersburg gehört im Ganzen zu den angenehmsten Tagen meines Leben. In Petersburg blieb ich 7 Wochen. Das ich Ihren Brief[43] besorgte, wissen Sie schon längst. Fuss äusserte mir seine innigstes Bedauern über die Verteilung der Hoffnung, Sie für Russland zu gewinnen[44]. Kaum wollte er auch glauben, das keiner von seinen 3 Briefen an Sie angekommen sei[45]. Ob Sie sich in den Verhältnissen in St. P besser gefallen würden als in Göttingen, wage ich nicht zu entscheiden. Meine Stelle als Ehrenmitgl von K[asan][46] ist noch unbesetzt. Hat Ihnen der Curator[47] noch keine Anträge desfalls gemacht? Er war wenigsten, als ich ihn in Petersb[48] deshalb frug, von dein wohlthätigen Einflusse einer solchen Verbindung der Universität mit Ihnen überzeugt. Doch es giebt so Mancherlei der Art, was darum [doch] nicht immer ausgeführt wird, In Moskau traf ich Eiche [?][49]. Er ist verheiratet mit einer jungen Engländerin. Bei seiner Ankunft daselbst wurde er in einem merkantilischen Privatinstitute mit 1000 Rubel Gehalt und Wohnung mit Kost engagirt. Mißverständnisse zwischen ihm und dem Entrepreneur haben ihn bestimmt, eine eigne Anstalt zu etabliren. Inwiefern er reüssirt, kann ich nicht sagen. In Moskau kann es indeß einem geschickten und thätigen Mann in dieser Art nicht leicht fehlen. Gern wäre ich in Moskau länger geblieben, ich hielt mich ungefähr 7 Tage daselbst auf, wenn ich nicht, um von einer Reisegesellschaft, die ich unterwegs [getroffen hatte], bis Kasan zu profitiren, meine Abreise hätte beschleunigen müssen.

Ich eilte nach Kasan und kann, die ich schon über 4 Monate hier bin. Ihnen jetzt ziemlich bestimmt über meine Lage betreffend, Nachricht ertheilen. Über die Universität selbst und das daselbst angestellte Personal: wahrscheinlich haben Sie schon einen kurzen Abriß davon in der Jenaer Litt[eratur] Z[eitung][50] gelesen oder werden Ihn doch bald erhalten, der ganz treu ist. Ich für meine Person habe alle Ursache, zufrieden zu sein. Da ich außer meinen 2000 R noch frei Wohnung[51] (bestehend aus 7 herzb[aren] Zimmern, Küche, Remise, Stallung etc.) habe, so glaube ich, da alles hier ziemlich wohlfeil ist, ziemlich gut und in ökon[omischer] Rücksicht durchzukommen. Als ein Fragment eines Preisverzeichnisses teilt Ihnen meine Frau, die sich Ihnen und Ihrer Fr[au] Gem[ahlin] herzlich empfielt, folgende Notizen mit: 100 Eier für 70 Kop, 1 Pfund Rindfleisch im Herbst 3, jetzt 5 Kop, ein paar wilde Enten 50 Kop, 1 Huhn 25 – 30 K, 1 Bouteille Donscher Wein 40 bis 50 K, Semlanski [52], eine Art Champagner, 1 Rubel, guter franz W[ein] 1 Rubel, 40 [illegible. O. S.] Mehl, das feinste Walzmehl, 2 Rub 10 Kop etc. Alle Lebensmittel sollen vor einem Jahr um ein Drittel, ja die Hälfte wohlfeiler gewesen sein. Die meisten Professoren halten Pferde, 4, 2, auch 1, auch eine Menge Gesinde. Ich begnüge mich mit einen Bedienten, der deutsch und Russisch spricht, und meiner Lisabeth[54]. [[Mein Wirkungskreis ist hier angenehmer, als vorbereitet. Zwei derselben studieren Ihre *Disquisit*[*iones arithmetikae*]]][55] Einige wenige Instrumente, Sextant, Fernohre, sind hier, noch fehlt uns aber ein Locale [I did not find any suitable explnation of this term. O. S.]. Ein Observat soll gebaut werden, wann aber, weis ich nicht[56]. Rennert[57] muß jetzt in Petersburg sein. Noch ist sehr vieles hier zu thun. Alles hängt von Umständen ab, die ich hier nicht ganz zu detailliren wage[58]. So viel ich Ihnen auch zu sagen hätte, so muß ich, wenn nicht alle meine Briefe liegen bleiben sollen, dießmal schließen. Küssen Sie herz[lich] Ihre Fr Ge[mahlin][59]. Ihren l[iebin] Joseph[60]. Sollten Sie mir ein paar Zeilen schreiben wollen, was mich herz[lich] freuen wurde, so dürfen Sie dieselben immer Herrn Daubert, Schreibmeister, meinem Anwald[61] in Braunschw[62] überschicken.

Brief No. 4. Dorpat, den 14. (26.) April 1821

Theuerster Freund, [[Mehr als ein Jahrzehend ist verflossen, daß wir gegenseitig auch nicht eine Zeile voneinander gelesen haben. Einen Brief erhielt ich bald nach meiner Ankunft in Kasan]] als Antwort. Ein Brief, den ich einige Zeit nachher an Sie schrieb und mit

Gelegenheit überschickte, scheint wohl nicht angekommen zu sein[65], wie ich aus einigen Umständen zu vermuthen Ursache habe; doch dem sei, wie ihm solle, ich glaube, mir schmeicheln zu dürfen, daß es Ichnen nicht ganz unangenehm sein wird, wenn ich die so lange Zeit abgerissenen Enden unserer Korrespondenz wieder anknüpfe. Von Zeit zu Zeit erhielt ich von den Verhältnissen in Ihrer häuslichen Lage Nachrichten, die ich, so unvollständig sie auch waren, immer mit der innigsten Theilnahme aufnahm. Die Verhältnisse in Kasan waren freilich nicht so, daß sie nichts zu wünschen übrig ließen. Doch man hätte wohl zu einer Zeit, wo überall in Deutschland nichts als Noth war, wenn man nur einigermaßen bescheiden in seinen Wünschen war, in einem Winkel der Erde, der vor allen Stürmen so ganz gesichert zu sein schien, zufrieden sein sollen. Freilich wurden meine Erwartungen bald nach meiner Ankunft in Kasan gar sehr getäuscht. Die Hauptursache was das Sinken des Curses , wodurch 2000 Silberrubel hinab sanken, die jedoch bei der Wohlfeilheit der Lebensmittel bei einer vernünftigen Ökonomie ausreichten. Die Fehden zwischen den deutschen und russischen Professoren trugen eben auch nicht sehr zur Verannehmlichung des Aufenthalt bei. Vielleicht hat Ihnen Litrow [Littrow. O. S.] davon geschrieben[66], aber er trug bei seinen mündichen und schriftlichen Schilderungen immer etwas zu prall auf[67]. Hätte mancher deutsche Kollege meine Bemerkung, daß auf einem Boden wie dem Kasanischen keine südlichen Früchte gedeihen, beherzigt, so hätten sie sich manche vergebliche Anstrengung und Verdrießichkeit ersparen könnten. [2] Die Deutschen siegten jedoch wegen ihrer Mehrzahl ob; allein die Maschine wollte doch nicht so recht in den Gang. In Rücksicht auf meinen Wirkungskreis hatte ich wohl unter allen am meisten Ursache zufrieden zu sein, und nicht leicht werde ich irgendwo so viel Sinn fürs mathematische Studium finden als ich in Kasan vorfand. Die unter der Universität Kasan stehenden Gymnasien sind im allgemeinen mit sehr wackern mathematischen Lehrern versehen. Die Invasion der Franzosen in Rußland erregte zur Zeit des Aufenthalts derselben in Moskau bange Besorgnisse, besonders wegen der Umgebung von so vielen Nationen[68]. Doch diese gingen auch bald vorüber. Nicht lange darauf brannte das Haus, worin ich wohnte, das war ein Kronsgebäude, die Typographie, ab[69]. Glücklicherweise was es am Tage, so daß ich alle meine Sachen, einige Meubles ausgenomen, retten könnte. Ich kaufte mir darauf ein eignes Häuschen, das etwa ein Jahr nachher bei dem großen Brande, der mehr als die halbe Stadt verzehrte[70], ebenfalls beinahe ein Opfer der Flammen geworden wäre. Unsere kleine Straße, worin noch 3 deutsche Professoren [wohnten], wurde, ungeachtet es fast rundumher brannte, verschont. Von der jüngsten Veränderung an der Kasanischen Universität, die so ganz unerwartet kam und die wahrscheinlich eine Folge von letzte Ereignissen in Deutschland war[71], werden Sie vermuthlich wissen. Es wurden 9 Professoren, meistens Deutsche und einige Russen, entlassen[72]. Die Ursache davon ist nicht bekannt gemacht worden. Ich war nebst einer Mediziner[73] der einzige Deutsche, welcher seine Stelle behielt. Indeß bestimmten mich doch diese Verhältnisse, ungeachtet meiner sonst nicht ungünstigen ökonomischen Verhältnisse (ich hatte in den letzten Jahren etwa 5000 Rubel Einnahmen), ernstlich an die Heimkehr ins Vaterland zu denken, da mir nämlich durch den hochs[eligen] bei Belle Alliance gebliebenen Herzog[74] eine Professur am Carolino[75] mit meinem alten Gehalt zugesichert war und von der nachherigen provisorischen Vormunds-Regierung anerkannt war. Ein Ruf, den ich [3] im vorigen November hierher erhielt, änderte jedoch meinen Entschluß, besonders da bis jetzt keine eigentliche Vakanz am Carol[ino] besteht und ich die ungeheure Reise nach Braunschw ganz auf meine Kosten hätte unternehmen müssen. [[Ich reiste den 6. Dez von Kasan ab und kam hier den 7. Jan an.]] Allem Anschein nach habe ich mir zu dieser Veränderung sehr Glück zu wünschen. Meine Verhältnisse mit meinen Kollegen, mit dem Curator[75] und meinen Zuhörern sind so, wie sie nur wünschen kann. [[In Rücksicht letzterer dürfte ich wohl etwas mehr Sinn für mathematische Wissenschaften wünschen. In Kasan war ich ungeachtet der übrigen nicht ganz angenehmen Verhältnisse in dieser Hinsicht immer sehr glücklich.]] Da unsere Universität vor allen anderen Universitäten Rußlands vom Monarchen[77] ganz vorzüglich begünstigt wird, so sind hoffentlich wohl nicht ähnliche Vorfallenheiten wie in Kasan zu besorgen. Wegen Eduard [Bartels'son? O. S.] ist mir die hiesige Anstellung auch viel Wert, da er hier Gelegenheit zu benutzen versteht. Er sowohl als Hannchen[78] sind natürlich in der Zeit sehr herangewachsen. Doch jetzt genug von mir und meinen Verhältnissen. Äußerst angenehm würde es mir sein, wenn Sie mir auch von den Ihrigen eine kurze Nachricht mittheilen. Besonders interessiert mich als ein alter Bekannter Ihr lieber Joseph. Ihrer Frau Gemahlin, die ich nicht das Vergnügen habe [zu kennen][79], bitte ich mich und meine Frau, die sie herzlichst grüßt, gütigst zu empfehlen. Nun noch eine Bitte, deren Erfüllung Sie mir hoffentlich nicht versagen werden. Ich war in Kasan Vormund der beiden Kinder Arnold und Sophie[80] des daselbst verstorbener Professor Finke, der kurz nach Renner von Göttingen daselbst ankam. (Daß Ersterer, dessen Anstellung in Kasan Sie noch veranlaßt haben, schon vor mehreren Jahren gestorben ist, wissen Sie wohl.) Der Prof. Finke war der Sohn des Doctor Finke in Göttingen. [4] der, wenn er noch lebt, jetzt über 80 Jahre alt sein muß. Vor einigen Jahren erhielt ich einen ich einen Brief von ihm. Seit der Zeit aber hat man keine Nachricht von ihm erhalten; wahrscheinlich ist er unterdeß gestorben. Sollte dies der Fall sein, so ersuche ich Sie ergebenst, sich doch gefälligst nach dem Nachlasse des Verstorbenen zu erkündigen, denn, soviel ich weiß, war der Dr. Finke ein sehr wohlhabender Mann, auch erhellt dies aus eignen Briefen. Sollte er wirklich todt sein, so ist es mir unbegreiflich, daß von Seiten seiner Verwandten oder des Magistrats keine Nachricht davon nach Kasan kam, da doch allgemein dort [ist], daß der Professor Finke von da mit einer Frau und den beiden benannten Kindern nach Kasan abgereiset. Auch ist es seine mitgebrachte Frau starb, er sich dann wieder verheirathtete und vor ein paar Jahren selbst gestorben ist. Mit der letzten Frau hat er keine Kinder. Sie würden mich unendlich verpflichten, wenn Sie über obige Familienverhältnisse Auskunft geben könnten, damit die Kinder und die Witwe (die zwar jetzt wieder verheirathet ist), wenn sie, was sehr zu vermuthen ist, dort noch rechtliche Forderungen haben sollten, nicht etwas ganz um das Ihrige kämen. Meinen alten Freund Hofr Himly bitte.

Personalverzeichnis

Bartels, geb. Saluz, Anna Magdalena, 1784 – 1847, aus Chur, seit 1802 Ehefrau von M. Bartels, Briefe 2, 3, 4, 5

Bartels, Heinrich Elias Friedrich, 1743 – 1819, Zinngießermeister in Brauschweig, Vater von M. Bartels

Bartels, Eduard, 1803 – 1837, russischer Militärarzt, Sohn von M. Bartels 2, 4

Bartels, Johann Martin Christian (Martin Fedorovic), 1769 – 1836, Mathematiker, 1, 2, 3, 4, 5

Bartels , geb. Kühler, Johanna Christine Margarethe, 1741 – 1814, Mutter von M. Bartels, 2

Bartels, Johanna Henriette Franziska, 1807 – 1867, Tochter von M. Bartels, 1835, Ehefrau von Wilhelm Struve

Böhlrndorf, Hermann Leopold, 1773 – 1828, Theologe, seit 1814 Prof, in Dorpat, 5

Braunschweig, Friedrich Wilhelm, Herzog (1806), 1771 – 1815, Sohn des Förderers von Bartels und Gauss, Karl Wilhelm Ferdinand, 3

Daubert, Karl August, 1773 (?) – 1844, Schreib- und echenlehrer, später Bürgerschuldirektor in Braunschweig

Eiche (?), Bekannter von Bartels und Gauss in Moskau; dessen Ehefrau, 3

Euler, Leonhard, 1707 – 1783, Mathematiker, 1727/41 und 1766/83 an der Petersburger, 1741/66 an der Berliner Akad. Wiss., 1

Ewers, Gustav, 1781 – 1830, Geograph, Historiker und Statistiker, 1818 – 1830 Rektor der Univ. Dorpat, 4

Finke (Fincke), Johann Karl, 1775 – 1813, Jurist, 1798 Privatdozent in Göttingen, seit 1809 Prof.in Kasan, sowie dessen Familienangehörige, 4

Fuchs, Karl Theodore (Karl Fedorovic), 1776 – 1846, Mediziner, seit 1805 Prof. in Kasan

Fuß, Nikolaus (Nikolaj Ivanovic), 1755 – 1825, Mathematiker, beständige Sekretär der Petersburger Akad., Eulers letzter Assistent und Eheman von dessen Enkelin, stammte ebenso wie Euler aus der Schweiz, 3

Gauss, Carl Friedrich, 1777 – 1855, 1, 2, 3, 4, 5

Gauss, geb. Osthoff, Johanna, 1780 – 1809, erste Ehefrau von Gauss, 3

Gauss, Joseph, 1806 – 1873, Hannoverscher Offizier, später Eisenbahndirektor, ältester Sohn von C. F. Gauss, 3, 4, 5

Gauss, geb. Waldeck, Minna, 1778 – 1831, aus Göttingen, zweite Ehefrau von C. F. Gauss, 4, 5

Harding, Carl Ludwig, 1765 – 1834, Astronom (ursprüngl. Theologe), seit 1805 Prof. in Göttingen, 4, 5

Hellwig. Johann Christan Ludwig, 1743 – 1831, Mathematiker, Prof. am Gymnasium Catharineum, danach am Collegium Carollinum in Braunschweig, ehemaliger Lehrer von Gauss

Hezel (Hetzel), Johann Wilhelm Frierich, 1754 – 1824, Theologe, seit 1802 Prof. in Dorpat, 5

Himly , Karl Gustav, 1772 – 1837, Ophthalmologe, seit 1801 Prof.in Göttingen, 4, 5

Klügel, Georg Simon, 1730 – 1812, Mathematiker, Prof. in Helmstedt, seit 1788 in Halle, Verfasser eines math. Wörterbuchs, 2

Lieven, Carl, Graf, 1799; 1826: Fürst, 1767 – 1844; 1817 – 1828, Kurator des Univ. Dorpat, danach russischer Volksbildungsminister, 4, 5

Lieven Theodore, Graf, 1803 – 1866, Sohn des Kurators der Univ. Dorpat, 5

Littrow, Joseph Johann (Iosif Antonovic), 1781 – 1840, Astronom, 1810 – 1816 Prof. in Kasan, danach in Ofen, später in Wien, 4

Lobaccevskij, Nikolaj Ivanovic, 1792 – 1856, Mathematiker, Prof. in Kasan, Schüler von Bartels, 3

N. N. Lisabeth, aus der Shweiz, Hausgehilfen von Bartels in Kasan, 3

Olbers, Wilhelm, 1758 – 1840, Arzt und Astronom in Bremen, Freund von Gauss, 1

Pfaff Johann Friedrich, 1765- 1825, Mathematiker, seit 1788 Prof. in Helmstedt (1799 Promotor von Gauss, seit 1810 in Halle, Lehrer von Bartels in Helmstedt, 2

Renner, Kasper Friedrich (Kaspar Fedorovic), 1780 – 1816, Mathematiker, 1802 Privatozent in Göttingen, seit 1808 Prof. der angew. Mathematik in Kasan, 3, 4 [perhaps the first such chair worldwide.]

Rumovsij Stepan Jakovlevic, 1734 – 1812, Astronom, 1754/56 Schüler Eulers in Berlin, seit 1803 Kurator der Univ. Kasan, 3

Rußland, AlexanderI I, Zar (1801), 1777 – 1825, 4

Segelbach, Christian Friedrich, 1763 – 1842, Theologe, seit 1810 Prof. in Dorpat, 5

Stahl, Konrad Dietrich Martin, 1771 – 1833, Mathematiker und Physiker, 1799 – 1802 Prof. in Jena, danach in Coburg, Würzburg, Landshut bzw. in München, Fürsprecher für Bartels bei dessen Promotion in Jena 1799, 4

Stimker aus Braunschweig, Hauslehrer bei Graf Lieven, ehemaliger Mitschüler von Gauss, 5

Struve, Friedrich Georg Wilhelm (Vasilij Jakovlevic), 1793 – 1864, aus Altona, Astronom, seit 1817 Prof. in Dorpat, Organisator und 1829 erster Direktor des Observatoriums in Pulkova, in zweiter Ehe Schwiegersohn von Bartels, 5

Zach, Franz Xaver Frh. V,, 1754 – 1832 aus Praßburg (Bratislava), Astronom, 1787 – 1805 Direktor der Sternwarte auf dem Seeberg bei Gotha, 1

Zimmermann, Eberhard August Wilhelm v. (1796), 1743 – 1815, Physiker und geographischer Schriftsteller. Prof. am Collegium Carolinum in Braunschweig, einflüßreicher Förderer von Bartels und Gauss, 2

Anmerkungen

1. *Briefwechsel zwischen C- F. Gauss und Wolfgang Bolyai*. Hrsg. v. F. Schmidt und P. Stäckel. Leipzig, 1899, p. 94.
2. Ebenda.

3. Gerardy T. Nachträge zum Briefwechsel zwischen C.- F. Gauss und H. C. Schumacher. Göttingen, 1969, p. 111.

4. Sartorius v. Waltershausen W. *Gauss zum Gedächtnis*. Leipzig, 1856, p. 14. Auch C. G. J. Jacobi (1804 – 1851) hat übrigens den *vortrefflichen Bartels* geschätzt (*Briefwechsel zwischen C. G. J. Jacobi und M. H. Jacobi*. Hrsg. W. Ahrens. Leipzig, 1907, p. 35).

5. Depman I. Ja. M. F. Bartels – ucitel N. I. Lobacevskogo. In *Istoriko-matemat. issledovanija* 3 (1950), pp. 475 – 508.

6. Engel E. Lobacevskijs Leben und Schriften. In *Lobacevskij N. I. Zwei geometrische Abhandlungen*. Aus dem Russ. übersetzt, t. 2. Leipzig, 1899, pp. 349 – 456, insbes. pp. 378 – 381.

7. Ebenda, pp. 442 – 444.

8. Meschkovski H. *Mathematiker-Lexikon*. Mannheim und Zürich, 1968, p. 174.

9. Es ließen sich zahlreiche weitere Belege hierfür zitieren. Vgl. auch Engel a. a: O., pp. 428 – 429. Zu Bolyais Unabhängigkeit von Gauss siehe Engel a. a. O., pp. 382 und 429. Mit Bolyais Vater Wolfgang (Farkas) Bolyai (1775 – 1856), seinem Jugendfreund, hat Gauss bekanntlich über die Grundlagen der Geometrie diskutiert, aber Wolf-gang hat seinen Sohn direkt gewarnt, sich mit der Parallelentheorie zu befassen! Die Selbstständigkeit des Juristen und Theologen F. K. Schweikart (1780 – 1857) in der Entwicklung seiner *Astralgeometrie* in Charkov (1812 – 1816) ist nie angezweifelt worden. Erst 1818, in Marburg, stellte C. L. Gerling (1788 bis 1864) Schweikarts Verbin-dung zu Gauss her (vgl. hierzu Gauss, *Werke*, Bd. 8, pp. 179 – 182. *Briefwechsel zwischen C. F. Gauss und C. L. Gerling*. Hrsg. v. C. Schafer. Berlin, 1927, pp. 190 – 191, 194 – 196. 666 – 667, 670. Hierzu eine Ergänzung bei Gerardy T. C. L. Gerling an C. F. Gauss, 60 bisher unveröffentlichte Briefe. Göttingen, 1964, pp. 86 bis 87. Auch Schweikarts geglückter Ansatz und andererseits die Vielzahl mißlungener zeitgenössischer Publikationen zur *Theorie der Parallellinien* beweisen, daß das Problem einer vom Parallelaxiom unabhängigen Geometrie reif zur Lösung war. Einer Anregung durch Gauss bedurfte es nicht. Erinnert sei auch an die Arbeiten vor Gauss von Girolamo Sacceri (1667 – 1733) und von J. H. Lambert (1728 – 1777); vgl. Engel a. a. O., pp. 376 – 377).

10. Engel a. a. O., p. 428.

11. Hierauf hat Engel, der die Briefe von Bartels an Gauss benutzt hat, bereits hingewiesen (a. a. O., p. 353): Depman hat diese Feststellung übernommen (a. a. O., p. 478).

12. Depman a. a. O., p. 478.

13. Engel a. a. O., pp. 381 und 424.

14. Zu aufrichtigem Dank ist der Verfasser für Mithilfe bei dieser Sache verpflichtet: Herrn Prof. Dr. E. Amburger, Gießen; Herrn Dr. Th. Gerardy, Hannover; Herrn Prof. Dr. Dr. J. E.Hoffman (gestorben);Ichenhausen; Herrn Prof. Dr. H. Wondratschek, Karlsruhe; Herrn Prof. Dr. B. L. Laptev (Kasan); Herrn Prof. Dr. A. P. Juschkevic (gestorben); Herrn Prof. Dr. F. Klemm (München); Herrn OMR (? O. S.) Dr. Dr. H. v. Knorre, Altdöbern; Herrn Dr. G. Lumiste (Tartu), Herrn Prof. Dr. Maruhn, Gießen; Frau Dr. E. P. Ozhigova [gestorben], Herrn Dr. U. Lumiste unterstützte mich ferner liebenswürdigerweise durch Zusendung der Arbeiten von Erdmann und Rago, siehe Anm. 23.

15. *Briefwechsel zwischen Gauss und Bessel*. [hrsg. v. A. Auwers]. Leipzig, 1880, p. 190.

16. Sartorius a. a. O., p. 17.

17. Engel a. a. O., p. 381; Depman a. a. O., p. 478.

18. Engel a. a. O., p. 429.

19. Depman a. a. O., pp. 481 – 482. Ders. ebenda 5 (1952), p. 145: Lumiste Ju. Predvoschiscenie formul Frenet v socinenii K. E. Senffa, in *Voprosy istorii fiziko-mat. nauk*. Moskau, 1963, pp 141 – 147.

20. Siehe Anm. 1.

21. Niedersächsische Staats- und Univ. Bibliothek Göttingen. Die Xerokopien wurden freundlicherweise durch Herrn Dr. Th. Gerardy, Hannover, vermittelt, dem auch an dieser Stelle dafür gedankt sei.

22. *Vorlesung über mathematische Analyse mit Anwendungen auf Geometrie, Mechanik und Wahrscheinlichkeitslehre*, Bd. 1. Dorpat, 1833. Mit autobiographischen Ausführungen in der *Vorrede*. Nur in wenigen Exemplaren verbreitet. Das Werk wurde nach Bartels' Tod von seinem Schwiegersohn W. Struve durch die

nachgelassenen, wenig umfangreichen Vorarbeiten für den geplanten zweiter Band ergänzt und herausgegeben unter dem Titel *Vorlesung über mathematische Analysis.* Dorpat, 1837. Über weitere Arbeiten von Bartels siehe Depman a. a. O., 1950, pp. 481 – 483.

23. Neben der in den vorhergehenden und folgenden Anmerkungen zitierten Literatur wurden benutzt: Recke J. F. v. und K. E. Napiersky, *Allgemeines Schriftsteller- und Gelehrten Lexikon der Provinzen Livland, Esthland und Kurland*, Bd. 1. Mitau, 1827, pp. 73 – 74, sowie *Nachtrag*, Bd. 1. Mitau, 1859, p. 35. Erdmann J. F., J. M. Bartels, in *Das Inland* (1837), Nr. 50 und 51, Sp. 825 – 829, 841 – 845. *Sammlung von Briefen, gewechselt zwischen J. H. Pfaff und* [..] *Anderen*. Hrsg. v. C. Pfaff. Leipzig, 1853, pp. 29 und 93. Zschokke H. *Eine Selbstschau.* Ausg. 6. Aarau, 1859, pp. 92, 103, 112, 113, 262. Poggendorff J. C. *Biogr. literar. Hdwb zur Geschichte der exakten Wiss*. 1 (1863), Sp. 107: 3 1898), p. 73. Balic N. *Iz pervych let Kasanskogo univ. (1805 – 1819)*, Casti 1 und 2. Kasan 1887/91. *Russkij biogr. slovar* 2 (1900), p. 518. Alekseev, Bartels, in *Biogr. slovar* [..] *Derptskogo univ*. Hrsg. G. V. Levitskij, T. I. Jurev, 1902, pp. 163 – 167. Stieda W. Alt-Dorpat, Briefe aus den ersten Jahrzehnten der Hochschule. *In Sächs. Akad. Wiss.* Abh. der Phil.- Hist. Kl. 38 (1926), Nr. 2, insbes. pp. 108 – 115. Selle G. v. *Die Matrikel der Georg-August-Univ. zu Göttingen 1734 – 1837*. Hildesheim und Leipzig, 1937, Nr. 16814. Modzalevsky L. B. *Materialy dlja biografii N. I. Lobacevskogo*. Moskau und Leningrad 1948, inbes. p. 698. Kagan V. F. *N. I. Lobacevskij.* Ausg. 2. Moskau und Leningrad, 1948, insbes, pp. 39 – 43. *Lebensbilder aus dem Aarau 1803 – 1953.* Aarau, 1953. Vogel K. Bartels. In *Neue Deutsche Biogr*. 1 (1953), p. 598. Rago G. Iz zizni i dejatelnosti cetyrech zameca-telnych matematikov Tartusk. Univ. In *Uch. Zap. Tartusk. gos univ.* 37 (1955), insbes. pp. 74 – 81 und 102 – 103. Depman I. Ja. Uciteli Lobacevskogo. In *Leningradsk. Gos. Pedag. Inst. Uch. Zap.* 197 (1958), pp. 195 – 211. [Autorenkollektiv] *Ferdinand Minding*. Leningrad, 1970, p. 28. *Deutschbaltisches Biogr. Lexikon 1710 – 1960*. Köln und Wien, 1970, p. 30. Lumiste U. Recent Advances in the Study of the History of Math. in Estonia. In *Items from History of Science. in Estonian SSR*. Tartu, 1971, pp. 72 – 94, insbes. pp. 76 – 77.

24. Gauss *kam dadurch in den Besitz des Binomischen Lehrsatzes in voller Allgemeinheit und wurde bald mit der Lehre der unendlichen Reihen bekannt, welche ihm den Weg in die höhere Analysis eröffnete,* berichtete Sartorius (a. a. O., p. 13). Bartels' Pflichten als Helfer des Lehrers bestanden darin, *den kleineren Knaben die Federn zu schneiden und ihnen im Schreiben nachzuhelfen* (Sartorius nach mündliche Mitteilungen von Gauss, ebenda); es waren also recht bescheidene Aufgaben, die Bartels übertragen worden waren (er war 14 Jahre alt, als er mit dieser Tätigkeit begann).

25. Das Seminar Reichman stand in den Ruf *die Jugend den revo-lutionären Grundsätzen zu bilden,* dort seien in Graubünden zuerst *bei öffentlichen jugendlichen Prüfungen das Ca ira und andere franzö-sische Freiheitslieder* ertönt. Am *hitzigsten* seien die *Fremdlinge* (die ausländische Lehrer) *für die französische Freiheit und die Vereinigung mit der Schweiz* eingetreten, berichtete der österreichische Gesandte in Graubünden 1799 seiner Regierung (Rufer A. *Johann Peter Nesemann und seine Zeit*. Chur, 1963, pp. 37 – 38). Die Zitate verdanke ich der Freundlichkeit von Herrn Dr. Heinz Balmer, Konolfingen, Schweiz. Daher hat Bartels wohl absichtlich über die in der Schweiz verbrachten Jahre später mit spärliche Mitteilungen gemacht, woraus sich die lückenhaften und widersprüchlichen Überlieferungen erklären. Nach der Schließung des Seminars infolge der politischen Ereignisse hielt sich Bartels noch bis zum Frühjahr 1799 in der Nähe auf Schloß Haldenstein bei Freuden auf.

26. Die Mitteilung verdanke ich Herrn Prof. Dr. G. Uschmann, Jena, der auf meine Bitte im Univ. Archiv Jena, in den Akten der Phil. Fakultät nach dem Promotionsvorgang von Bartels von Bartels suchte und ihn dort fand: M No. 210, Bl. 96 – 104. Konrad Stahl (1771 – 1833), dessen Vermittlung sich Bartels bediente, hatte bei der Übergabe der Bartelschen (unveröffentlicht gebliebenen) Dissertation *Elementa Calculi Variationum* dem Dekan folgendes eröffnet: *Er* [Bartels] *sey in Graubünden Professor gewesen, aber von den Aufrührern vertrieben worden. Er habe Hoffnung, in Bremen angestellt zu werden und glaube, durch das gesuchte Diplom seinen Zweck geschwinder zu erhalten. Da ihm als einem Fremden unsere Gebräuche, z. B. daß er sich durch ein Schreiben an die Facul dazu melden müsse,*

unbekannt wären, so hatte Er ihn ihn [Stahl] *um die Besorgung gebeten, und zwar alles so geschwinde zu betreiben ersucht, als irgend möglich seyn wolle*. (Schriftsatz des Dekans vom 11.7.1799, a. a. O., Bl. 96; nach einer vom Univ. Arhiv freundlicherweise zur Verfügung gestellten Kleinbildaufnahme. Wie oben gesagt, wurde das Diplom bereits unter dem 18.7.1799 ausgefertigt. Ein Exemplar der Urkunde befindet bei der Akten a.a. O.

27. Gauss' Dissertation aufgrund deren er durch die Univ. Helmstedt promoviert worden war: *Demonstratio nova theorematis omnem functionem algebraicam rationalem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse*, Helmstedt 1799.

28. Es ist nicht bekannt, ob Bartels in Bremen eine Lehrtätigkeit ausgeübt hat, wie er beabsichtigte.

29. Bemerkenswert ist, daß der Briefwechsel zwischen Gauss und Olbers erst 1802 begann; die meines Wissens bisher nicht bekannte Übergabe der Gaussschen Schrift führte also noch nicht zu direkten Beziehungen zwischen den später so eng Befreundeten.

30. Gauss hat sich auch bei anderen Korrespondenten darüber beklagt, das Zachs zustimmende Antwort auf seine Auftrage, ob er sich bei ihm in der praktischen Astronomie üben dürfte, auf sich warten ließ. Vgl. z. B. Gerardy T. Der Briefwechsel zwischen C. F. Gauss und Carl Ludwig von Lecoq. In *Nachr. Akad. Wiss. Göttingen*, II. Math.-phys. Kl. (1959), Nr. 4, pp. 37 – 63, insbes. pp. 53 und 58. Die obigen Ausführungen von Bartels lassen darauf schließen, daß Gauss seine Dissertation mit einem Begleitbrief versehen hat, der als verloren gelten muß.

31. *Institutiones calculi differentialis cum ejus usu in analysi finitorum ac doctrina serierum*. Berlin, 1755. L. Euleri *Opera omnia* 1/10 = Eneström 212.

32. In die Zeit der Tätigkeit von Bartels in der Schweiz fielen französische Interventionen, die Proklamation der Helvetischen Republik mit zeitweiligen Sitz ihrer Zentralbehörden in Aarau, z. T. der zweite Koalitionskrieg sowie die innerschweizerischen Auseinandersetzungen zwischen Demokraten und Aristokraten, Unitariern und Föderalisten, Patrioten und Reaktionären, zwischen Anhängern Frankreichs und Österreichs.

33. In die Zeit der Tätigkeit von Bartels in der Schweiz fielen französische Interventionen, die Proklamation der Helvetischen Republik (mit zeitweiligen Sitz ihrer Zentralbehörden in Aarau), z. T. der zweite Koalitionskrieg sowie die innerschweizerischen Auseinandersetzungen zwischen Demokraten und Aristokraten, Unitariern und Federalisten, Patrioten und Reaktionären. Zwischen Anhängern Frankreich und Österreich.

34. Die Kantonschule Aarau war am 6.1.1802 gegründet worden. Zu den Auseinandersetzungen an und mit der Kantonschule vgl. Müller-Wolfer Th. *Die Aarganische Kantonschule in den vergangenen 150 Jahren*. Aarau, 1952, pp. 22 – 25; Bartels: pp. 26 und 28.

35. Es dürfte sich also nicht um druckreife Manuskripte gehandelt haben.

36. Bartel kehrte 1805 nach Brandenburg zurück, wo ihm, ebenso wie Gauss, ein Gehalt gewährt wurde. Er lehnte daher einen noch in Aarau erhaltenen und zunächst angenommenen Ruf nach Kasan vorerst ab. Vgl. Balic, a. a. O., 1, 1887, pp. 229 – 231.

37. Es ist noch nicht bekannt, inwieweit Gauss, mittelbar oder direkt, an der Bewilligung der Mittel für Bartels durch den Herzog von Braunschweig beteiligt gewesen ist.

38. Der Astronom C. A. F. Peters (1806 – 1880) urteilte über die ihm persönlich gut bekannte Anna Bartels: *Eine Dame von edelstem Charakter* (Gerardy a. a. O., 1969, p. 111).

39. Eduard Bartels.

40. Wohl in Sinne von Gutachten bzw. Befürwortung gemeint.

41. Mit eine Adresse Sr. Wohlgeb Herrn Dr. Gauss in Braunschweig.

42. Gemeint sind die vor den französischen Truppe Geflüchteten. Die Reise nach Memel [Kleipeda] begann am 18. Okt. in Braunschweig und ging über Helmstedt (Wiedersehen mit Pfaff), Magdeburg, Brandenburg, Berlin (Abreise am 26. Okt.) und Königsberg (*überall auf dem Wege nichts als Elend und Jammer*). Auf der Weiterfahrt von Memel nach Petersburg wurden Mitau, Riga und Dorpat [Tartu] berührt. In Petersburg hielt sich Bartels vom 22.12.1807 bis zum 6.2.1808 auf. Diese Einzelheiten entnehme ich einem hochinteressanten Brief, nach welchem übrigens

im Gegensatz zu der Mitteilung an Gauss der Aufenthalt in Memel [?] nur 14 Tage gedauert hat, von Bartels vom 23.12.1807 (beendet am 5.2.1808) an H. Zuschokke (1771 – 1848), dessen Xerokopie ich zusammen mit denen von 11 weiteren Briefen von Bartels an den gleichen Empfänger aus dem Nachlaß Zschokkes sowie den Kopien von einigen Bartels-Archivalien aus dem Nachlaß von F. X. Bronner (1758 – 1850) der Liebenswürdigkeit von Herrn Dr. Heinz Balmer, Konolfingen, Schweiz, zu verdanken habe. Die Originale alle dieser Dokumente befinden sich im Staatsarchiv in Aarau, Schweiz, welcher Institution ich für die Benutzungserlaubnis verpflichtet bin.

43. An N. Fuß vom 10.10.1807, *Trudy Inst. istorii nauki i techniki AN SSSR*, 1, 1934, vypusk 3, pp. 229 bis 230.

44. Gauss hatte den Ruf nach Göttingen angenommen, wo er am 21.11.1807 eingetroffen war.

45. Antworten auf Gauss' Anfrage vom 20.10.1806 (siehe Anm. 43, pp. 226 – 227). Indessen ist *eine* Antwort von Fuß richtig in die Hände von Gauss gekommen. In diesem Brief vom 11.12.1806 (siehe Anm. 21) wiederholt Fuß seine Bereitschaft, dem Wunsch von Gauss entsprechend, die erfördentlichen Schritte für eine Berufung durch die Petersburger Akademie erneut einzuleiten, nur bittet er, um nicht ein zweites Mal durch eine Ablehnung durch Gauss *compromittirt* zu werden, um die bindende Erklärung, daß es Gauss' *ernster Wille und unabänderlicher Entschluß* sei, den Ruf anzunehmen. Das Präsentatum lautet … May [?] 1 [1807].

46. Bartels war besoldetes Ehrenmitglied der Univ. Kasan geworden, als er auf Wunsch des Braunschweiger Herzogs den ersten Ruf dorthin abgelehnt hatte, und beriet von Braunschweig aus den Kurator S. Ja. Rumovskij. Zum Beispiel empfahl er letzterem Renner und Bronner; vgl. Bulic, a. a. O., 2, 1891, pp. 25 – 26 und 186.

47. Rumovskij, siehe die vorangegangene Anm.

48. Die Kuratoren hatten damals ihren Wohnsitz nicht unbedingt am Univ. Ort.

49. Es dürfte sich um einen gemeinsamen Bekannten von Bartels und Gauss handeln: Näheres ist nicht bekannt. Da sich Bartels in Petersburg nur von Leuten hatte beraten lassen, die im Inneren von Rußland noch nicht gereist waren, *riskierte* er auf der Weiterreise von Moskau nach Kasan *mehrere Male* sein und seiner Familie Leben. (Bartels an Zschokke, 22.1.1809; Quelle: siehe Anm. 42.

50. [M. Bartels]: Kasan, in Intelligenzblatt der *Jenaischen Allg. Literatur-Zeitung* 5 (1808), Nr. 48v. 20. Juli, Sp. 393 – 396. Dieser Artikel fehlt in der Bibliographie, die Depman a. a. O., 1950, pp. 480 – 481, wiedergibt. Aus dem Bericht geht hervor, daß von 28 ordentlichen Professoren erst 8, von 12 Adjunkturen nur 3 besetzt waren. Bartels nennt die Mitglieder des Lehrkörpers namentlich und fügt bei seinem eigenen Namen hinzu: *vorher Ehrenmitglied der Univ. Kasan mit 200 R*[ubeln] *Gehalt*. Es ist hervorhebenswert, daß Bartels zuvor nur eine einzige Arbeit publiziert hatte, und zwar eine Übersetzung der *Histoire de l'astronomie moderne* von J. S. Bailly's *Geschichte der neueren Astronomie.* Übersetzung: Leipzig, Bde. 1 – 2, 1796/97. Datierung der Vorrede: Reichenau 20.2.1796.

51. Nach Bartels' Angaben in der JALZ (siehe vorangegangene Anm.) konnten wahlweise 500 Rubel im Jahr oder frei Wohnung in Anspruch genommen werden.

52. Wohl Cymljanskischer Wein, nach der Stadt Cymljanskiy (Zymljanskiy) an der Mündung des Flusses Cymla in den Don (freundlicher Hinweis von Herrn Prof. Dr. A. P. Juskevic).

53. *Rocken* ist eine alte Schreibweise von Roggen. Zum Vergleich sind einige Preise interessant, die Bartels am 9.4.1809 Bronner mitteile (Quelle: Anm. 42) und die zusätzliche Informationen vermitteln bzw. die Verteuerung widerspiegeln: 1 Pfund Rind- oder Kalbfleisch 7 Kopeken; Liehte 22 K; Butter 30 K: 40 Pfund feinsten Mehl 3R 20K. ordinäres Mehl 65K: Zucker das Pfund 1R 80K; Kaffee 1R 60K; Rum 1 Bouteille 4R 50K; Wein 1R. Alles in Kupfer oder Assignaten. Bier und Quas braut man selber.

54. Die Gauss von Braunschweig her bekannte schweizerische Hausgehilfin der Familie Bartels.

55. Das erscheint um so bemerkenswerter, als in Deutschland damals noch kaum ein Mathematiker in Gauss' zahlentheoretisches Meisterwerk *Disquisitiones* … Leipzig 1801, tiefer eingedrungen war und kein Beispiel dafür bekannt ist, daß es zu dieser Zeit als Studienobjekt für Studenten benutzt worden wäre. So ist es verständlich, daß Gauss über die Mitteilung von Bartels höchst efreut war und die Nachricht an seine Freunde Bolyai und Olbers am 2. bzw. 14.9.1808 weiter gab. (*Briefwechsel*

zwischen Gauss und Bolyai, a. a. O., p. 94 , bzw. Olbers W., *Sein Leben und seine Werke*, hrsg. v. C. Schilling, Bd. 2, Abt. 1. Berlin, 1900, pp. 422 – 423. *Daß Studium* [der Mathematik] *wird noch unter allen am meisten kultivirt und ich bin so glücklich, da der größte Teil meiner Zuhörer aus Leuten besteht, die für dieß Fach Sinn und Talent haben, aber solche Gegenstände lehren zu dürfen, die ich durchaus auf keiner deutschen Univ. vortragen* dürfte. *Auf die Weise sind mir meine Berufsarbeiten nicht allein nicht lästig , sondern gewähren mir in meiner Lage den größten Genuß, da sie mir Gelegenheit geben, in meinen Kenntnissen fortzuschreiten*, heißt es in einem Brief von Bartels an Zschokke vom 23.7.1809 (Anm. 42). Was die beiden von Bartels Gauss gegenüber erwähnten Hörer anbetrifft, so dürfte der eine von ihnen Lobacevskij gewesen sein, da dieser seit dem Jan. 1807 Student war. Die Zweifel von Engel (a. a. O., p. 355) an dieser Annahme sind unbegründet, da sie auf einer Verwechslung von N. I. Lobacevskij mit dessen jüngerem Bruder Aleksej beruhen. Unentschieden muß bleiben, wer der zweite Hörer gewesen ist. Man ist geneigt, zunächst an den Astronomen Ivan Michajlovic Simonov (1794 – 1855) zu denken, da er und Lobacevskij sich 1811 unter Bartels' Anleitung eingehender mit den *Disqu. Arithm.* befaßt haben, aber Simonov hat sein Studium erst 1809 begonnen (vgl. B. V. Fedorenko, Gody ucenija Lobacevskogo i ego pervye geometriceskie issledovanija, in *Trudy instituta istorii estestvoznania i techniki AN SSSR* 17(1957), 163 – 228, insbes. 168 – 180. Ferner avtobiografija I. M. Simonova (1848). In *Istoriko- astron..issl.* 1 (1955) 268 – 280, insbes. 268. Sowie Biermann K. R. Einige Episoden aus den russischen Sprachstudien des Mathematikers Gauss. In *Forschungen und Fortschritte* 38 (1964) 44 – 46.

56. Auch in dem zitierten Bericht von Bartels im Intelligenzblatt der JAlZ heißt es: *Anatomie und Sternwarte sind noch nicht da*. Es wird das hier deshalb hervorgehoben, weil in der Literatur zur Geschichte des Observatoriums in Kasan auch anderslautende Angaben zu finden sind.

57. Irrtümlich statt Renner.

58. Wohl im Hinblick auf eine Briefzensur.

59. Gauss' erste Frau Johanna.

60. Gauss' ältester Sohn.

61. In Sinne von Beauftragter.

62. Vornamen und Lebensdaten von Daubert habe ich der Freundlichkeit von Herrn Dr. Querfurth in Braunschweig zu verdanken.

63. *Theoria motus* … Hamburg, 1809.

64. In Sinne von Bestellung, Auftrag.

65. Da sich der Brief nicht in Gauss' Nachlaß gefunden hat, dürfte er in der Tat auf dem Wege nach Göttingen verlorengegangen sein.

66. Littrov hatte 1818 Kasan, wo er seit 1810 tätig gewesen war, verlassen und war nach Olen (Buda) übergesiedelt.

67. Bei Engel (a. a. O., p. 426) findet sich ein Beleg hierfür.

68. Gemeint ist wohl, es hätte der Umstand, daß Napoleon seinen Krieg gegen Rußland mit fremden Hilfstruppen führte, den Unwillen der Bevölkerung Kasans gegen die dort aussässigen Ausländer hervorgerufen. F. X. Bronner schrieb aus Kasan am 16.12.1812 an N. Fuß: *Eine Art Erbitterung gegen alles, was nicht einheimisch ist, fängt an, auch da und dort zu äussern*. Naguevskij D. *Prof. F. K. Bronner, ego dnevnik i perepiska (1758 – 1850)*. Kasan, 1902, p. 393.

69. 25.4.1813. Vgl. Naguevskij, a. a. O., pp. 406 – 408.

70. 3.9.1815. Vgl. Bronner F. X. Der Brand von Kasan. In *Erheiterungen*. Hrsg. v. H. Zschokke 1 (1816), pp. 61 – 95, insbes. p. 83.

71. Bartels nimmt an, daß die von dem Kurator M. L. Magnitcki (1778 – 1844) bei der *Revision* der Univ. getroffenen Maßnahmen, von denen eine in folgenden durch ihn genannt wird, auf die Attentate gegen Kotzebue und Ibell bzw. auf die Karlsbader Beschlüsse zur Unterdrückung jeder fortschrittlichen Regung an den Universitäten zurückzuführen seien. Vgl. Kagan a. a. O., pp. 82 – 111. In die gleiche Richtung zielt die Bemerkung von A. Vucinich, Magnickijs *thought* sei *nourished by the Holy Alliance* (N. I. Lobachevskij – the man behind the first non-Euclidean geometry, in *Isis*, 53 (1962), 465 – 481. Zit. 474. Auch bei Vucinich findet sich übrigens die in dieser Form. wie oben ausgeführt – problematische Behauptung, *it is quite probable that Bartels was familiar with this special interest* [in a non-Euclidean geometry] *of his former pupil* [Gauss], *and that Lobachevskii heard it from him*. Es heißt dann weiter: *It is certain however that even though Lobachevskii might*

have heard about Gauss' interest in a non Euclidean geometry from Bartels, he was given no inkling as to how the whole problem could be treated mathematically (a. a. O., p. 471).

72. Die Namen der neun 1819 entlassenen Professoren gibt z. B. an: Korbut M. K. *Kasanskij gos. univ. za 125 let 1804/05 – 1929/30*, t. 1. Kasan, 1930, p. 19. Die Namen wurden hier nicht in das Personen-register aufgenommen.

73. K. Th. Fuchs.

74. Der Herzog Friedrich Wilhelm von Braunschweig, Sohn des Förderers von Gauss und Bartels, Karl Wilhelm Ferdinand, war in der Schlacht von Quatrebas am 16.6.1815 gefallen.

75. Collegium Carolinum in Braunschweig. Bartels hatte übrigens auch sein rückständiges Gehalt von 1806/07 nachgezahlt erhalten.

76. Carl Graf Lieven.

77. Alexander I. von Rußland.

78. Johanna, die Tochter von Bartels. Über sie vgl. Struve O. F. *V. Struve* (d. i. russ. Übers. der Schrift von Otto Struve, *Wilhelm Struve. Zur Erinnerung den Vater den Geschwistern dargebracht*. Karlsruhe, 1895. In Struve V. Ja. *Sbornik statej*. Moskau, 1964, pp. 75 – 116, insbes. p. 104.

79. Gauss' zweite Frau Minna, die er nach dem Tode seiner ersten Frau 11.10.1809 am 4.10.1810 geheiratet hatte.

80. Vgl. Bulic a. a. O., 2, 1891, pp. 64 – 65.

81. Vgl. Mack H. *C. F. Gauss und die Seinen*. Braunschweig, 1927, p. 10.

82. Bartels' ehemaliger Kommilitone in Helmstedt und Fürsprecher für seine Promotion in Jena.

83. Mit eine Adresse: Sr. Wohlgeb Herrn Dr. Gauss. Prof. der Math.; Direktor des Observatoriums, Mitglied mehrerer Gesellsch in Göttingen. Frei.

84. Im Sinne von Personen.

85. Graf Lieven.

86. G. Ewers.

87. H. L. Böhlendorf und C. F. Segelbach wurden 1823 wegen ihrer religios-rationalistischen Richtung auf Betreiben des pietistischen Kurators Graf Lieven vorzeitig emeritiert (freundlicher Hinweis von Herrn Obermedizinrat Dr. Dr. H. v. Knorre, Altdöbern).

89. *Disquisitiones quatuor ad theoriam functionum analyticarum pertinentes pro muneri in academia Caos*. Dorpatensi professoris matheseos publici ordinarii rite aduendo. Dorpat, 1822.

89. Eine Habilitationsschrift. Die Habilitierung mußte erfolgen, auch wenn der Betreffende zuvor bereits an anderen Univ. als Professor tätig gewesen war. Eine solche *Umhabilitierung* wurde auch *Nostrifizierung* genannt.

90. Bei Verwendung alphabetischer Bogensignaturen also maximal etwa 25 Druckbogen; vgl. hierzu den Brief von Bartels an N. Fuß vom 18.11.1822 bei Stieda, a. a. O., pp. 110 – 111.

91. Bartels hat also nicht zum Krise derer gehört, denen Gauss Separata seiner Veröffentlichungen zukommen líeß.

92. Gauss hatte offenbar in seinem Brief angefragt, ob Bartels geneigt sei, später einmal nach dem Tode ihres gemeinsamen greisen Bekannten und früheren Lehrers am Collegium Carolinum, Hellwig, wieder nach Braunschweig zurückzukehren.

Biermann was able to make a gigantic work, and hardly anywhere else so detailed information can be found. It is for this reason that I left so many details. But he translated many sources too formally and his style is inadmissible, so I corrected him quite often. It is too difficult to update his sources but readers can consult Sheynin O. (2017), *Theory of probability. Historical essay*. Berlin. **S, G,** 10.

V

Thor Andersson

Statistics and insurance

Nordic Stat J., vol. 1, 1929, pp. 235 – 240

Statistics is the basis of insurance. E. Phragmèn

The only solid basis for insurance is formed by statistics. V. E. Gamborg

The actuaries would be wrong if wholly confining themselves to mathematics. Firstly one must thoroughly know a territory before being able to use mathematics on it in a reasonable way[1]. E. Czuber

[1] The first publication within the science of statistics is Graunt (1662). For the first time an attempt, but indeed only an attempt is here made to deduce a life table from real observations. The defectiveness of the material as well as the insufficient knowledge of mathematics have caused that, nowadays, his attempt is of interest chiefly from a historical point of view, even if it was highly estimated by his contemporaries[2]. When bringing to memory that at that time for the publishing of Graunt's work the foundation of the science of probability was just being laid, it does not arouse any astonishment that Graunt, at his first statistic attempt had not sufficient mathematical experience at his disposal, The same lack of sufficient mathematical knowledge that distinguished the first statistician in the history of scientific statistics is found ever since in the vast majority of those whose names meet in the general history of statistics, The absence of necessary mathematical practice by the leaders and practitioners of practical statistics has led to the fact that more than 200 years elapsed before Lexis could issue the independence declaration of the statistical science[3]. The founder of population statistics of Sweden, Wargentin, has however as the first given a correctly calculated life table for the population of a whole country in his chief work published in 1766, containing life tables for men and women[4] founded upon censuses in Sweden as well as the death rates during the years 1755 – 1763.

Even when Graunt's work was published and the science of probability was being founded, several of the chief problems of modern insurance were in the centre of politics. The state finances often deranged by many wars were in need of good income. Attempts were made to procure money by offering annuities. For this and other insurance work death [mortality] tables were necessary upon which indispensable calculations could be built.

[2] The insurance mathematicians had to make these calculations in a scientifically satisfying way. Insurance mathematics had experienced many vicissitudes before being able to form the basis that was in several respects satisfying and upon which nowadays sound insurance rests. That this basis in spite of the extensive work still needs strengthening and widening is essentially owing to the fact that statistics was so long in gaining full independence as a science and in being able to vindicate its position as a first science[5]. Statistics cannot do without mathematics as it has acquired its independence by using probability calculations. The science of mathematics is the servant of statistics, not its mistress[6].

From the very start of scientific statistics the practice of insurance has exercised a great influence upon the development of statistical science. Especially the need of insurance for mortality statistics has produced a fertilizing effect on the development of general statistics. The mortality moment has however been thereby pushed in the foreground so that statistical investigations had for a long time almost quite neglected and still neglects in a too high degree the great problems of nativity in population statistics. The leading work of insurance mathematicians during a long time on several fields of mortality statistics effected that the mathematical part of their work emerged in the foreground and the large fundamental statistical portion was pushed into the background, wherefrom it has almost nowhere advanced to the place it ought to have, or the centre in the work of scientific insurance. Czuber, whose work is known worldwide, even within the field of insurance mathematics, has surely had a clear understanding of the importance of mathematics for the practice of insurance. This had however not hindered him from declaring that [see epigraph]. Statistics gives complete knowledge and for its compete use for insurance purposes mathematics comes into use[7].

[3] In large parts of the world the name of actuaries is nowadays given to the men who execute the scientific labour within insurance, upon which the soundness and success of insurance depend. In the *Enc. Brit*. [eleventh edition, vol. 1, 1910] one will find the following under *actuary*:

The name of actuaries sc. scriba [or scribe] *in ancient Rome was given to the clerks who recorded the Acta Publica of the senate and also to the officers who kept the military accounts and enforced the due fulfilment of contracts for military supplies. In its English form the word has undergone a gradual limitation of meaning. At first it seems to have denoted any clerk or registrar; the more particularly the secretary and adviser of any joint-stock company, but especially of an insurance company; and it is now applied specifically to any who makes these calculations as to the probabilities of human life, on which the practice of life insurance and the valuation of revisionary interests, deferred annuities, and so on, are based*.

In the first-mentioned meaning the title of actuary is still used also in England where for instance the staff at the province [diocese] of Canterbury consists of vicar general, registrar and actuary. In the old Prussian administration the title of actuary was used already at the beginning of modern times for a person who was something like a superior caretaker or porter and had to take care of the acta of the civil service departments. In the departments of the Nordic countries the title of actuary is still used for a person who exercises the care or received or sent acts. In some Swedish departments publishing the statistical reports for the entire kingdom the employees in middle position are also designed as actuaries although they have not, in most cases, as yet got any scientific education in statistics. In some leading private commercial enterprises actuary is also sometimes used as a title of an account-keeper who has been in the company for a rather long time.

[4] The most eminent scientist of Sweden today, the chairman of the Swedish Actuary Society and president of the organising committee for the next ninth international actuary congress[8], Professor Phragmèn, has declared that [see epigraph]. The most eminent late chairman of the Danish Actuary Society, Gamborg, has acceded to the same opinion [see epigraph].

Statistics is the chief thing for the insurance of the future and the basis of its scientific activity. Insurance mathematics is, as Phragmèn has said, not to be regarded as a scientific branch in the literal sense of the word, but as an applied science of rather inferior importance. The activity of the insurance scientist is now in its fundamental character of statistical nature, a fact which ought to be clearly expressed in his title. An insurance statistician should thus be the most adequate denomination. In Muret – Sanders English – German dictionary the English *actuary* is also translated by *Versichungsstatistiker*[9].

The title of insurance statistician now proposed for the first time (?) for the so-called actuary might not appear very attractive. Modern society has an innumerable lot of so-called statisticians but a very small number of them have the right to be so called in the modern statistical-scientific meaning of the word. The denomination of insurance statistician signifies that its just bearer is not only a worthy representative of the science of statistics but also of the appliance of this science in practical economic life and for high social-ethical purposes. Thus the insurance statistician is in no way to be compared with the superior caretaker in the old Prussian administration who was probably the first person entitled as an actuary in a country where this denomination has not been used for insurance scientists.

If the word statistics is used in the denomination of those who execute the scientific part of insurance, statistics should attract the attention of the whole insurance. The importance of statistics to insurance has not always been so highly estimated as it is now by the foremost Nordic insurance men, and the opinion as to its usefulness is invariably different in different insurance branches. There is even the so-called insurance Many of their practisers regard statistics as almost worthless.

The most eminent contemporary representatives and practisers of insurance, at least in the Nordic states, are not strangers to the fact that scientific statistics forms the basis of any practice of insurance and that it can assert just claims on the pure name of insurance, applies the soundest experience of practical economics and strives towards high social-ethical aims.

[5] Ever since it can be spoken of insurance in the modern sense, private insurance has been built on the basis of statistics. Life insurance, the oldest[10] and in consequence of its object and scientific practice, the first among the always increasing number of insurance branches, had striven for a scientific statistical basis before scientific statistics existed. Also in other branches of private insurance they have in recent years begun to understand the importance of good statistics.

Within the large branch of fire insurance as well as in those of accident, sick [morbidity] and liability insurance an ever more extensive work is being performed to procure a firm statistical ground at the same time as the science of statistics ever deeper penetrates all the dominion of insurance.

The sound practice of insurance is the [relates to the] foremost economic performance in a sound community. But even in the communities where this idea has been at least to some extent adopted, the state does not do much to protect and forward the sound practice of insurance. One of the numerous means at the disposal of the state for this purpose is that state statistics should pay regard to all the desires of insurance and try to meet them. It may be enough to remark that no country has as yet suitable fire insurance, no shipping statistics is being performed with due attention to the special demands of marine insurance, and what is still worse, the foremost work of state statistics, the census, is as yet going on all over the world without noteworthy endeavours to procure the data that could now be of an inestimable value. This can however been ascribed to the insurance itself, which still has not given the due place to statistics in the scientific insurance work. When this will be the case, Burn will not be right in saying that insurance mathematics is finished [completed], Even if it is at present in slack water, it will, just through the extension and progress of statistics be better prepared for significant work in the service of insurance statistics and insurance as a whole.

Information about a few persons

Burn Sir Joseph, 1871 – 1950, actuary. In 1926 – 1928 President of English Institute of Actuaries

Gamborg Villards Emanuel, 1866 – 1929, actuary

Phragmén Lars Edward, 1863 – 1937, mathematician, actuary

Notes

1. Gauss (W-12, pp. 201 – 204) in ca. 1841 stated the same.

2. Graunt had a sound statistical flair. Among those who praised him were Hyugens and Hald (Sheynin 2017, p. 43).

3. I missed that declaration, but anyway Cournot (1843, §§ 103, 106, 106) preceded him.

4. In 1826 Quetelet (Sheynin 1986, p. 294) compiled separate data for men and women living in Bruxelles.

5. Read: a fully fledged science. Now, Pearson (1892, p. 15) stated that

The unity of (a particular) *science consists alone in its method, not in its material*.

Two corollaries follow: statistics is an independent science (although lacking its own subject) since it is determined by its method. That method is mathematical statistics or rather theoretical statistics with a wider scope. And second, medical statistics is the application of the statistical method to medicine; the theory of errors is its application to the treatment of observations and measurements (and should be studied by statisticians) etc.

6, Chuprov (1922) effectively expressed the same thought.

7. This is an unconscious hint at the emerging econometrics.

8. Brief information about that Congress is in *Nature* (vol. 126, 1930, p. 76).

9. That dictionary was published in two volumes in 1869, as stated in Wikipedia, There also, it is called a German – English dictionary.

10. But O'Donnel (1936, p. 78) remarked that

Life insurance came to its own not by a front-door entrance but by marine insurance porthole.

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The author provides brief information about statistics in the Nordic countries and describes the situation of statistics over Europe (certainly omitting the USSR) in his day. Many references are lacking and much important material is left without justification. His English is extremely careless (I corrected it somewhat) and statistics was in plural.

VI

Anders Angström

Statistics and meteorology

Nordic Stat. J., vol. 1, 1929, pp. 228 – 234

[I am only copying the end of this paper since its beginning is of no interest whatever.]

[1] To illustrate the importance of statistics for meteorological problems I will give some actual cases which seem to me particularly instructive.

In quite a succession of years Dr. Abbot of the Smithsonian Institution of Washington has been occupied with an extremely extensive work to determine the so-called *solar constant* which is a measure of the intensity of the solar radiation at the outer border of the atmosphere. A direct determination of this quantity is not possible, and so it is necessary to deduce the solar constant from measurements of the radiation *within* the atmosphere and from certain fundamental presumptions concerning the absorption of radiation in the atmosphere. By his investigations Abbot was led to the opinion that the solar constant does not retain a constant value but is subjected to variations from day to day amounting to about 10%, and from year to year with an amplitude of some per cent. Under these circumstances it was not possible to determine the probable error[1] in the method from the basis of determinations made at a single station, and then the question presented itself: what part of these pretended fluctuations is to be attributed to occasional errors in the measurements and what part answers to a real variation in the solar constant[2]. At first, Abbot asserted the opinion that the incomparably greatest part of these observed variations consisted of real variations and pointed out that the simultaneous measurements at two of one another independent stations showed a very clear parallel course. Marvin and Kimball subjected, however, the material from two stations to a minute statistical investigation and stated that the amplitude of the real variations could not exceed at the average about a half of the pretended. Hence it follows that the phase difference between the pretended and the real variations is very considerable reaching such a size that the solar constant determinations at *one* station cannot very well serve as a basis for the study of the relation between solar activity and atmospheric phenomena on the whole. Only by means of statistical treatment of the material it was possible, in this case, to put the measurements and their bearing in their proper light.

A rather important meteorological problem is further the determination of periodic variations and their amplitude and phase as well as the probability of their real existence. Most meteorological phenomena, as for instance temperature, rainfall, water level are subjected to considerable variations of which the yearly and daily period is as a rule clearly suspicious and exactly determined. But do other periods exist apart from the mentioned, and if so, with what degree of exactitude can these periods be determined?

[2] The determination of periods was a popular sport, particularly in high favour with somewhat uncritical scientists before the development of statistical methods and especially before the application of these methods to meteorology and geophysics when there existed scarcely any problem in which they did not pretend to see one or more distinct periods. Statistical research in this field, whose development is perhaps due quite as much to the work of theoretically instructed pioneers in geophysics as to the achievement of purely statistical nature, have brought order and system into this field of investigation and built up a necessary objective ground.

Thus the eminent physicist and mathematician A. Schuster has worked out, for the study of periodic variations, a method which introduces a comparison between the amplitude of the presumed period, on one hand, and a theoretical amplitude, the *expectancy*[3] on the other which is calculated by the theory of probability under the assumption that the variations are of completely occasional nature. The relation between these two amplitudes gives a measure for the probability of the real existence of the first-mentioned period. Schuster is of the opinion that this relation ought to amount to 3 or 4 to justify the assumption that a given period is really existing. As an actual instance it can be mentioned that Dr. F. Bergsten has studied the variations in the water level at Lake Wäner[4] according to his method. He has found periods embracing 52/3, 71/3, 112/3, 16 and 26 years for which the relation of amplitude to expectancy has the respective values of 1.8, 2.5, 2.9, 2.8 and 1.9. Therefore, none of these periods can be regarded as altogether certain according to Schuster. The answer to the question concerning *certainty* and *not certainty* involves, however, a certain arbitrariness, and the principal thing to point out in particular is the fact that we, on the basis of statistical methods, are in position to neglect the question concerning existing and not existing and replace it with a question concerning the *degree of probability* which can be taken as a basis for an answer of at least some objectivity.

In a similar way the introduction of the notion of correlation founded on the theory of probabilities (!) has provided a more objective method for the determination of the probability of a relation between different phenomena and thus a method which opens ways to the discovery of new physical relations[5].

[3] As an illustration of the productive cooperation which takes place in meteorology between theory and hypothesis on the one hand and the statistical treatment of given observations on the other, still another problem of a more decidedly meteorological nature may be mentioned here.

The theory concerning cyclones worked out by V. and J. Bjerknes is founded, as is well known, on the assumption that the cyclones are to be regarded as a wave formation, at the boundary between cold air, often polar, and warm air, often of equatorial origin. This wave motion assumes, from a theoretical point of view, in many respects the same character as the waves which can be produced, for instance, at the boundary level between a superficial body of fresh water and the salt, and, therefore heavier, water below the body. From this hypothesis many important consequences can be deduced concerning the rate and direction of motion etc. of the cyclones. For instance, Bjerkness state under certain simplified presumptions, which might, as a rule, be practically permissible, that the rate and direction of motion etc. ought to be proportional to the square root of the difference in temperature between the cold and warm air. Recently Dr. E. Palmén in Helsingsfors [Helsinki] has achieved a very interesting statistical investigation concerning the rate and direction of motion of the cyclones and thereby got a confirmation of this law. It cannot be denied that this relation between the difference in temperature and the rate of motion can be deduced in other ways than by assuming the fundamental nature of the phenomenon to be a wave motion, but the essential thing to be emphasized here is the incitement of the theory leading to the statistical discovery of a definite law.

[4] It can sometimes be observed that, as this is also the case in other branches of statistics, there is a certain distrust in the application of statistical methods to meteorological problems. This distrust is partly justified, partly unjustified, the former insofar as it turns against the form of statistics which tries to hide the physical and real relations instead of revealing them[6]. Meteorological statistics which is not based on a familiarity with measuring methods and physical relations is evidently dangerous, in the first place on account of the fact that this familiarity is a condition for the possibility of estimating the extent to which the primary material satisfies the claims forming a basis for the application of statistical methods. This hostility has, on the other hand, often its source in the temperament of those scientists who see their pet ideas threatened by an objective examination and who, in conformity to most people of an intuitive character, are inclined for dogmatizing.

Notes

1. Bessel introduced the probable error in 1816. In spite of its known (but not generally) dependence on the law of distribution, it had been widely used until the mid-19th century but then became gradually forgotten. Its use by the author was dated.

2. Separation of the action of several causes became the aim of the analysis of variance.

3. Expectancy (usually expectation) in italics meant either that this most important notion was still little known or that the author was rather ignorant.

4. I only found Lake Warner (in the U. S. A.).

5. As regards the importance of statistics and particularly the theory of probabiities (!) for meteorological forecasts, I refer to my essay (1922). A. A.

6. This is wrong. In former times many statisticians did not comment on their results but gradually this practice disappeared.

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The author describes important investigations but leaves much material without justification. His style is bad and I edited his text.

VII

E. E. Slutsky

On the Existence of Connection between the Solar Constant and the Temperature

Zhurnal Geofiziki, vol. 3, 1933, pp. 263 – 281

Summary [in Slutsky's original English]

Abbreviation: CC = correlation coefficient
MT = max. temperature
SC = solar constant

1 – 3. The daily Montezuma values of the SC which have been used here, were obtained by the critical examination of the following data:

1) The values found by measuring the ordinates on the enlarged photocopy of the C. G. Abbot's diagram in *Smiths. Misc. Coll.*, Publ. 3114, p. 2 – 3, covering the period 1924 – 1930;

2) Ten day SC values for the same period (l. c., p. 12);

3) The daily values of the SC published in the *Daily Weather Map* of the United States Weather Bureau for the period from 24 July 1927 till 31 Oct. 1931.

The errors found by the comparison of our values with the *Annals of the Astro-Phys. Obs. of the Smiths. Inst.* (vol. 5), which came to us when this study was rather finished, are given in Table 1. Only on one case they are to be imputed to the misreading of the Abbot's diagram, in ten cases to errors in the *Daily Weather Map*, in the remaining 65 cases to the errors which are to be found in the Abbot's diagram re-published now without alteration in the Annals (vol. 5, p. 246). The mean frequency of the errors being less than 1:30 and their influence being found quite negligible in the one of the most doubtfull cases, it is to be hoped that the results of this study cannot be substantially vitiated by the said errors.

4. As we intended to prove the existence of the correlations between the SC and the MT found by H. H. Clayton, the deviations of the ten-daily means from the thirty-daily means of the SC and the MT for Cordoba (Argentina) have been computed. Then we have found the st. d. for every three-months period of the each year and the analogous st. d. based on the data for the whole period 1924 – 1931.

A glance on the Fig. 1 tells us that these st. d. are to be considered as periodic time-functions. Having calculated 3 (resp. 2) harmonics (see the full lines, Fig. 1), we reduced them by the due factors. The momentanious st. d. having thus been found, the original deviations were standardized by dividing them with the values proportional to these standard deviations.

5. From the series of the MT thus obtained we have chosen the partial series corresponding to the 56-th till 155-th and to the 156-th to

the 255-th day of each year and we have thus correlated them with the SC values 1) for the same year and 2) for the two preceding resp. the two following years with the additional lags from 0 to 15 days (see Tables 3a & 3b). The all 16 correlational functions thus obtained for the corresponding years and a specimen containing 4 functions for the different years are shown on the Fig. 2[1].

After the second partial series had been divided in the two equal parts, the same combinations of years have been considered and for each combination the largest from the CC corresponding to the additional lags from 0 to 15 days was found. They were found thus 8 + 8 + 16 CC between SC and MT values relating to the same year and 26 + 26 + 52 CC between the values relating to the different years, each CC being the largest (as to its absolute value) from the 16 CC corresponding to 16 additional lags from 0 to 15 days which were tried for each combination. These CC are shown on the Fig. 3. Thus, it is fairly evident that there is no signifiants difference between the CC found for the data relating to the same year and the CC found for the data relating to the different years, whence it follows that *the true CC between the values of the SC of radiation and the MT in Cordoba must be quite negligible the correlation c. which can be empiricaly found being nothing else but the errors of the random sampling.*

6. Table 4 gives the values of the momentanious st. d. of the deviations of the ten-daily from the 30-daily means of the SC calculated for the middle points of respective months. In discussing these values the author comes to the conclusion that during six months from the twelve the errors there involved constitute presumably the greater and the true value the lesser part of the values of the said deviations.

7. The distributions of the CC relating to cases when SC preceeds MT and to cases when MT preceeds SC (in both cases with the lags from 1 to 2 years) cannot be considered as significantly different, the value of chi-square being 16.84 and the corresponding probability P = 0.3. Combining both we find $\sigma_r = 0.2672$ instead of 0.1 given by the Pearson's formula (for $r = 0$, $n = 100$), this formula being inapplicable to connected series, i. e., to the series composed by the casual values which are not mutually independent.

Applying further the theory of the R. Fisher's function z for the connected series developed in our paper (*J. Geophysics*, vol. 2, No. 1(3)), we find $\sigma_z = 0.2873$ which leads to the theoretical distribution of the CC ($\chi^2 = 12.84$, $n' = 15$, $P = 0.5$). The values of z being thus normally distributed, it is possible to find, for instance, the probability of the deviation 0.65, this being the largest CC in the case of the correlation of values of the SC and MT relating to the same year. This probability being 0.007 the mathematical expectation of the number of such cases in the universe of 256 cases will be 1.8 the actual value, as a matter of fact, being only 1. The same theoretical distribution has been compared (see Table 6 and 7) with the distribution of the CC between the values of the SC and of the MT relating to the same year.

The distributions of the Table 6 being at the first sight significantly different, the author analyses the discrepancies and comes to the conclusion that there is probably no significant divergency, the disrepancies being enlarged by the correlation between the CC constituting the set of values under consideration. [See also the paper of the present author in the *Journal of Geophysic* vol. 2, No 1(3)]. This point of view is confirmed by the distribution of the absolute values of the CC (Table 7), and by the value of the standard deviation for the distribution of the Table 6 (0.250) being not substantially different from the value (0.267) of the st. d. of the CC for the case of different years.

8. There were found further 4·192 CC between the SC values with the lags equal, or nearly equal, to one and to two years and n = 40, 60, 80, 100. The empirical st. d. of these CC are shown in Table 8 where the last column gives the theoretical values according to the formula

$$\sigma_r^2 = \frac{A}{n} + \frac{B}{n^2} + \frac{C}{n^3},$$

A, *B*, *C* being found by the method of least squares. As we know (see the paper of the author cited above) the coefficient

$$A = 1 + 2\sum_1^{n-1} r^2(t)$$

whence it follows that

$$\sum_1^{n-1} r^2(t) = 0.5(A-1) = 4.14.$$

As it was found (see Table 9)

$$\sum_1^{31} r^2(t) = 3.45$$

it must be admitted that the values of $r(t)$ for $t > 31$ cannot be regarded as negligible. As it has been necessary to postpone the further study of the serial correlations, the theoretical value of the st. d. between the SC and the MT which (under the supposition of the zero-correlation) is given by

$$\sigma_r^2 = \frac{1}{n}[1 + 2\sum_1^{n-1} r_x(t) r_y(t)]$$

could not be evaluated. Nevertheless it is to be noted that the substitution of the sum

$$\sum_1^{31} r_x(t) r_y(t)$$

in the preceeding formula gives us the value of σ_r = 0.30 not substantially different from the value 0.27 found above by the direct computation based on 832 CC.

As it follows from the values of the serial correlations for the SC and for the MT given in the Table 9, there is a great similarity between the serial correlations for the periods 1924 – 1927 resp. 1928 – 1931, the relatively small differences being probably of the casual provenience. This fact cannot be underestimated and deserves further studies.

The main text

1. Introductory remarks. The subject of study

Abbreviation: see Summary

The solar constant is the amount of energy received [in 1 minute – not mentioned by Slutsky] from the sun by a surface perpendicular to the sun's rays, $1cm^2$ in area and located outside the earth's atmosphere at the earth's mean distance from the sun.

The remarkable investigations of Abbot and his collaborators (*Annals* 1932) have apparently definitively proved that this magnitude is actually not constant but fluctuates from year to year, from month to month, and perhaps even from day to day. Not so is it with the Clayton – Abbot (Abbot 1931, p. 1) theory of weather which maintains that exactly those alterations in the intensity of the solar emanation constitute the most essential cause of all meteorological changes which in their totality compose what is called weather.

The provided justification of that proposition does not seem convincing to us and we aim here to report about the work done for at least partly checking it. Clayton's study that went on year after year led him to conclude that each alteration in the SC produces changes of temperature, of the same sign in the equatorial and polar zones, and of the opposite sign in the temperate zones, and that first of all those alterations are reflected in the equatorial zone and in the high latitudes of the temperate zones. These perturbations move in waves towards the equator and shift eastward travelling at speeds inversely proportional to the length of their periods, and, in the tropical regions, are superimposed on the waves generated in the equatorial zone (Clayton 1923, pp. 215 – 269).

Clayton took into account a large number of stations, ensured a geographical coherence of the entire picture, and, last but not least, his separate, masterly selected illustrations are inspiring. At first, this creates an impression of reliable validity; only after having a closer look you begin to notice that the edifice of Clayton's constructions is not so robust.

First of all, it is necessary to remark that the number of stations indicating a correspondence between the course of meteorological processes and the changes in the SC cannot be especially significant. Since those processes are interconnected, such parallelism observed at one station will almost certainly be revealed in a number of other stations. It is much more important to cover the longest possible period and exactly in this respect Clayton's work leaves too much to be desired.

Clayton, to be truthful, determines CCs many times exceeding their mean square errors. Thus, at Sarmiento in Argentina after two days the CC between the SC and the temperature in winter of 1916 reached 0.82, see Clayton (1923, p. 224); on p. 269 he expressly mentions a small probable error. For 77 days of observation the CC eleven-fold exceeded its mean square error.

These data would have provided a reliable guarantee had he issued from series consisting of mutually independent terms. But, when this condition is lacking, as it always does when dealing with wavy series, the usual formula for the mean square error becomes absolutely unsuitable (Slutsky 1929; 1933) and its application can lead to most deplorable blunders.

Indeed, Clayton compares series mostly representing deviations of moving decade averages from similar monthly averages. Suppose that rhythms of about the same length occur in the series of temperatures as well as in that of the SC. That resemblance will be strengthened by averaging and it is not difficult to imagine that when the series are suitably shifted one with respect to the other intervals of 3, 4 and 5 wavelengths will quite often provide sufficiently high CCs.

That, however, is just what Clayton does when he calculates those coefficients after each shift up to 15 days. Shoot the flight of a crow in Moscow and of another one in New York. Measure the ascent of the wing on each film and calculate the CC. If your series are not too long, after a suitable shift [of one film relative to the other one] you will likely find a high coefficient, but does it mean that the flights of those two crows were causally connected?

And so, we decided to restrict our investigation by considering one station, but to take into account the entire period covered by the data on the SC, i. e., the eight years from 1924 to 1931. It was necessary to establish whether Clayton's results pertaining to the country which he especially studied and for which they, the results, occurred most striking were corroborated[2].

2. The Data

When beginning our work, the *Annals* (1932) had not yet appeared whereas (Abbot, no reference provided) it was already known that a large part of the previously published values of the SC should now be considered dated because the methods [of measurement] had been since improved and a number of new corrections introduced. We could therefore only base our study on the following sources.

1. The diagram of the daily values of the SC at mount Montezuma in Chile for 1924 – 1930 (Abbot 1931).

2. The Table of the mean decade and monthly values of the SC (Abbot 1931, p. 12).

3. The *Daily Map* (no date) containing the same data on Montezuma for the period from 24 July 1927 to the end of October 1931[3].

Here is how we proceeded. The ordinates on a photo of the Abbot diagram (22.5·17.5 *cm*) enlarged 2.5 times were measured twice and all the doubtful cases thoroughly considered. A number of values of the SC was thus established. Abbot distinguished satisfactory, almost satisfactory and unsatisfactory data by differing symbols (S, S– and U) and we were therefore able to determine decade and monthly means in which he neglected those of the last-mentioned type. A comparison of our means with his was satisfactory; namely, for all eight years the decade means of the CC were 0.990 with fluctuations in separate years from 0.977 to 0.994 and the monthly means for all that period, 0.9998. Deviation of the former from the latter, 0.963 with fluctuations in separate years from 0.946 to 0.986.

We compiled the series of values of the SC selected for the further work in three parts: from Jan. 1924 to 23 June 1927 (obtained from the Abbot diagram), from 24 June 1927 to 31 Dec. 1930 (the data corrected by critical comparison with the *Daily Map*), and for 1931 (*Daily Map*, the only source here). The *Unsatisfactory* data were neglected.

For comparing the SC with MT, we selected the data pertaining to Cordoba (Carta del Tiempo) in Argentina[4]. They only had a few essential gaps (40 days in succession from 1 Jan. 1929, and 15 days both in Dec. 1928 and Dec. 1931); other gaps were not longer than two days in succession (in the mean, missing was a little less than one day monthly) and we decided that it was permissible to fill those [shorter] gaps by linear interpolation.

3. Comparison of our series of the solar constant with Abbot's final data

Those final data (*Annals* 1932, Table 31, Montezuma 1920 – 1930, pp. 195 – 213) only became available after we concluded our work. We may certainly ignore the deviations concerning the *Unsatisfactory* cases, the rejection of those cases or the change from gaps to *Unsatisfactory* or vice versa as well as the change from *Satisfactory* to *Almost Satisfactory*.

There were 76 deviations left (Table 1), 10 of them (with symbol W attached) based on the *Daily Map*, one of those caused by an unfortunate reading of the Diagram (28 Oct. 1927). The rest 65 cases, as we ought to state regretfully, were mistakes of the Diagram itself, reprintted without change in the *Annals* (1932, p. 246). Concerning their influence on the results, the number of mistakes can be thought unimportant.

The worst case concerns Jan. – March 1925 (16 mistakes). Ten of the other mistakes, each amounting to not more than 1 or 2 units [of the last digit] were absolutely insignificant; 51 that had occurred during 81 month are left, 1 mistake per 48 days, and they certainly cannot discredit our conclusions.

As to the worst case mentioned above, we made the necessary calculations anew. For 100 days of the comparison of MT with the SC (from the 56th to the 155th day of the year) we obtained the highest in absolute value CC of 0.39 for a shift of 10 days instead of 0.40 for a shift of 11 – 12 days according to the previous calculation. Thus, even for the worst case, the error turned out to be absolutely inessential.

4. The treatment of the series

For the sake of convenience we adopted the following artificial calendar (Table 2) considering that each year had 365 days. That assumption would not have been possible to make for a longer period, but for eight years the inaccuracy thus introduced may apparently be neglected.

We bear in mind the study of periods lasting 100 days: from the 56th to the 155th day and from the 156th to the 256th day of the year. The latter approximately corresponds to the period for which Clayton had considered the connection between SC and MT in Argentina, and we indicate the appropriate calendar dates in Table 2. For the calculations below, months were thought to be 30 days long except for December (35 days), and an artificial trick explained below was introduced for ensuring intervals of equal duration.

Following Clayton, we had to study the correlation between the decade and monthly mean deviations, so we began by calculating the appropriate series; the means were taken with respect to the fifth and the fifteenth days of the appropriate moving time intervals. For the MT, because of the filling of the random gaps in the data (§ 2), the number of consecutive terms was always the same (10 and 30); for the SC, we calculated the arithmetic mean for the data at hand in those decade and monthly intervals; following Abbot, we did not exclude cases in which even only one observation was available. The units adopted were 0.001 *cal/cm*^{2} and 1°C and the means and the deviations were calculated to one decimal point.

The numbers in the first series were rounded to integral units; the same was done with those of the second series after multiplying them by 10/3. We then calculated the sums and the squares [of those numbers?] for the moving twelve three-monthly periods of each year (January – March etc.). The lacking data on the SC for Nov. and Dec. 1931 were filled up by the means calculated for the same months of the other years [of all other years?]; and, when calculating the sums for the first three months of 1924 and the last three months of 1931, we replaced Dec. 1923 by Dec. 1931 and Jan. 1932 by Jan. 1924. For each three months we denoted the square of the mean square deviation $\sigma^2_{3,ij}$ where i denoted the month, and j stood for the year.

Then, separately adding up the appropriate numbers of each month for all the years, we called the 12 numbers $\sigma^2_{3,i}$, $i = 1, 2, \ldots, 12$, which described the mean fluctuation of each three months for all the eight years. These numbers are shown on Fig. 1 by small circles, separately for SC and MT. There also, are the $\sigma^2_{3,ij}$ shown by points for each year.

Becoming thus convinced in the presence of a yearly course of fluctuations, we expanded each empirical function $\sigma^2_{3,i}$ in a Fourier series. It occurred that they can be satisfactorily represented by three (SC) or two (MT) first harmonics shown on Fig. 1 by continuous curves. Their parameters were (A_0 – arithmetic mean; A_i and B_i – coefficients of cosines and sines of harmonic i respectively):

SC: $A_0 = 11.191$, $A_1, A_2, A_3 = 2.015$, 0.947, 0.777
$B_1, B_2, B_3 = 0.153$, 3.031, 0.239
MT: $A_0 = 38.958$, $A_1, A_2 = -17.255$, 4.830
$B_1, B_2 = -2.800$, -0.136

For three-months periods the arithmetic means of SC and MT are very near to zero, and we will therefore insignificantly violate reality by replacing them below by expectations and by considering those latter equal to zero. And so, let there be m series of random variables

$x_{j1}, x_{j2}, \ldots, x_{jN}, j = 1, 2, \ldots, m,$

$$\mathrm{E}x_{jt} = 0,\ \mathrm{E}x_{jt}^2 = \sigma_t^2 = f(t).$$

Let

$$s_{2n}^2(t+1/2) = \frac{1}{2mn}\sum_{j=1}^{m}\sum_{k=-n+1}^{n} x_{j,t+k}^2$$

be the square of the mean square [literal translation] for the appropriate parts of all the series with centres at [t + (1/2)]. Then, obviously,

$$\mathrm{E}s_{2n}^2(t+1/2) = \frac{1}{2n}\sum_{k=-n+1}^{n} f(t+k) = F(t+1/2).$$

If $2mn$ is sufficiently large, then, according to the law of large numbers, the mean value will be approximately equal to its expectation. But in our case $2mn$ is indeed sufficiently large as can be supposed on the basis of the smooth course of the magnitudes $\sigma_{3,i}^2$ which, owing to their meaning, ought to coincide with $s_{2n}^2(t+1/2)$. Let us call $f(t) = \sigma_t^2$ the *instantaneous*, and $\sigma_{3,i}^2$, the *mean three-month variability*.

As proved above, we will have an approximate equality

$$\sigma_{3,i}^2 = \frac{1}{90}\sum_{k=-44}^{45} \sigma_{t+k}^2$$

where t is the fifteenth day of month i. Supposing that σ_t^2 is a sum of several sine curves, we recall a well known fact: $\sigma_{3,i}^2$ will then be equal to the sum of the same number of sine curves having the same periods and phases, but altered amplitudes. Knowing the coefficients of the harmonics for $\sigma_{3,i}^2$ and wishing to determine the coefficients of the harmonics comprising the instantaneous variability σ_t^2, it is only necessary to multiply them by

$$Q = \frac{2n\sin(h/2)}{\sin(hn)},$$

where, in our case, $2n = 90$, $h = 1, 2, 3$ for harmonics 1, 2 and 3 respectively, Note that its 12 ordinates, when expanding $\sigma_{3,i}^2$ into a Fourier series, were treated as being equally spaced in spite of the 35-day long December. This means that December was squeezed into 30 days so that at that stage of our work a year consisted of 360 days.

This is exactly why the abovementioned values of *h* were obtained.

Now, after calculating the coefficients of the expansion of σ_t^2, and shifting the origin of the system of coordinates from mid-January 15.5 days back, we multiplied the coefficients of the appropriate harmonics by 4 (for the SC) and divided them by 2.25 (for the MT). Here are their final values.

The solar constant
$a_0 = 44.764$, $a_1 = 8.445$, $a_2 = 4.710$, $a_3 = 4.819$
$b_1 = 3.048$, $b_2 = 19.387$, $b_3 = 9.704$
The maximal temperature
$a_0 = 17.315$, $a_1 = -7.839$, $a_2 = 2.939$
$b_1 = -3.608$, $b_2 = 1.655$

Now, calculating the appropriate sine curves for each day of the 360-day year, then increasing the days of December up to 35 by interpolation, we compiled a table of the values of $10/k\sigma_t$ with $k = 2$ and 2/3 for SC and MT. The deviations of the decade means from the monthly means (see above) were multiplied by those values and the results rounded off to integers. Thus we obtained final series of standardized deviations. The multipliers *k* were selected so that the absolute values of numbers in the final series will not exceed 21 or 22 which provided sufficient precision and essentially simplified further calculations.

5. Lack of correlated connection between the solar constant and the maximal temperature in Cordoba

That correlational connection was studied according to the following pattern. For MT, two intervals of 100 terms each were selected for each year, – from the 56th to the 155th and from the 156th to the 256th day, and two more of 50 terms each were obtained by dividing that second interval into halves. By comparing the SC with the MT of the day having the same number or a number less by 1, 2, … we were able to obtain CCs with differing "shifts". For the larger intervals CCs with shifts of 0, 1, 2, …, 15 were calculated, and for the shorter intervals, only the CCs maximal in absolute value among the same shifts. When determining these maxima, we were guided by the maximal values of the products, partly by superimposing graphs and we checked our work by calculating a few CCs around the supposed maxima.

As ascertained above, it was impossible to apply in our case the usual formula of the mean square error, but the use of the suitable theory encountered some difficulties (see below), so that we applied the following method. First, we calculated the CC between the values of SC and MT for the same year, i. e., by combining our series in pairs (1924, 1924), …, (1931, 1931). Second, we did the same for differing years, i. e. correlating MT of some year with the SC one or two years apart in either direction (Table 3) [call them combinations A and B].

The course of the CCs for combinations A and both large intervals is shown on Fig. 2. As an illustration, there also we show 4 correlation functions for the second interval and 4 combinations B. Our attention is at once arrested by the lack of any essential difference between combinations A and B. And it is also seen that even for the former combinations it is hardly possible to say that regularities are clearly discerned either in magnitude, sign or the shift corresponding to the maximal in absolute value CCs.

We now take a look at Fig. 3 where all the maximal in absolute value CCs are seen in a decreasing order; horizontal lines separate the larger and the lesser CCs and we clearly see that CCs of the same magnitude appear in both types of combinations and not rarer in the mean in group B. Thus, for the period between the 156th and the 255th day there are 8 [and 26] CCs in groups A and B; a half of those groups is not less than 0.49 and 0.36 respectively. However, we still ought to indicate that almost a quarter among group B reaches 0.49 whereas only 5 CCs from group A are higher than 0.39. It thus occurs that the difference only depends on one CC out of the eight which can well be a random occurrence.

Then, the insignificant superiority of group A in the series 156 – 255 is compensated by a superiority of B over A both in the interval 56 – 155 (the medians almost coincide, but considerably larger CCs are in group B) and in the shorter intervals (superior in both respects).

From all the above it follows that in Cordoba, if judging by the deviations of the decades from the monthly means, correlational connection between SC and MT either does not exist at all, or is quite insignificant and the comparatively high CCs are simply maximal values of random errors.

We will confirm this conclusion by another method (§ 7) whereas § 6 is devoted to a slight digression.

6. On the error of determining the solar constant

When calculating the instantaneous variability σ_t^2 for the middle of each month (see Table 4), we clearly see the magnitude of errors from which the determination of the SC was yet unable to get rid of. Represent the deviation of the mean decade from the mean monthly [values] x as the sum of the real deviation ξ and its error ε and denote the squares of their mean square deviations by σ^2, α^2 and β^2 respectively. For any two months we will have

$$\sigma_1^2 = \alpha^2 + \beta_1^2,\ \sigma_2^2 = \alpha^2 + \beta_2^2.$$

If $\sigma_1^2/\sigma_2^2 = p,$ then

$$\frac{\beta_2^2}{\alpha^2} = p - 1 + p\frac{\beta_1^2}{\alpha^2} \geq p - 1.$$

Comparing now all the months in Table 4 with November we find that for 6 months out of 12 $p \geq 2$. It follows that for these months not less than half of the magnitude of the deviations which we are studying are errors of observation. The deviations of the separate values from the monthly means are certainly corrupted by errors even more. It is hardly necessary to note that these conclusions, being a by-product of our work on which we cannot dwell anymore, should be specified by studying the probable errors of the numbers in Table 4.

7. The mean square error of the coefficient of correlation of the solar constant and maximal temperatures

When shifting the series of SC and MT with regard to each other by 1 or 2 years and some days, from 0 to 15, we obtained, as stated above, 832 CCs, each of them for the two series consisting of 100 terms. Separating them into two groups depending on whether the SC precedes MT (a) or vice versa (b), we obtain two distributions of the CCs (Table 5, columns a and b). For estimating the homogeneity /heterogeneity of those distributions, we can apply Pearson's formula; in our case it will be

$$\chi^2 = \sum \frac{(a_i - b_i)^2}{a_i + b_i}.$$

We obtain $\chi^2 = 16.84$; for $n' = 15$[5], we have $P = 0.3$ which shows a sufficient correspondence between those distributions. This circumstance confirms our assumption that in any case when the shift is 1 year or larger, the CCs between SC and MT vanishes, and the empirical CCs are nothing but "errors". Considering now both groups together (Table 5, column c), we calculate the mean square error of those CCs: $\sigma_r = 0.2672$. Had our series been lacking internal connections, such an error for ($r = 0$) would have taken place if the number of terms $n = 1/(0.2672)^2 = 14$. Or, the presence of such connections influences the square error and the number of terms is lessened from 100 to 14.

Supposing after Fisher that

$$z = \operatorname{arctanh} r = \frac{1}{2}\ln\frac{1+r}{1-r}$$

and, taking into account that in our case we may suppose that the real CC is zero, we find that $\sigma_z = 0.2873$[6]. Assuming that z is normally distributed, we calculate the theoretical numbers corresponding to the group in Table 5 (column m)[7]. If, as it is done after Pearson, the extreme groups having theoretical numbers less than 1 are combined with the neighbouring groups, we will have $n' = 15$

$$\chi^2 = \sum \frac{(m-c)^2}{m} = 12.84$$

and the probability $P = 0.5$ of a random deviation of the empirical distribution from the theoretical.

This fact is not devoid of interest since it again confirms my hypothesis formulated in the abovementioned contribution[8]. In addition, and it is here certainly more important, we become able to estimate the most considerable CCs which occur when comparing SC and MT for the same years. In Table 3 we see that out of 256 CCs of that group not a single one exceeds 0.65. And since

z = arctanh 0.65 = 0.7753,

which exceeds the calculated σ_z = 0.2873 only by a factor of 2.7, it means that not a single CC out of those 256 deviates from zero by three mean square errors. At the same time, according to the tables of the integral of probability, the theoretical number of deviations ≥ 2.7σ is 256·0.00693 = 1.78 > 1.

These considerations, as it seems, decidedly confirm the conclusion which we reached by following quite another approach, i. e., that there are no grounds for believing that the CC between the SC and MT in Cordoba appreciably differs from 0.

We will now check this conclusion in yet a different way by comparing the distribution of 256 CCs of group A with the theoretical obtained by studying the 832 CCs for pairs of different years (Table 6). It is not necessary to calculate χ^2 here: we see at once that it ought to be very considerable and the corresponding probability, very low. We ought to recall, however, that, as I had discovered in the quoted above paper, the χ^2 test is suitable, strictly speaking, only for totalities comprised of independent elements. It can be applied to totalities of dependent magnitudes[9], if at all, only tentatively since an entirely adequate criterion is yet lacking.

It seems that dependence has a stronger influence when the number of terms is comparatively small which is well illustrated by Tables 5 and 6. Indeed, a close look at the latter rather sharply brings home that the deviation between the empirical and theoretical distributions occurs owing to the essential accumulation of few cases in which the smoothness of the empirical distribution is grossly corrupted in a way that always takes place exactly in distributions of an insufficient number of elements.

In our case it is easy to explain this. Table 5 consists of 832 CCs, 52 groups of 16 terms each (shifts from 0 to 15 days) whereas only 16 such groups are in Table 6. At the same time the CCs in each separate group between certain series of the SC and MTs provide a series of 16 terms corresponding to shifts of 0 – 15 days closely correlated with each other; this is indeed revealed by the smooth wavy course of the relevant series (Fig. 2).

Therefore, if the maximal range of such a wave is about 0.55, say [?], and the wave forms a smooth stretched peak, a few consecutive CCs will at once be placed in the same cell. Two such waves are sufficient for 6 – 8 superfluous unities to occur, and they very considerably augment the value of the chi-square. This, for example, occurred the deviation between empirical and theoretical numbers in Table 6, third cell from above (15 and 7.7). This is easy to become convinced of when having a look at Tables 3a and 3b.

If these considerations are valid, an essential improvement will happen at once when the number of groups is decreased by combining symmetric categories, see Table 7. We get χ^2 = 9.61 and P = 0.2. In other words, not more probable deviations occur roughly once in five cases of independent elements. There are therefore no grounds for concluding that that distribution essentially differs from those indicated by the theory when independence is assumed.

Calculation of σ_r by issuing from data of Table 6 provides 0.250 which almost coincides with the case of different years. The conclusion is obvious.

8. Some preliminary results of analysing the series of the solar constant and maximal temperatures and derivation of the mean square error of the correlation coefficient

If SC and MT are really not correlated, the mean square error of the empirical CC should be represented by a comparatively simple formula

$$\sigma_r^2 = \frac{1}{n}\sum_{t=-n+1}^{n-1} \rho_x(t)\rho_y(t)$$

in which $\rho_x(t)$ and $\rho_y(t)$ are the true CCs between x_t and x_{i+t} and y_t and y_{i+t}. The difficulty in applying that formula consists in that, instead, we have to make do with the statistical CCs, $r_x(t)$ and $r_y(t)$; for more details, see my paper Slutssky (1932) quoted above. The errors of these CCs can essentially corrupt the results because a large number of terms are being added up. In that previous paper the problem was really solved, at least in principle, for the case of $\rho(t) = 0$, $t > \omega$ and not large values of ω as compared with n. An example of a more dif-ficult case is apparently encountered with the SC. We will assume an obviously highly probable hypothesis that the CCs between the values of SC separated by a year or more are either zero or negligible.

Comparing segments of the series of MT with numbers 156 – 255 taken either entirely (n = 100) or by parts with 40, 60 and 80 terms with the corresponding segments of the series of SC differing in time by one or two years in either direction and additionally shifted by 0 – 15 days we have calculated 112 CCs for shifts of about 1 year, and 80 CCs for shifts of about 2 years for each of the cases n = 40, 60, 80, 100. Table 8 contains empirical mean square errors of the CCs calculated accordingly and we note that for shifts of about 2 years all the σ's are somewhat smaller which perhaps argues for the presence of some remaining correlation (in any case, quite insignificant) at shifts of about 1 year. This can be checked by a similar study extended to shifts of 3 and 4 years. Anyway, the indicated differences can be neglected in the first approximation, and this is what we do.

Issuing from the known expansion

$$\sigma_r^2 = \frac{A}{n} + \frac{B}{n^2} + \frac{C}{n^3} + \ \ldots$$

and restricting it to three terms, we determine by least squares that

$A = 9.28$, $B = -164$, $C = 2190$.

The *theoretical* (i. e., the adjusted) values of σ_r^2 are shown in the last column of Table 8.

We consider the satisfactory adjustment as a testimonial that the number of terms allowed for in the formula above was sufficient and that, as I have shown in the paper quoted above, the value of *A* should therefore satisfy the approximate equality

$$\frac{A-1}{2} = \sum_{t=1}^{\omega} \rho_x^2(t). \qquad (*)$$

Replacing here ρ by empirical CCs *r*, we can determine an approx.-imate value of ω which is calculated by taking $\rho_x(t) = 0$ for $t > \omega$; if $\omega > (n - 1)$, it should be replaced by that difference.

The next table (Table 9) provides the values of the serial CCs for SC and MT with shifts of 1, 2, …, 31 days and for the 156th – 255th days of each year when correlated for shift *t* with the segment (156 – *t*; 255 – *t*). All these CCs were calculated for the first and the second half of the 8-year period, and for that period as a whole.

The following remark suggests itself first of all: the first and the second 4-year period both for SC and MT provide sufficiently close correlational functions at least when the CCs are still more or less considerable; the discrepancy between them can be certainly explai-ned by random errors[10]. A curious conclusion is that both SC and MT, after eliminating the 30-day level [?], and a suitable standardization of the fluctuations can be considered homogeneous, at least in the first approximation. If the future confirms and extends that inference to other geophysical series, it will be quite an important step in their statistical studies.

We have found the value of the coefficient *A*, $A = 9.28$. Therefore, the right side of (*) is equal to 4.14. We do not know the true CCs or values of ρ_x, but when replacing them by their approximate values r_x, the sums of the squares of the CCs calculated by means of Table 9 provide

$$\sum_{t=1}^{31} r_x^2(t) = 3.45$$

and it is obvious that, since the further CCs are doubtless small, a large number of them are needed for coming near to 4.14, so that ω should be considerably greater than 31.

However, bearing in mind that the squared sum of all the rest CCs in the series of SC from $t = 32$ to infinity is a magnitude of the order of 0.5 (approximately equal to the difference 4.14 – 3.45), we may hope that the sums of the products of serial CCs for the SC multiplied by the same CCs for the MTs can also be established although some-what roughly. Multiplying the appropriate values taken from Table 9, we find for $n = 100$ the approximate equality

$$\sigma_{r_{xy}} = \sqrt{\frac{1}{100}[1+2\sum_{t=1}^{31} r_x(t) r_y(t)]} = \sqrt{0.0897} = 0.30$$

which is very near to its empirically determined value 0.27.

In all probability, the further CCs (for shifts $t > 31$) are not important and, in addition, the error made by neglecting them was possibly compensated by dropping the term of order $1/n^2$. In any case, it is hardly accidental that the values of the mean square error of the CCs between SC and MT derived by such different methods are so close.

Explanation of tables and figures

Table 1. It lists the values of SC both adopted by Slutsky and either published in the *Annals* (1932) indicating categories *satisfactory* (S), *almost satisfactory* (S–) and *unsatisfactory* (U), or included with symbol W in the *Daily Map*, and the differences between them.

Table 2. Lists the month and day for the 1st, 56th, 155th, 255th and 365th day of an artificial calendar. Example: the 155th day of 1927 = 3 June 1927.

Fig. 1. Cordoba, SC and MT, separately. Shows by points their mean variability σ^2_{3ij} over three months (Jan. – March, Febr. – April, etc.) for 1924(1)1931. Their mean variability (the deviations of the decadic means from the monthly means) over those eight years σ^2_{3i} shown by small circles. Continuous curves show the sum of three or two harmonics for SC and MT respectively. Translation of legend partly tentative owing to difficult original text.

Table 3a. Lists CCs between SC and MT for period 56th – 155th day, years 1924(1)1931, shifts 0(1)15 days; separately shown are combinations of same year and of different years.

Table 3b. Same for period 156th – 255th day.

Fig. 2. CCs between SC and MT for same year (two upper series) and different years (the lower series), shifts 0(1)15 days. Additional curves shown with inadequate explanation moreover only given in text.

Fig. 3. Maximal in absolute values CCs between SC and MT for same year (A) and different years (B) for series of 100 and 50 days and shifts of 0(1)15 days.

Table 4. Lists magnitude σ^2_t for each of 12 months, year not indicated. Explanation lacking; explanation in text (§ 6) only states that SC is meant.

Table 5. Frequency table of CCs between SC and MT for different years, separately for SC preceding MT and vice versa and combined. Theoretical magnitudes additionally provided.

Table 6. Frequency table of CCs between SC and MT for same year, empirical (m') and theoretical (m) values.

Table 7. Same for absolute values of those CCs. Magnitude $[(m' - m)/m]^2$ additionally provided leading to $\chi^2 = 9.61$ and $P = 0.2$, see end of § 7.

Table 8. Lists empirical mean square errors of coefficients of serial correlation for SC, σ_r^2, shifts of about 1 year and about 2 years, and both these shifts combined, periods of 40, 60, 80 and 100 days. Theoretical values of σ_r^2 additionally provided.

Table 9. Lists coefficients of serial correlation for SC and MT, shifts of 1(1)31 day, periods 1924 – 1927, 1928 – 1931 and 1924 – 1931, interval 156th – 255th day.

Notes

1. In § 5 of the main (Russian) text, Slutsky wrote: *We show* [on Fig. 2] 4 *correlation functions* etc. Anyway, it is difficult to understand what exactly is shown there. In the context of this paper, *correlation function* means *values of the CCs*. O. S.

2. Abbot (*Annals* 1932, p. 277 and 255ff) has recently put forward a new concept concerning the connection between SC with the weather. He assumes that each periodic component of that constant is reflected in the phenomena of weather with differing shifts moreover variable in time. Separate waves are superimposed upon each other and the connection can be lost in the general picture. The material he adduced for proving this thesis is still too scanty for being convincing but it is extremely interesting, suggests ideas and for the time being compels us to abstain from a final judgement. A check of that new theory was not included in our aims. E. S.

3. Abbot (*Annals* 1932, Table 31, pp. 195 – 213) had since essentially corrected the values of the SC published there before the indicated date. E. S.

4. For Cordoba, Clayton derived one of his best results, CC = – 0.74. True, the CC was even higher for some stations in Argentina, – up to – 0.82 in Sarmiento, – but upon revealing that there were so many missing days we preferred Cordoba. E. S.

5. When being increased by 1, there will be 16 (groups) – 2(connections) + 1 = 15 degrees of freedom, as Fisher called it. E. S.

6. By applying the formula

$$\sigma_z = \sigma_r\sqrt{1+2\sigma_r^2+2\frac{1}{3}\sigma_r^4+4\sigma_r^6+\frac{1}{5}\sigma_r^8+24\frac{2}{3}\sigma_r^{10}+\ldots},$$

see Slutsky (1932, pp. 95 – 96). E. S.

7. I took the values of z corresponding to r = 0.5 [0.05?], 0.15, 0.25 etc. from Romanovsky's table (1928, p. 147). E. S.

8. Apparently, Slutsky (1929). O. S.

9. It was Fisher, who, in 1925, showed that the chi-squared test was not suited for studying dependent trials, see Hald (1998, p. 201). O. S.

10. We saw that for sufficiently large values of n and $t > 2\omega$ we may take $\sigma_r = \sqrt{9.28/n}$ for the CC between SC and MT. According to the above calculations, we have $\sigma_r = 0.267$ at $n = 100$ and we may therefore approximately assume that $\sigma_r = 0.13$ at $n = 400$. Although all the necessary formulas are available, we are not yet able to calculate σ_r for serial CCs at lesser shifts, but the indicated magnitudes probably provide sufficiently correct indications about their order. E. S.

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VII

E. E. Slutsky

On the Solar Constant

K voprosu o solnechnoi postoiannoi. *Zurnal Geofiziki*, vol. 4, 1934, pp. 392 – 399

Summary [in Slutsky's original English]

1. Serial correlations found by C. G. Abbot for the solar constant values showing discordant features for the different years scarcely can be considered as really significant owing to the relative paucity of the data constituting the separate yearly series. The formula of the probable error employed by the same author is unapplicable to the series of this art, the consecutive terms forming the series being not independent from each other. The serial correlations found by the same author for two groups of three years each (see Fig. 1) must also be discarded being biased by the method of their formation (the similarities and dissimilarities of the serial correlations for separate years being the ground of the unification or of the rejection of the data).

2. The serial correlations published in the present note are the correlational functions for the deviations of the ten-daily from the thirty-daily means of the solar constant of radiation standardized by the factors inversely proportionate to the momentaneous standard deviations (for more details see [xi]). The said deviations relating to 156 – 255 days of each year (1924 – 1931) were multiplied by the respective values t days before and the correlation coefficients were then formed 1) for the first four years (n = 400), 2) for the second four years (n = 400), and 3) for all eight years (n = 800). Each series contains the correlation coefficients from r_0 to r_{143} (see Table 2 and Fig. 2).

3. In analysing the results the method of the formation of the series under consideration must be accounted for.

Let x_1, x_2, … be some series of the mutually independent random values taken at random from the same general population. Then the deviations of the art used here [see formula (1) in the text] will be intercorrelated, the serial correlations being given by the formula (3) leading in our case to the values of the Table 2 (see also the little crosses line, Fig. 2). The values of this function for T > 30 being 0, the striking similarity between the correlational functions for two consecutive four-years groups must be therefore regarded as probably significant. The positions of the maxima and minima suggest the hypothesis that the approximate regularity observed therein may probably be occasioned by the revolution of the Sun. Whether there is a strong period in the solar constant values, or the cycles occasioned by the Sun's revolution are of the pseudo-periodical character we cannot say as yet. The problem evidently deserves further studies.

[The Main Russian Text]

Abbreviation: CC = correlation coefficient
SC = solar constant

1. Abbot (1922) published the results of his study of the serial correlational connection of the SC. He separately investigated each year from 1908 to 1916, only leaving out 1912 due to bad conditions for observations caused by the eruption of Katmai [a volcano in Alaska]. Multiplying the values of SC by their values 1, 2, 3, … days earlier, he thus determined the relevant CCs for r_1 to r_{40}.

It is not necessary to reproduce his graphs; Abbot himself, when commencing his study, remarked first of all that the appropriate curves were dissimilar. I will only provide the mean course of the CCs for two groups of three years each (Fig. 1). I selected the first three years (1908, 1911 and 1913) because of some similarity in those courses; I entirely rejected two years (1915 and 1916) owing to the sharp peculiarity of their correlational functions, and I combined the remaining years (1909, 1910 and 1914) into the second group. The reader will see that the two graphs indeed indicate quite different courses and in many features they are even contrary.

If periodic components are present, the correlational function must reveal the appropriate periods, and Abbot concludes that not a single clearly visible periodicity in the fluctuations of the CCs had been preserved over all the eight years of his study: *Each season is a law unto itself*. That conclusion, generally speaking, would not at all been unlikely, but the foundation that led Abbot to it ought to be questioned.

The main point is that he considers that the discord between the results for separate years was essential because the CCs calculated by him often exceeded their probable error many times over. However, he determined that error (as it is regrettably done very often) by means of a formula only suited when connections between the terms of a series are lacking. His reasoning therefore falls down and all of his other arguments are up in the air. Indeed, whether the discrepancies between the calculated results are significant or not; could they be occasioned by a random coincidence of circumstances or not, – judging that by the eye without any chances of checking yourself by a rigorous calculation is certainly impossible[1].

When considering the graph of the course of the SC we indeed convinced ourselves in that that magnitude can by no means be disconnected, be such whose consecutive values do not at all depend on each other in the stochastic sense. No calculations are even needed for reaching such conclusions since the wavy fluctuations in the course of the SC are seen too strikingly. These waves are very diverse. Some are short, lasting a few days, others cover months and there also are waves, that is, regular sinking and rises, going on for years on end.

Under such conditions, if the studied series does not last a large number of years, the determination of the serial CCs for the SC seems to be rather helpless. For coherent series, the number of observations is only enough if they cover sufficiently many waves. When there are very few longest waves, they have to be treated individually rather than statistically, to be separated as a secular component by some statistical method. True, none of these latter can be considered quite satisfactory for an objective analysis; it is much more rightful to see them as practical tricks for arbitrarily treating a given numerical series and providing *preparations* rather than its real components.

When mentioning *preparations*, I conscientiously wish to recall biological analogies, for example microscopic sections treated by various chemicals. Such preparations are not real but artificially created parts of the studied organism. And what we discern then represents corrupted pictures of reality. Nevertheless, they are known to be useful provided we are familiar with the properties of the operations made in the process and precisely understand the essence of the inserted corruptions.

With regard to the statistical methods of making preparations or at least to some of them, we possess such knowledge. Series can be treated in a way that neither the periods, nor the phases of harmonic functions, into which it can be expanded, will change, only the amplitudes will be corrupted which can be easily taken into account. Reducing long-period waves to insignificant amplitudes we obtain series sufficiently long compared to the essentially important for them shorter fluctuations so that hopefully they can be successfully treated.

2. Clayton applied one of such preparations (deviations of the decadic means from the monthly means) when studying the connection of SC with temperature. In the paper indicated above[1] I thoroughly analysed his conclusions by examining one of his examples (Cordoba, in Argentina). I naturally had to apply his methods of smoothing series and thus obtained as a preparation from a number of values of the SC the deviations of the decade means from the monthly means. Since my main aim was to study the connection between SC and temperature, some issues concerning the SC remained unascertained.

In particular, I only determined the serial correlation coefficient for shifts of up to 31 day although it seems almost unquestionable that the connection does not vanish there. Naturally I wished to fill that gap. Concerning the SC we now have CCs for shifts from 1 to 143 days (Tables 1 and 2). Owing to lack of time they were only calculated for 800 days rather than for the whole material at hand, namely only for 100 days (from the 156th to the 256th day) of each of the 8 years 1924 – 1931 for which we had the data on SC.

The CCs were calculated separately for both 4-year periods and for the 8-year period as a whole. A glance at the diagram (Fig. 2) is sufficient exactly now, when we have series of the SC from r_1 to r_{143}, for becoming convinced in the reality of connection. The following reasoning will show why it was by no means possible to be satisfied by the previous data, i. e., by series ending with the shift of 31 days.

We have to do not with the SC itself, but with a preparation. So how was it constructed? Denote the values of SC by x_1, x_2, …, then the numbers in our series will be represented by the formula (1):

$$y_{i+15} = \frac{1}{10}(x_{i+11} + x_{i+12} + ... + x_{i+20}) - \frac{1}{30}(x_{i+1} + x_{i+2} + ... + x_{i+30}),$$

$$y_{i+15} = -\left[\frac{1}{30}(x_{i+1} + ... + x_{i+10}) + 2(x_{i+11} + ... + x_{i+20}) + (x_{i+21} + ... + x_{i+30})\right]$$
.

Suppose that the values of SC, x_1, x_2, …, are random numbers not connected with each other. We know that, when forming moving sums from the terms of such a series according to the formula

$$y_i = \sum_{k=1}^{s} A_k x_{i-k}, \qquad (2)$$

the CCs will be

$$r_t = \frac{\sum_{k=1}^{s-t} A_k A_{k+t}}{\sum_{k=1}^{s-t} A_k^2}. \qquad (3)$$

Above, see formula (1), we have provided the values of A_k for our case and now we calculate the CCs by formula (3), see Table 2; on Fig. 2 they are shown by crosses. Generally speaking, they are so close to the actually obtained CCs that, until we restricted our investigation to series up to r_{31}, we could have apparently considered the obtained picture to an essential degree as a sole result of smoothing and would have thus *completely* explained the coincidence of the series of the CCs for the first and the second 4-year period.

It turns out, however, that the issue is not at all as simple as that. Suppose that all the coefficients of the serial correlation are zeros, then, up to n_{30} their course will be such as shown on Fig. 2 and vanish after r_{30} [?]. However, it would be absolutely incomprehensible why the further courses of our series for both 4-year periods will then be so similar. In both series we have

minima at shifts 11, 40 – 41, 68, 83 – 85 [rather 93 – 95],
and 115 – 116
maxima at shifts 30 – 31, 49 – 54, 83 – 85, 103 – 105
and 124 – 125

Consider that the maxima for the curve describing the entire 8-year period are t = 31, then 226, 3272/3, 4261/4, 5244/5, and allow for the possible influence of random errors. Then the hypothesis that the course of the correlation function reflects the rotation of the Sun about its axis becomes very likely since the figures above are close to the synodic period [close to those that would follow from the synodic period] of that rotation.

This would have provided a material cause for the presence of the main wave revealed above in the correlation function. Deviations, if becoming real after analysing more complete materials, could have possibly be explained as the result of interference with other periodic or pseudo-periodic components.

We also note that one of the latest contributions of Abbot (Abbot & Bond, Publ. 3172), even if not yet proving that strictly periodic components of SC do exist, had at least made their existence highly likely. Most convincing seems to be the agreement between the phases of the waves of different parts of the series and their concord with the phases of the wave established for the series as a whole, see waves C_1, C_2 and C_3 with period of 8 months and waves D_1, D_2 and D_3 with period 11 months on Fig. 3 of their p. 5. This issue certainly deserves further study.

Explanation of Tables and Figures

Fig. 1. The mean course of the CCs along the series of SC for shifts of 1(1)40 days for two groups of three years each (Abbot).

Table 1. Lists CCs in a smoothened series of SC. The coefficients are shown for t = 1(1)143 separately for 1924 – 1927, 1928 – 1931 and for the period 1924 – 1932 as a whole.

Fig. 2. Correlation function for SC separately for 1924 – 1927, 1928 – 1931 and for 1924 – 1931 as a whole. Crosses indicate the course of that function for a smoothened disconnected series.

Table 2. Lists the values of the CCs (of 60r) for a disconnected series smoothened according to formula (1) and t = 0(1)30. The text makes it clear that this table deals with SC.

Note

1. Curves shown on Fig. 1 are also unconvincing since Abbot combined the years in a group not consecutively, but according to similarity/distinction of the correlation function. He thus introduced an element of selection that entirely compensated the increase in the number of observations and utterly corrupted the independence of the series. E. S.

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See also Bibliography to previous contribution of Slutsky

See Slutsky’s biography in his *Collected Statistical Papers* translated by me. Berlin, 2010. **S, G,** 40.

Explanation in his previous paper is insufficient and in many cases he calculated with an excessive number of significant digits, Strangely enough, in both papers he used the probable error.

IX

Gerhard Tintner

Statistical work of Oskar Anderson

:

J. Amer. Stat. Assoc., vol. 56, 1961, pp. 273 – 280

The death of Professor Oskar Anderson in Munich on February 12, 1960, in his 73rd year is a great loss for statisticians everywhere. He was born on August 2, 1887 in Minsk, Russia. He studied mathematics, physics, economics and law at the Universities of Kazan and Petersburg [mistake, see other obituaries]. He was an assistant of the well known Russian statistician Chuprov. […]

We propose to discuss the works of Anderson under seven heads […].This list (?) includes an account of Anderson's teacher Chuprov and of the work of L. von Bortkiewicz which is remarkable for the treatment of the contributions of this outstanding scholar to statistics and economics.

2. Probability [there was no 1]

The point of view taken by Anderson about the interpretation of probability is most original and worth noting. The origin of his idea can of course be traced to the Russian school of probability. Very remarkable is the definition of a social-statistical probability [1957, p. 100]:

Probability of an attribute or a characteristic of a social statistical population is the frequency in a population of higher order, out of which the present population has been taken. It is necessary to be precise about the way in which the given population has been derived from the higher population, as much as practical applications of probability theory are concerned. The population of higher order can be finite or infinite …

In statistical inference the point of view of Anderson is somewhat different from the accepted statistical textbooks. He bases himself essentially upon certain ideas of the great French mathematician [wrong] and economist Cournot in connection with the law of large numbers: The connection between the purely mathematical theorems, like the law of large numbers and practical statistical applications is established by the *Cournot bridge*. This consists of three parts:

1. Events whose probability is very small, happen very infrequently. This is a purely empirical proposition.

2. Consider the deviation of relative frequency from the corresponding probability, and in general the probability of deviations of certain characteristics of the sample (e. g., the arithmetic mean) from the corresponding expectation values. The probability that such a deviation will be greater than a given magnitude, which is fixed in advance, will be the smaller the larger the number of observations.

From these two lemmas follows the theorem:

3. If only the number of observations is sufficiently large, the deviations can be supposed to be frequently very small (1957, p. 106).

These considerations justify the application of Bernoulli's theorem Chebyshev's [the Bienaymé – Chebyshev] inequality etc. in applied statistics.

3. Sample surveys

Anderson must be counted among the pioneers of modern sample surveys. He participated in 1913 – 1917 [in 1915] in a representative sampling survey of agriculture in Turkestan. He was also influential in the preparation of the Bulgarian agricultural sample census of 1926. In the same country he started in 1936 a yearly sample census of agricultural acreage and production. In his many theoretical contributions to this subject he stresses the point of view that the sample census must be based upon a probability model. The level of tolerance and the desired accuracy of results should be fixed in advance.

4. The variate difference method

The work of Anderson on this method is perhaps best known in America. His own work follows some articles of Poynting, Hooker, Cave and March and is contemporary with the work of Student (W. S. Gosset). His main contribution is the German monograph of 1929. An account of the earlier history and the contributions of Anderson may be found in my own monograph[1].

The Variance Difference Method is an attempt to deal with time series while making a minimum of assumptions. We assume that the series consists of a *smooth* systematic part (trend and long cycles, business cycles) and superimposed independent random errors. These errors are not autocorrelated. Then we can completely eliminate the smooth part of the series by taking finite differences if the smooth part is a polynomial. If it is a *well behaved* function of time we can at least reduce the systematic component indefinitely by computing differences. After taking enough differences we are left with the random component alone, or at least with a series which contains only insignificant reminders of the systematic part of the time series.

The problem is how many differences we ought to take. If our task is done in the *k*-th difference series, then this series and the series of all higher differences will contain only the random part. Anderson worked out formulas which permit the comparison of the variances of two consecutive difference series. His results were later improved by Zaycoff, one of his Bulgarian students. I have myself proposed a somewhat inefficient method in this field based upon the assumption of normality of the error component and the principle of selection. This procedure utilizes only a part of the available differences at a time. Later I succeeded to find the exact small sample distribution of the variances of Variate Differences if the errors are normally distributed and we deal with a circular universe[2]. This work is being continued by J. N. K. Rao and me.

Criticism of the Variate Difference Method was offered by Bowley, Persons, Fisher, Bartlett, M. G. Kendall and Wald. They pointed out that higher differences are not likely to be very accurate, that the existence of autocorrelation makes the method inapplicable, as does the appearance of short periodic fluctuations (seasonal movement). No doubt this criticism is sometimes justified. Nevertheless, in spite of the great progress made in the field of statistical analysis of stationary time series the Variate Difference Method still offers a possible treatment of evolutionary statistical series, i. e. series which contain a trend. Since this is always the case with empirical economic time series, the method has not yet lost practical interest for econometricians.

In practical econometric work the idea of working with first differences which is very popular in applied econometrics, may be considered as an adaptation of the Variate Difference Method[3]. It has been the practical experience of most workers in the field that for short yearly series used in econometric work the linear trend contributes most of the systematic variation. Hence the use of first differences which eliminates a linear trend (or an exponential trend, if the data are logarithms) will sometimes greatly reduce the autocorrelation of the original time series.

Apart from these applications the Variance Difference Method did not become very popular among practicing econometricians. But work in this field, essentially based upon the fundamental ideas of Anderson, is still continuing[4].

5. Time series analysis

This work of Anderson is closely related to his work on the Variate Difference Method. Most outstanding among his papers is his devastating criticism of the Harvard method of analysis of economic time series. This publication contributed much to the replacement of these methods by more efficient ones.

6. Econometrics

Anderson was among the founders of the econometric society and its fellow. He must be counted among the most important contributors of this science during the early period. Among his contributions we mention only an effort to verify statistically the quantity theory of money which is of special interest now because of the resurrection of the quantity theory by the Chicago school of economists[5]. His work on the scissor problem, i. e. the divergent movement of agricultural and industrial prices should be of special interest for agricultural economists. His publications on Bulgarian economics are very important sources for the economic history of this country during the period between the two world wars. His interesting review of the famous book of von Neumann and Morgenstern on game theory has contributed much to acquainting the German speaking world with these new ideas.

7. Index numbers

Anderson's work includes many contributions to the theory of index numbers. He was most interested in the practical construction of index numbers of production and cost of living index numbers. The problem of chain index numbers was also treated by him. On the whole he was sceptical to the ideas which are now generally treated under the heading of the aggregation problem and also to Wald's ideas on cost of living index numbers.

8. Correlation

His work on statistics included some remarkable contributions on the general problem of correlation, which are always much influenced by the ideas of Chuprov in this field.

Anderson feels very strongly that the usual theory of correlation and regression, as presented in statistical textbooks, is not applicable in the social sciences because the underlying populations are not normal. Hence he has developed nonparametric (distribution-free) methods or tests of significance of correlation and autocorrelation coefficients. This approach certainly deserves the attention of theoretical statisticians and econometricians.

9. Textbooks

Not the least contribution of Anderson to theoretical statistics is his three textbooks. The reader can find in them a very clear presentation of the fundamental ideas of statistics with a minimum of mathematics. The German textbooks have had a great influence in Germany and the German speaking countries. It would be most desirable if the last edition of his latest textbook of 1957 could be translated into English. It is an excellent presentation of statistical methodology for the use of workers in the social sciences. Because of the difference of some of Anderson's ideas about statistics from the prevailing Anglo-American school which is very well brought out in this text, a translation might stimulate discussion on these problems.

Author's notes

1. G. Tintner (1940), *The Variate Difference Method*. Bloomington, Indiana. *Econometrica* (1952), New York, pp. 208ff. (?)

2. G. Tintner (1955), The distribution of the variances of variate differences in the circular case. *Metron*, vol. 17, pp. 3ff.

3. G. Tintner, *Econometrica*, Op. cit., pp. 325ff.

4. See, e. g., A. R. Kamat (1954), Distribution theory of two estimates for standard deviation based on second variate differences. *Biometrika*, vol. 41, p. 1ff.

A. R. Kamat (1958), Contributions to the theory of statistics based upon first and second successive differences. *Metron*, vol. 19, pp. 97ff.

P. G. Moore (1955), Properties of the mean square successive difference in samples of various populations. *J. Amer. Stat. Assoc.*, vol. 50, pp. 344ff.

A. P. Moore, F. E. Grubbs (1957), Estimation of dispersion from successive differences. *Annals Math. Stat*., vol. 18, pp. 194ff.

J. N. K. Rao (1959), Note on mean square successive differences. *J. Amer. Stat. Assoc*., vol. 54, pp. 801ff.

5. M. Friedman (1959), *Programme for monetary stability*. New York.

M. Friedman, Editor (1956), *Studies in the quantity theory of money*. Chicago.

I have left out a large portion of the unnamed introduction because much of it is included in other obituaries of Anderson and since it contains mistakes; for example, Anderson taught in Kazan! I have corrected some mistakes in the text itself, but Tintner's treatment of Cournot, apparently copied from Anderson (§ 2), should be discussed separately. First, following his close friend Bienaymé, Cournot never mentioned the law of large numbers, and for this reason alone his arguments ring hollow. In any case the *Cournot bridge* was Anderson's (?) brainchild. Then, *physical impossibility* is the same as *moral certainty* (introduced by Descartes, 1644). Cournot's *theorem* (just where did he introduce it?) was based on empirical observations and expressed the strong law of large numbers. That law although not its wide scope was known then and Cournot's merit was greatly exaggerated.

And now I ought to apologize. I somehow lost the author’s bibliography (pp. 278 – 280 of his paper) but still hope that my curtailed reprint is still useful.

X

Oscar Sheynin

Gumbel, Einstein and Russia

Moscow. Sputnik, 2003

1. Introduction

Emil Julius Gumbel (1891 – 1966) was an outstanding German, and later American statistician best known for his work on the extreme-value theory. I describe his political activities (not leaving aside its statistical element) and his unpublished correspondence with Einstein, and I attempt to show why he, and many more celebrated Western intellectuals had been supporting the Soviet Union in the 1920s – 1930s in spite of the horrors there perpetrated. I also dwell on Gumbel's unknown connections with other mathematicians and natural scientists including Mises.

In the 1920s, Gumbel tirelessly battled against the rightist movement in his native Germany, and among his likeminded colleagues was Einstein with whom Gumbel became closely associated. This activity coupled with his Jewish origin made him a prominent target of various attacks, in particular by students infected with Nazism; his academic career had been blocked for many years and his very life was endangered[1].

In 1933 he emigrated to France, and in 1940 barely escaped to the United States where he lived and worked until his death. Gumbel never ceased his social and political activities. In France, he tried his best to help his fellow-refugees and denounced Nazism, and in the US he published several political papers and letters in newspapers[2] and became a member of two bodies for the liberation of Germany (Jansen 1991, p., 390).

In 1926, Gumbel worked for several months in Moscow and he visited the Soviet Union in 1932. Because of the situation in Germany, he wished for some time to remain there permanently, but happily failed (end of § 4). Johnson & Kotz (1997) briefly described Gumbel's life and work and cited previous pertinent writings[3] whereas Jansen (1991) and Vogt (1991) examined his political activities. Both Jansen and Gumbel [28] include reprints of quite a few of his political contributions and the former, drawing on archival sources, also appended a valuable list of 583 Gumbel's writings and related materials[4]. However, it is composed pell-mell: scientific works, tiny reviews, popular pieces (about 30 in all), some independently entered translations of Gumbel's works, anniversary articles, abstracts, political writings, and literature about him, – all these items follow one after another chronologically. A few of Gumbel's papers in the Russian periodical *Matematichesky Sbornik* are recorded there twice, the second time as though having also appeared in *Recueil Math. Soc. Math. Moscou* which is the additional French title of the same journal.

Jansen's description of Gumbel's life and work is based on many archival and newspaper sources, but he had not provided a bibliography of the pertinent comments, nor did he furnish a list of his numerous abbreviations. Again, he had not offered a proper bibliographic description of Gumbel's contributions included in his book: in a few cases he mentioned the appropriate English articles, – but who translated them, and/or changed their original titles?

I consider Gumbel's writings and statements on Russia (§ 2) and his unpublished correspondence with Einstein (§ 3)[5]. In § 2 I also indicate some previously unknown points concerning Gumbel the statistician. In a special section (§ 4) I examine the implications of § 2 and provide Gumbel's conclusions in a historical perspective by describing the relevant views of other intellectuals. I consider the Einstein – Gumbel correspondence in several subsections one of which is devoted to Einstein's political thinking. There, drawing on previous authors, I begin by sketching his attitude towards the Soviet Union.

Gumbel allegedly desired to describe Russia comprehensively and readers might have indeed expected that he, having been a statistician and an economist[6], had painted a truthful picture, but he did not.

I draw on the *Bolshaia Sovetskaia Enziklopedia* [Great Soviet Encyclopedia], three editions: 66 vols, 1926 – 1947, 51 vol., 1950 –1958, 30 vols, 1969 – 1978, respectively. The third edition is available in an English translation (separate translation of each volume). I abbreviate this source as BSE or GSE respectively and in the latter case I indicate the appropriate years of both versions.

I conclude here by a letter from Gustav Radbruch[7] of 24 Nov. 1930 to Einstein (46519, see Note 5) and a description of the related developments. Here is the letter itself.

Gestatten Sie mir, streng vertraulich und ohne Wissen des Hauptbeteiligten mich mit einer Bitte an Sie zu wenden, die ich nur durch das Bewusstsein der Gesinnungsgemeinschaft zu rechtfertigen vermag. Sie haben früher bereits an der Angelegenheit des hiesigen Privatdozenten und jetzigen Professors[8] Dr. Gumbel Anteil genommen.

Sie wissen auch, dass in den letzten Wochen von national-sozialistischer Seite aus Anlass der Ernennung Gumbels zum Titularprofessor nicht nur unter Berufung auf die sechs Jahre alte unglückliche Äußerung Gumbels vom "Feld der Unehre"[9], sondern auch auf seine gesamte Enthüllungspolitik gegen Geheimrüstungen, politische Morde und Fememorde der Kampf gegen Gumbel erneuert worden ist. Wie die Dinge auf deutschen Universitäten einmal liegen, fürchte ich, dass, – weniger infolge einer entschiedenen politischen Rechtseinstellung als, was schlimmer ist, infolge von Konfliktsangst, kaum eine Fakultät mehr den Mut finden wird, Gumbel zu berufen.

Für Heidelberg aber ist der Fall Gumbel eine unerschöpfliche Quelle immer neuer Beunruhigungen, die gerade auch wegen der Angreifbarkeit des ursprünglichen Ausgangspunktes der ganzen Hetze die Stellung der links stehenden Heidelberger Professoren sehr erschweren. Ich glaube dass Gumbel trotz unleugbarer Taktfehler in seiner Vergangenheit durch seinen ebenso unleugbar großen politischen Mut es verdienst, dass man sich seiner Zukunft annimmt. Über Gumbels mathematische und statistische Fähigkeiten und Leistungen steht mir zwar kein Urteil zu, aber sie werden, soweit mir bekannt ist, von Fachleuten hoch eingeschätzt.

Und so möchte ich die Frage und Bitte an Sie, hochverehrter Herr Professor richten, ob Sie nicht in der Lage sind, Ihren großen Einfluss für eine Berufung Gumbels in eine seinen Fähigkeiten und Leistungen entsprechende andere Position, etwa bei der Kaiser Wilhelm Gesellschaft[10], einzusetzen. Ich darf nochmals betonen, dass dieser Brief ohne Wissen Gumbels ergeht, er ergeht aber im Einverständnis meines nationalökonomischen Kollegen Lederer, der die akademischen Aussichten Gumbels unter den gegebenen Verhältnissen ebenso ungünstig beurteilt wie ich und auch seinerseits nur von Ihnen noch Hilfe erwartet.

On 29 Nov. 1930 Radbruch (46520) thanked Einstein for his answer, and on 27 Nov. Lederer (46522) wrote to Einstein as well. He largely repeated Radbruch's letter; described Gumbel's strained circumstances; and stated that the "nationalsozialistischen Studenten" will likely resort to ruthless attacks against Gumbel. And he also explained how Gumbel was invited to Heidelberg:

Er wurde uns seinerzeit, als wir einen Statistiker gewinnen mussten, von Prof. Von Bortkiewicz auswärmste empfohlen, und die Wertschätzung der Fachkreise geht ja auch aus der guten Resonanz seiner Publikationen in der Literatur hervor.

Bortkiewicz rarely recommended anyone (Woytinsky 1961, pp. 452 – 453)!

Extracts from Einstein's answers to Radbruch (§ 3.1.1, No. 1, § 3.3, NNo. 1 and 3) were published as a single whole in the Editorial (1931, p. 109). I partly reproduce Einstein's answer to Lederer in § 3.3, No. 2.

Acknowledgement. The Albert Einstein Archives, The Jewish national and University Library, Hebrew University of Jerusalem, that keeps the Einstein correspondence, allowed me to quote/publish the relevant letters. I am also thankful to Dr. Barbara Wolff, Assistant Curator of the Archives, for copies of the relevant letters and to Dr. A. L. Dmitriev (Petersburg) for some Russian materials.

2. Russia

2.1. The Year 1922. For a leftist intellectual whom Gumbel became, it was natural to turn his attention to Russia; in 1922, he published his first pertinent publication [2]. There, he stated that Soviet Russia served as a catalyst of social struggle the world over (p. 194) and that communism was "our wish" (p. 195).

Gumbel added, however, that the Bolshevist way to it led "durch Blut und Hunger"[11]. He thought that the transition to communism by parliamentary methods was impossible (p. 195)[12]; that the Soviets failed to ensure the participation of masses in governing Russia (p. 199) with all power having gone to the Bolshevist party (p. 200). However, the downfall of the Soviets will not necessarily be repeated elsewhere (Ibidem)[13]. The proper way to communism, Gumbel also stated, lay through a "geistiger Wechsel" with which the Bolsheviks do not agree because of their materialistic philosophy (p. 198). Other necessary conditions for the transition of a country to communism are its healthy economy and a majority approval of the changes (p. 202). Gumbel (p. 199) recognized that Russia must have a "gebundenes Wirtschaftssystem" with yet unknown features but he did not elaborate[14].

2.2. The Year 1926

2.2.1. Marx's Mathematical Manuscripts. On 21 June 1925 Gumbel (43811) asked Einstein to recommend him, in particular, to the eminent biologist Julius Schaxel hoping that the latter will help him find a position in Moscow[15]. His plan proved only partly successful. Indeed, on 30 April 1926 Gumbel (43814) informed Einstein:

> Ich war jetzt sechs Monate in Moskau und habe im Marx – Engels [16] Institut die sogen. Mathematischen Manuskripte von Marx druckreif gemacht. Es handelt sich dabei um Notizen zur Differenzialrechnung, die ein gewisses philosophisches Interesse besitzen und zeigen, dass Marx die Anfangsgründe des Differenzierens wohl beherrscht hat. Meine Arbeitsbedingungen waren außerordentlich günstig. Allerdings lebt die Mehrzahl der dortigen Gelehrten in großer Notlage.

An article by Kolman (1968)[17] preceded the publication of Marx's manuscripts (MSS) (1968). He (p. 104) ridiculously alleged that Marx's statements on mean values in economics were of exceptional methodological value for mathematical statistics. Then Kolman (p. 106) reported that the Marx – Engels Institute had entrusted Gumbel with "working on the manuscripts", but that he "was unable to appreciate in full measure either the importance of their publication or their philosophical and historical-mathematical significance"[18].

This is doubtful (see Gumbel's letter above) and in any case Gumbel published a preliminary report [9] where he classified the MSS; then, his final report had never appeared (Vogt 1991, pp. 20 – 22) whereas Yanovskaia, the future eminent specialist in mathematical logic who eventually prepared the MSS for publication, had spent incomparably more time on this than Gumbel had[19].

Quite a few mathematical articles were devoted to these MSS, e.g., Kennedy (1977), who referred to earlier commentators. A preliminary version of the MSS is Marx (1933). It was accompanied by Yanovskaia's commentary (1933) and preceded by an introductory note by the Marx – Engels – Lenin Institute (where Gumbel was not mentioned). It is also necessary to cite Glivenko (1934). To conclude, I note that Marx's contributions do not reflect his studies of mathematics[20] and that his MSS contain no items on statistics or probability [21].

2.2.2. Statistics and Class Struggle. During his work in Moscow, Gumbel apparently met Schmidt who then held some position at the Communist Academy there[22]. Indeed, on 14 Dec. 1926 he wrote a letter to Schmidt [28, pp. 179 – 180] describing the contents of five of his prepared "works" on probability and asked whether they will interest the "verehrter Genosse Otto Julewitsch". At least three of these had really been put out in Russian periodicals. In all, Gumbel published five political writings (1923 – 1937) and ten scientific contributions in the Soviet Union[23], not all of them in Russian. Some of them appeared earlier, and some later in the West.

Soon after leaving Moscow, Gumbel published a paper on statistics and class struggle [6] and his observations on life in the Soviet Union, see below and § 2.2.3. Regarding the class nature of statistics in capitalist countries, Gumbel [6] stated that

1) Due to moral and economic reasons, statistics is unable to discover the causes of social phenomena (p. 132).

2) Statistical data (on harvests, p. 142; unemployment and industrial accidents, p. 147, etc.), are distorted or hushed up (prostitution, abortions, p. 139) so that the ensuing calculations (e.g., of subsistence le-vels, p. 147) are wrong.

3) Although statisticians may well consider themselves objective, the application of statistics "belongs" to the ideological class struggle (p. 133).

4) Many statisticians attempt to prove Malthusianism (p. 134). However, taken by itself the notion of overpopulation is meaningless (p. 135). And the aging of the population is of no consequence as compared with the other evils of the society[24].

5) Criminal statistics shows the devastating nature of capitalism (p. 140). It reflects the intensity of class struggle; not by chance did Czarist Russia possess ideal pertinent data (pp. 141 – 142).

It is difficult to understand his last statement especially since elsewhere Gumbel [8, p. 106] maintained that statistics in pre-revolutionary Russia was "ganz unentwickelt"![25].

Gumbel tacitly assumed that capitalism was unable to change and naively thought that the socialist system was much superior. Thus, the shackles restricting statistics (his Item 1) will only disappear in a classless society[26].

Two additional points. First, Gumbel noted that statistics was connected with national economy which was the reason for its low scientific level (p. 134); that an empirical check of the so-called laws of the latter was still impossible (p. 142); and that (p. 148) only mathematical statistics will be able to solve the problems of economics. These statements may be regarded as heuristic arguments in favour of creating the then not yet existing econometrics[27].

Second, I quote Gumbel's extraordinary declaration (p. 141):

> Bei politischen Morden selbst ist zu unterscheiden, ob sie revolutionär oder könterrevolutionär sind.

Only one step thus separated him from exonerating the death sentences meted out by phoney courts in Russia[28]. To some extent, Gumbel repeated his deliberations elsewhere [11, Bd. 5] and the notorious statement just above is also there (p. 19).

2.2.3. Gumbel's Travel Notes. Gumbel [8, p. 83] saw the overall social problem confronting the world as tracing the route to socialism; and the main question (p. 164) was, how long will capitalism still survive[29]. The "usual formal democracy" of the Western type will not do, what is needed is dictatorship of the proletariat (p. 113)[30]. Accordingly, the restoration of Russia's economy achieved in the absence of private ownership is the Russian communists' "immortal merit" (p. 112).

The terrorism, that the communists unleashed during the previous years against profiteers and even petty violators of the draconian commercial regulations, was economically justified (p. 99). Horrible political terrorism also took place (pp. 100 and 125) but it was only a side-effect of the civil war (p. 125) and partly occasioned by sabotage (p. 95). At present, capital punishment is "often" pronounced (p. 126), and the secret police, the GPU, enjoys the right to exile citizens from the main cities; again, the GPU "often" imprisons people for months on end before even beginning the investigation[31].

The New Economic Policy (NEP) which was introduced in 1921 brought about some economic freedom, and Gumbel noted the presence of street vendors (p. 120), privately working physicians (p. 125) and private publishers (p. 144). Overall, the existing economic system is state capitalism with a socio-political bias ("Einschlag") (p. 110); or, state capitalism coupled with a detestable bureaucracy (p. 164)[32], also see below.

In spite of the official materialistic philosophy (p. 132), practical idealism is widespread (p. 133) and this constitutes "perhaps" the greatest ethical merit of the Russian communists; top people remain poor (Ibidem; but see § 2.3), and, more generally, party members are not allowed to earn more than an established amount of money (p. 114)[33].

"Usual" prisoners may leave jail once in a month (p. 126), soldiers are free to spend nights outside the barracks (p. 149) and foreign newspapers are sold in town (p. 135). Naïve comments on the relation between the state and the Russian Orthodox Church follow (p. 140)[34].

All power belongs to the party within which there exists democracy (p. 113) and quite exceptional opinions are tolerated (p. 136). The author apparently sees no contradiction between these statements and his other observations: "from time to time" purges are taking place in the party (p. 114) and deviationists are punished and even expelled from the party (p. 142). He (p. 159) also notes "political struggles" going on in the party and names Zinoviev and Trotsky[35] and correctly remarks that communism is a religion of sorts (pp. 140 – 141).

Civil rights do not exist (p. 116); even foxtrot is banned (p. 119). The complicated voting system ensures "necessary" results (pp. 103 and 115), the national republics cannot actually leave the Union (p. 116) and Zionism is forbidden (p. 139). Only 60% of the children attend school (p. 129), the professorial staff is underpaid (p. 130) but researchers fare good enough (p. 131). The evolution theory is the most important discipline of natural sciences whereas the theory of relativity was for a long time regarded as hostile (p. 133) and all scientific problems are considered together with their "final philosophical consequences" (p. 134)[36].

The housing conditions in Moscow are horrible, which is a corollary of its having become the capital and of the influx of rural population rather than the communists' fault (pp. 121 – 123)[37].

Bureaucracy is omnipotent (pp. 116 – 117 and 155). So as to prevent the build-up of a new bourgeoisie, draconian measures are being taken since 1924 against successful NEP-men (p. 157). Gumbel lists these measures (both political and economic) and adds that economic steps should be applied instead; he apparently thought about subtle "European" methods. The black-market value of the rouble is lower than its official value and often experiences slumps (p. 109).

The agrarian problem is the most acute issue (p. 105). A half of the peasants is poor (p. 107) and depends on the rich ones, the kulaks (p. 102). The situation is dangerous and agricultural cooperation is necessary (pp. 107 and 162)[38]. Either the state, or the kulaks and the NEP-men will accumulate capital more rapidly and the stability will persist or not, respectively (p. 163).

Gumbel is thus apparently prepared to abandon his advice regarding subtle economic measures (above). Colonies are the soft spots of imperialism; Russia supports their nations (p. 152) and the Red Army might possibly help a revolution elsewhere (p. 148)[39]. The independence of Finland, the Baltic states and Poland was recognized on the strength of the right of nations to self-determination (p. 147)[40].

2.3. The Year 1932. In 1932 Gumbel spent three weeks in Moscow and published his new travel notes [17]. As compared with 1926, Moscow became better-looking (not many beggars; no waifs or strays; less hawkers; more state-owned cars and trucks), but the housing situation worsened [still more] (p. 298). Inflation did exist and is dangerous because industrial plans, when formulated financially, become fictitious; however, without any capitalist class present, nobody benefits from its action (p. 302). He should have said: nobody benefits except for the state (for example, due to almost forced participation of the working people in yearly long-term state loans) whose interests did not at all coincide with the desires of the population, see Note 14. Food was rationed and its shortages led to hoarding (p. 300); the black-market cost of a Deutschmark was ten times higher than its official value (p. 301)[41].

The top people were poor ("persönlich arm")[42] but frightfully powerful (p. 299) whereas scholars were compelled to toe the political line (p. 301). In principle, Russian problems are solved (p. 305); contrary to the situation in the West, people are living better than before; "from their sweat, blood and tears new factories belonging to them [?] are being built" (p. 306).

2.4. The Eye-Opening years. 1) From 1934 onward, Gumbel began to express second thoughts (Jansen 1991, p. 67). In his letters of 1936 and 1938 he wrote about his deep disillusionment. "Insbesondere", as Jansen claims, he was affected by the Moscow "Schauprozesse" of these years.

2) No less indicative was the decision of Heinrich Mann, Gumbel and "andere" who founded, in 1937, a *Bund freiheitlicher Sozialisten*, to separate themselves "programmatisch scharf gegen den Marxismus" (Jansen 1991, p. 42)[43]. It seems nevertheless that (Ibidem, p. 67)

Bei aller Skepsis [much too weak] über den sowjetischen Weg zum Sozialismus hatte er [Gumbel, in 1934 – 1936] doch am historischmaterialistischen Fortschrittsdenken festgehalten.

Also in 1937, Gumbel undoubtedly had to note the absence of any Soviet mathematician (e.g., of Kolmogorov and Khinchin) at a conference on probability theory (Compliments 1937) attended by such figures as Cramér, de Finetti, Feller, Hostinský and Polya and by him himself.

3) In 1939 Gumbel signed a manifesto prepared by the German members of the Union Franco-Allemande which claimed that the Hitler – Stalin pact was a betrayal of peace by Russia (Jansen 1991, p. 44).

4) In 1954, Gumbel [23, p. 329] scornfully described the situation in East Berlin, and, on p. 330, he mockingly called the late Stalin the greatest philosopher "of our time".

5) In 1957, reporting on his travel impressions, Gumbel (Jansen 1991, p. 70) said that

Die Stalinisten der Sowjetzone [of Germany] sind Papageien, die Worte eines Herrn nachplappern, der längst tot ist.

6) In 1960, Gumbel [25, p. 338] did not restrict his criticism of East Germany to food shortages (Note 14). His verdict was, that the emigration from there

> Verdankt sich nicht nur materiellen Gründen. Grundlegend ist der intellektuelle Druck und der Mangel an Sicherheit.

7) In 1961, Gumbel [26, pp. 264 – 268] described Russia's participation in Germany's secret rearmament (1922 – 1933)[44] and remarked (pp. 265 – 266) that "All diese Tatsachen … waren bereits in der Weimarer Republik bekannt" – and to him as well?

Then, he [26, pp. 268 – 269] denounced the "russischen Prozesse":

Von 1937 an reinigte Stalin die Partei von den alten Bolschewiken. … tausende wurden nach geheimen Verfahren hingerichtet … [In 1956] hat Chruschtschow Stalin als großen wahnsinnigen Tyrannen angeprangert …

8) Finally, in 1964 Gumbel reviewed an English translation of one of Solzhenitsin's officially published novels. He [27] remarked that the real situation in the Soviet Union became known even earlier[45] and that the author had properly chosen to show the fate of an ordinary man who was thrown into
a labour camp just in case, and, practically speaking, for life.
Although Gumbel believed that there were "perhaps" 10 mln such victims [see § 4], he did not say anything about his earlier illusions.

3. Einstein

3.1. He Tries To Help Gumbel.

From 1923 to 1932 Einstein wrote at least six letters recommending

Gumbel to five universities, all of them beyond Germany, and in a few other cases he expressed his willingness to help him secure an academic position and/or his high opinion of Gumbel.

3.1.1. Einstein's Opinion. 1) His letter of 28 Nov. 1930 (46526) to Radbruch.

Herr Gumbel ist zweifellos als Fachman[n] hinreichend tüchtig, um als Vertreter seines Faches an einer Hochschule zu wirken. Als Persönlichkeit schätze ich ihm noch viel höher. Sein politisches Wirken und seine Publikationen sind von einem hohen Ethos getragen …

Das Richtigste für Herrn Gumbel dürfte es wohl sein, an einer ausländischen Universität eine Stelle zu suchen. Ich habe mich in diesen Sinne schon öfter für ihn bemüht und bin gerne bereit, mich jederzeit für ihn einzusetzen …

2) His letter of 25 July 1932 (50120) to Gumbel.

Es ist mir klar, dass Sie von hier fort sollen. … Wenn Sie mir eine Stelle oder eine Persönlichkeit angeben, will ich gerne dorthin schreiben.

3) His letter of 2 Jan. 1932 (50110), probably to E. Montel[46].

Ich schätze ihn [Gumbel] sehr hoch … unter den gegenwärtigen Verhältnissen nicht nur seine Position, sondern auch sein Leben bedroht ist.

4) His letter of 16 May 1933 (38615) to Gumbel.

Charakterleistungen sind ebenso viel Wert wie wissenschaftliche; deshalb brauchen Sie nicht in den Schatten zu stellen.

This was Einstein's partial response to Gumbel's letter of 10 May 1933 (38614). There, Gumbel described the difficult conditions of life for German academics who had fled to France, mentioned an appropriate "Vorschlag" made by Perrin and concluded by stating (more generally) that

Ein großer Teil der Abgesetzen, wie etwa Franck, Born etc [et al] steht so hoch, dass ein Vorschlag meinerseits gar nicht notwendig erscheint[47].

5) His letter of 12 Oct. 1943 (55236) to Gumbel. "… bin ich bereit Sie dort [wo Statistiker gebraucht werden] zu empfehlen".

3.1.2. He Recommends Gumbel. 1) His letter of 15 April 1923 (43810) to C.F. [?] Nicolai in Cordova [evidently, South America][48].

Herr Dr. Gumbel ist mir seit einer Reihe von Jahren als ein scharfer wissenschaftlicher Geist und als vortrefflicher Mensch aufs beste bekannt.

Von Studium Physiker hat er sich als Spezialgebiet die Statistik im weitesten Sinn gewählt, deren Berührungspunkte mit der Nationalökonomie ja zutage liegen. In seiner schriftstellerischen Tätigkeit hat er allgemein politische und nationalökonomische Fragen behandelt, soweit sie die Gegenwart betreffen. … Ich bin überzeugt, dass er vermöge seiner großen Belesenheit und der Beweglichkeit seines Geistes sehr wohl geeignet wäre als Lehrer der Nationalökonomie zu wirken.

2) His letter of 25 Jan. 1928 (46508) to Karl Pearson.

Ich schätze Herrn Dr. Gumbel sowohl persönlich wie als außerordentlich intelligenten wissenschaftlichen Arbeiter sehr hoch, wenn ich auch in dem hauptsächlich von ihm bearbeiteten Spezialgebiet der Statistik mir kein Urteil erlauben darf.
Ich möchte erwähnen, dass Herr Gumbel durch zahlreiche mutige politische Schriften sich große Verdienste im öffentlichen Leben Deutschlands um die Gerechtigkeit erworben hat[49].

A few years before that Gumbel published two notes in *Biometrika*, and quite a few letters were exchanged in 1928 in connection with his attempts to secure a (provisional) position at the Galton Laboratory, University College.
To achieve this goal, Gumbel applied for a fellowship to the European Office of the then existing International Educational Board [50].
Pearson agreed to take Gumbel on; see the Board's letter of 18 Jan. 1928 to him (46504), and Gumbel's letter of 26 Jan. 1928 to Einstein (46509).
Einstein (his letter to Gumbel of 25 Jan. 1928, 46506), however, mentioned "mehrfache schlechte Erfahrungen, die ich [er] mit dem Education Board schon gemacht habe …" Gumbel, as he remarked there, had already overstepped "die obere Altergrenze" for a fellowship.
On 12 May 1928 Gumbel informed Pearson (Pearson Papers 709) that Mises as proposer and Bortkiewicz as seconder will formally apply for the fellowship, and he also adduced a letter of recommendation from Einstein (apparently lost).
Neither Mises nor Bortliewicz is known to have been engaged in political life of Germany, and a few years later, in 1931, the latter died [51] and the former fled Germany by the end of 1933 or very early in 1934. It is therefore all the more interesting to put on record their attempt to help Gumbel. Furthermore, on 22 April 1931 (46545) a Geh. Regierungsrat, Prof. Holde, in a letter to Einstein, listed quite a few intellectuals who were prepared to sign an "Erklärung" supporting Gumbel's efforts to hold his academic position against political attacks. Among these personalities were Radbruch, Rademacher and Mises. Einstein (his previous letter to Holde of 21 April 1931, 46544) was "selbstverständlich bereit Ihren Erklärung zu unterzeichnen".
3) His letter of 13 April 1931 (46538) to Prof. Berwald (Prague).

Ich habe gehört, dass an der deutschen Universität eine Lehrstelle für theoretische und praktische Wahrscheinlichkeitslehre[52] zu besetzen ist. Ich empfehle Ihnen für diese Stelle den fähigen und fleißigen Herrn Dr. Gumbel, der an der Universität Heidelberg Privat-Dozent [see however Note 1] ist, und von dem ich überzeugt bin, dass er als Lehrer und Forscher die auf ihn gesetzten Erwartungen getreulich erfüllen wird. Er hat sich auch durch Publikationen rechtlich-politischen Charakters große Verdienste erworben, die ihm gegenwärtig gehässige Verfolgungen eintragen, die aber wohl später ihre gerechte Würdigung finden werden.

4) His letter of the same date (46540) to Lieber Herr Professor Philipp Franck at the same university.

Herr Gumbel ist ein klüger Kopf und hat sich durch seine mutigen Bücher über die Entgleisungen der Militärgewalt in Deutschland ein wirklich großes Verdienst erworben. Er wird deshalb von der reaktionären akademischen Kamarilla wütend verfolgt. Lassen Sie sich nichts weismachen, sondern stehen Sie bitte mannhaft für ihn ein, wie er es verdient.

5) His letter of 2 Jan. 1932 (50110) partly quoted in §3.1.1, likely to Montel.

Herr Gumbel ist zweifellos ein Mann, der mit einem seltenen Mute und seltener Hingabe für Gerechtigkeit und Verbesserung der zwischen-staatlichen Verhältnisse gekämpft hat[53] . … Gumbel ist auch als wissenschaftlicher Statistiker (angewandteWahrscheinlichkeitstheorie) als tüchtiger Fachmann bekannt, wenn auch seine fachlichen Leistungen nicht als außergewöhnlich bezeichnet werden können.

And so, Einstein understood statistics as applied probability; above (Item 3), when mentioning the "practical theory of probability", he also apparently meant statistics. I (1998b; 1999) have discussed the relations between probability and statistics and (1998b, p. 104) noted that Mises, evidently in the 1940s or a bit later, and Neyman, in 1950, had thought that some classes of probability problems belonged to statistics. However, Kolmogorov, in 1938, had kept to the opposite opinion: statistics gradually ceases to be applied probability and probability ought to be considered as a "structural part" of statistics.

Montel answered Einstein on 4 Febr. 1932 (50111): Gumbel was luckily invited to deliver lectures at the Institut Henri Poincaré; and Langevin lui-même will certainly confirm this.

6) His letter of 3 Dec. 1932 (50124) to Prof. MacClelland at University of Pennsylvania.

Ich habe erfahren, dass an Ihrer Universität eventuell eine Lehrstelle für mathematische Statistik gegründet wird. Mit Rücksicht auf diese Eventualität erlaube ich mir hiermit, Sie auf Herrn Professor Dr. Gumbel aufmerksam zu machen … Herr Gumbel ist bezüglich seiner Fähigkeiten und seiner menschlichen Qualitäten ein in hohem

Masse würdiger Kandidat für eine derartige Lehrstelle. Er wäre wohl schon Inhaber einer ordentlichen Professur an einer deutschen Universität, wenn er nicht durch wertvolle Publikationen allgemeinen allgemein-politischen Inhalts den Zorn der gegenwärtig leider in so hohen Masse irrgeführten studentischen Jugend dieses Landes erweckt hatte.

3.2. His Participation Desired

1) Gumbel's letter to him of 26 Dec. 1934 (50133).

Das Institut de Science Financière et d'Assurances der Universität Lyon, an dem ich als Assistent tätig bin, beabsichtigt demnächst eine kleine Zeitschrift herauszugeben, welche sich mit Wahrscheinlichkeitstheorie und verwandten Gebieten beschäftigen soll. Bisher haben I. Hadamard, M. Fréchet, G. Darmois und Francis Perrin ihre Mitarbeit zugesagt.

Ich gestatte mir die Anfrage, ob Sie prinzipiell bereit wären, ebenfalls als Mitarbeiter zu figurieren. Darüber hinaus wäre ich Ihnen sehr verbunden, falls Sie bereit wären, uns ein kurzes Leitwort zu senden das wir zu Beginn der ersten Nummer publizieren dürften.

Einstein's response is unknown, but the periodical hardly ever appeared.

2) Gumbel's letter to him of 18 Nov. 1935 (50135).

… ich erlaube mir, Ihnen in der Anlage [lost] den Plan zu einem Buch zu übersenden. Obwohl ich mit den Vorbereitungen erst heute anfange, möchte ich Sie bereits in diesem Stadium sei es um Ihre Mitarbeit, sei es um ein Vorwort bitten. Am liebsten wäre es mir, wenn Sie sich mit beidem, zunächst prinzipiell, einverstanden erklären würden. Jede Zeile von Ihnen wäre mir wertvoll.

3) Einstein answered on 3 Dec. 1935 (50137).

Ich kann mich mit Ihrer Idee nicht befreunden. Ein Buch mit kurzen Referaten über Facharbeiten aus allen Gebieten kulturellen Schaffens dürfte kaum Absatz finden. Der Umstand, dass die Arbeiten von Vertriebenen herstammen, dürfte kaum für die Käufer einen hinreichenden Anreiz bieten. Was mich betrifft, so wüsste ich überhaupt nicht, wie ich über meine Publikationen in einem solchen Rahmen referieren sollte. Ein Geleitwort könnte ich vielleicht geben, wenn die Sache wirklich gelingen sollte, der ich einstweilen skeptisch gegenüber stehe.

Apparently Einstein had not indeed published any popular account of his work.

4) Gumbel's letter to him of 1936 (50130).

Gumbel appends a list of participants in his project and the seven titles of their future contributions and again asks Einstein to submit a foreword. The titles include: Die Gleisschaltung der deutschen Wissenschaft; Finanzpolitik des Nationalsozialismus; Obituary of Emmy Nöther.

5) Gumbel's letter to him of 24 Jan. 1936 (50138).

Gumbel lists the seven authors adding that he hopes that about a dozen more will agree. All the authors are refugees from Germany, and among them is Schaxel (Moscow), see beginning of § 2.2.1.

6) Einstein's letter of 9 July 1936 (50139) to Gumbel; apparently his answer to a missing letter.

Ich kann mich nicht dazu entschließen, das gewünschte Vorwort zu schreiben, zumal ich die geplante Publikation für verfehlt halte. Eine derartige Publikation, welche so bunt gemischte Beiträge enthält, kann weder wirksam, noch finanziell erfolgreich sein.

7) Gumbel's letter to him of 25 April 1938 (53267).

Einstein's negative answer led Gumbel to change the plan of the proposed book. It will be a collection of contributions written by authors

Die von den Nazionalsozialisten auf ihrem Wissensgebiet erhobenen Forderungen zurückweisen. Insofern ist das Buch gleichzeitig bunt gemischt und doch einheitlich.

Once more, the extant correspondence is apparently incomplete; no answer from Einstein is available. Anyway, the book [28] appeared without Einstein's participation. Gumbel himself contributed an Introduction and wrote several pieces. There is also a section providing information about the authors,

Gumbel included (his biography and bibliography, on pp. 231 – 233).

One of Gumbel's notes entitled "Arische Mathematik" [28, pp. 218 – 221] is a non-mathematical review of the first two issues of *Deutsche Mathematik*.

Here is what he (p. 221) had to say about Einstein as pictured there:

Einstein spielt die Rolle des bösen Geistes. Sein Werk wird von einem Studenten [!] als "eine Kampfansage mit dem Ziel der Vernichtung der nordisch-germanischen Naturgefühl" bezeichnet.

At the same time, as Gumbel remarks, Jewish contributions are cited and generalized in the periodical and the original representation of the "Relativitätsprinzip" is [correctly] attributed to Einstein.

3.3. His Political Views. Over the years, Einstein made many attempts to help the victims of political oppression. In 1947 he (Sayen 1985, p. 207) wrote a letter to Stalin on behalf of Raoul Wallenberg and in 1950 he (Courtois et al 1997, p. 442) protested against the death sentence meted out to a Czech, Milada Horakova, on trumped-up political charges. For Einstein, his endeavours concerning Gumbel, although exceptionally numerous, were not therefore unusual.

During the 1920s – 1930s, Einstein (1960, pp. 194 – 199), together with likeminded intellectuals, had been striving to prevent war in Europe but he avoided anything that would support the Soviet regime;

he apparently knew the real situation in the Soviet Union. Even in 1928 he (Courtois et al 1997, p. 819) protested against an earlier trial there of the so-called Industrial Party. Then, in 1932, he (1960, p. 196) remarked that his close friend, Henri Barbusse, had he been a Soviet citizen, would have likely found himself in prison or in exile if left alive at all[54].

Nevertheless, Einstein (Ibidem, p. 334) believed that the Soviet Union laboured to promote international security; actually, did its damnedest to stir up world revolution. And, back in 1926, he praised Gumbel's essay [8], then not yet published, calling it objective (Jansen 1991, p. 84, without sufficient documentation).

Just the same, by the end of the 1940s he (letter of 1948, Sayen 1985, p. 112) explained away the Russian expansion into Eastern Europe and saw some "great merits" in the doings of the Soviet government, also see Item 8 below. In 1946, because of the threat of a new world war, Einstein (1960, p. 381) proposed to establish a single world government, but the Soviet authorities and obedient Soviet scholars rejected his (not really original) idea (Ibidem, pp. 444 – 450).

In a letter of 1953 Einstein (Sayen 1985, Chapter 17, Note 2) again condemned the Soviet and Czech political trials. Next year, however, in another letter, he (Ibidem, p. 210) stated that criticisms "cannot help" because "the Russians" will not hear them. He was patently wrong. In spite of permanent jamming, many Russians had by that time acquired the habit of listening to programs broadcast from abroad by several stations.

I continue with describing Einstein's archival materials concerned with Gumbel.

1) His letter of 28 Nov. 1930 (46526) to Radbruch partly quoted in § 3.1.1, No. 1.

Das Verhalten der akademischen Jugend gegen ihm [Gumbel] ist eines der traurigen Zeichen der Zeit, welche das Ideal der Gerechtigkeit, Toleranz und Wahrheit so wenig hochhält. Was soll aus einem Volke werden, dass solche Zeitgenossen brutal verfolgt und dessen Führer [Hindenburg] dem gemeinen Haufen keinen Widerstand entgegensetzen?

2) His letter of 3 Dec. 1930 (46524) to E. Lederer.

Es scheint, dass man in Deutschland dem Studententerror gegenübersteht wie einem Naturereignis. Der Balkan hat seine Grenzen westwärts verschoben[55] … Zum großen Teil beruht die Verblendung der Jugend auf einer in diesem Lande früher kultivierten, jetzt wenigstens geduldeten Glorifizierung des Militarismus und "Heldentums". Auch die Demokraten und Sozialisten machen in diesem gefährlichen Punkt Kompromisse und sehen nicht, dass sie an diesem Strick leicht aufgehängt werden können.

The last phrase was prophetic!

3) His letter of 25 March 1931 (46529) to Radbruch; see its beginning in Note 28 to § 2.2.2.

Gumbel's Buch [13] habe ich neulich zum Teil gelesen und aufs Neue den Mann, seine Intelligenz, seine noble Gesinnung und seine Energie bewundert. Es ist furchtbar, wie man die unerfahrene Jugend hier aus eigennützigen Beweggründen irreführt. Wenn es so weitergeht, werden wir über ein fasc[h]istisches Gewaltregime zum roten Terror kommen.

Einstein had not explained his last statement, but at least he correctly noted the similarity between Nazism and practical communism, as I would say.

4) His letter of the same date (46527) to Gumbel.

Ich habe neulich in Ihrem Buche [13] mit voller Bewunderung gelesen. Wie schrecklich wird doch die Jugend in diesem Lande irregeführt, aus wie niederen Motiven!

5) His letter of 9 July 1936 (50139) to Gumbel.

Ich finde, dass es sich in Amerika gut lebt und arbeitet. Ich habe seit Jahren nicht die Möglichkeit gehabt, so still und zurückgezogen zu leben.

Frankreich ist einstweilen der einzige Lichtblick, aber wie Lange? Wird Blum[56] wirklich genug sein, um mit seinen mächtigen und raffinierten Gegnern fertig zu werden?

6) His letter of 28 June 1952 (59894) to Gumbel.

Der Gedanke, einen solchen korporativen Brief einzusenden, hat etwelche Berechtigung. Der Haken liegt aber in Folgendem. Wenn der Brief ausschließlich oder hauptsächlich von Refugees unterzeichnet wird, also von Juden, dann werden die Gegner sagen, er komme von nicht objektiven Leuten. Wenn aber koschere Gojim mitmachen sollen, kann man sich schwer auf einen Text einigen.

Der vorgeschlagene Text ist meiner Absicht nach nicht gut. Das Hauptargument ist doch, dass die Remilitarisierung fast zwangsläufig zum Weltkriege führen muss. Aus diesem Grunde ist nach meiner Ansicht der Plan hier ursprünglich in Szene gesetzt worden. Heute aber, wo die Pleite in Korea etwas moderierend gewirkt haben dürfte, ist es schwer, einen honorigen Rückzug zu bewerkstelligen, nach der langen systematischen Hetze.

Wenn so ein Brief überhaupt inszeniert wird, muss James Warburg [57] genannt werden, der den Kampf sozusagen allein geführt und durch sehr gute Argumentation gestützt hat.

7) The response above was apparently occasioned by a draft (June 1952, 59895) of what likely became a letter co-authored by Gumbel, but not Einstein, and soon published in several American newspapers [22] which I have not seen. Here are a few extracts from the draft.

The rearming of Germany in any form will soon harm the interests of the United States. … The German masses are against re-militarization. … The militarists and the rightist elements would rather make an accord with the Soviets … The treaty[58] will strengthen Russian domination of Eastern Europe and Russian influence in the West.

So much for Gumbel's toying with communism!

8) Einstein's letter of 25 Nov. 1948 to Solovine (also see Note 56) apparently throws light on this issue.

… There are attempts to uphold "our" policy of bringing the Nazism back to power in Germany in order to use them against the wicked Russians. It is hard to believe that men learn so little from their toughest experiences.

Following his suggestion, I sent Hadamard a telegram to support opposition to the policy.

4. The Soviet Union: Facts and Impressions

During ca. 70 years, the Soviet regime either exterminated or indirectly brought to death 20 mln of its citizens (Courtous et al 1997, p. 14)[59]. No wonder that Upton Sinclair (1962, p. 305) in 1957 compared Stalin ("the Lenin of today", see Note 54) with "Tamerlane [Timur] or Genghis Khan, or any other of the wholesale slaughterers of history"[60]. Just one illustration (Solzhenitsin 1974, vol. 1, pt. 1, Chapter 11, p. 424): In 1932, six kolkhozniks (collective farmers) were executed for mowing the grass left round the tussocks after the harvesting of their kolkhoz' meadow. For this crime alone, the author concluded, Stalin should have been quartered.

Here are devastating descriptions of another kind. In very general terms Russell (1920a) condemned the communist regime; on p.114 he remarked that the adoption of the Bolshevik methods by the "Western nations" would result in a "relapse into the barbarism of the Dark Ages". He (1958, p. 110) "hated" Russia and he (1920b, p. 180) stated that the "better" Bolsheviks were endeavouring to "create a Plato's Republic", – a slave-owning society ruled by an elite[61]!

Gide (1936 – 1937) formulated many negative conclusions about what he saw in Russia; and in particular about the lack of political freedom (pp. 69 and 132 – 133). He (pp. 116 – 117) referred to Soviet newspapers listing astonishing setbacks in industry, mentioned the "new law" prohibiting abortions, terrible housing conditions and (pp. 194 – 195) scarcity and low quality of condoms and cited a local physician to the effect that "masturbation is practiced most generally"…

So why did many foreigners paint rosy pictures of the Soviet Union?

1) The difficult economic situation in the 1930s the world over; the dangers posed by Nazi Germany and its allies; and, later, Russia's part in winning World War II against them; and (§ 3.3) the threat of World War III, – all this contributed to distort the harsh reality.

2) Political blindness and/or premeditated deceit. In 1937, a French

newspaper (Courtois et al 1997, p. 324) mentioned Stalin's "monstrous deeds" and accused several men including Romain Rolland[62] and Paul Langevin (a friend of Einstein, cf. § 3.1.2, No. 5) of being "delighted" by the Soviet regime. In 1930 – 1951 Theodore Dreiser published about 35 papers and short notes in the Soviet Union (some of them translated from Western leftist periodicals) and constituting a volume of his works (1955). And Louis Aragon (1972), who was Stalin's henchman, pure and simple, contrived to omit any mention of communist atrocities.

Among those politically blind I cite Feuchtwanger (Note 54)[63] and Bernard Shaw. In 1921, the latter sent Lenin a complimentary copy of his book *Back to Methuselah* (published in 1921) with an inscription (translated back from the BS, 2nd edition, vol. 48, 1957, p. 159):

To Lenin, who, alone from among the statesmen of Europe, possesses the talent, the character, and the knowledge required of a man holding such a responsible position.

3) Superficiality. It was incumbent of any author to analyse beforehand the inferences formulated by his predecessors, the more so since some visitors to Russia doctored their accounts (Russell 1920a, p. 20), and, in addition, since they disagreed one with another (Zweig 1945, p. 308). Nevertheless, each author apparently only relied on his own impressions[64].

Then, visitors hardly realized that a positive conclusion should have been thoroughly checked rather than taken at face value. A similar statement was (and is) well known to statisticians, and I note that Einstein (1979, p. 19) made an analogous utterance with respect to experiments, but Gumbel obviously forgot this requirement. A special point here is that many Soviet citizens, especially before 1928, felt themselves like participants of a great mission (Zweig 1945, p. 305). Earlier Russell (1920a, p. 60) had denied this, but I myself heard similar statements from older men.

4) Propaganda. Year in and year out, the poverty-ridden and hungry nation spent a lot of money to keep communist parties abroad. At home, two events marked the beginning of the Great Terror: the appearance of a patriotic song that swept the country[65] and the adoption of a sham constitution.

The life of Maxim Gorky is highly relevant. From 1917 onward he managed to save the lives of many intellectuals, and he tried to defend national science and culture against the Bolsheviks (Vaksberg 1999). He also began to adapt himself to the Establishment but continued to be a meddler and in 1921 he was forced to emigrate (Ibidem, p. 48).

In 1928 Gorky visited the Soviet Union and next year he returned for good; in Europe, he only was a one-time writer whereas in Russia he remained a classic. During his last years, Gorky became the most authoritative propagandist of the Stalinist regime (below), but he was unable (to bring himself?) to write Stalin's biography (Ibidem, p. 263). Furthermore, The Great Leader and Teacher felt himself crowded by Gorky (Ibidem, p. 360) and in 1936 he was poisoned

(Ibidem, p. 374) 66 . I would add that with the Great Terror already underway, Gorky remained potentially dangerous.

In 1929 Gorky visited a labour camp and approved of the methods of re-educating the inmates, and a youngster, who dared tell him the truth, was immediately executed (Solzhenitsin 1974, vol. 2, pt. 3, Chapter 2). Then, without waiting for the (stipulated beforehand) verdict, Gorky (1930a, p. 3ff) condemned the defendants at a phoney trial in Moscow as guilty of high treason. He (Ibidem, p. 15) also blamed the kulaks for "organizing famine", cf. Note 38. On the same page he maintained that, *With the blessing of the head of the Christian Church* [?], European politicians *are preparing a marauding attack on the Union of Soviets*.

Soon Gorky (1932, p. 23) declared that the dictatorship of the proletariat [?] was temporary, necessary for "re-educating" tens of millions of people[67].

Actually, Gorky for a long time was experiencing hostile feelings with respect to his own people. Russians are "apathetic" (1922, p. 9) and "very fond of beating, no matter whom" (p. 20); "special cruelty" is in their nature (p. 17)[68]. And, just as the Jews who fled Egypt did not live to see the Promised Land, as Gorky (p. 43) finally declared, so also the

Semi-barbarian, stupid, difficult people in the Russian villages will die out … and a new generation will replace them.

Was not this idea formulated during his talks with leading Party figures?

I return now to Gumbel (§ 2). Recall that his last travel notes described the year 1932 so that he should have known enough. Nevertheless, he had not noticed the brutish nature of the Stalinist system; he either had not realized the essence, or had believed in the fairness of the trial, in 1928, of the Industrial Party, cf. Einstein's proper attitude (§ 3.3). Earlier he [2, p. 202] mentioned the "wilful sabotage" allegedly committed by intellectuals. But still, Gumbel surely heard truthful stories from his friends in Moscow. Even Zweig (1945), who only spent a fortnight in the Soviet Union (p. 302), discovered an anonymous note in his pocket explaining that Soviet citizens did not dare tell him their real opinions (p. 308).

Concerning his professional level, I do not believe that Gumbel managed, in 1932, to miss Kolman's notorious paper (1931), "Sabotage in science", appropriately published in the Party's leading organ, or that he knew nothing about the decimation of Soviet statisticians[69]. Again, did not he feel that a rigidly planned economy (§2.1) coupled with dictatorial rule had imposed great difficulties on the population (and led to falsification of statistical returns)?

Although he had made many interesting observations, Gumbel compiled a false account of the Soviet Union. As a finale, consider two of his statements taken together [19, p. 94; 8, p. 159], both of them describing the year 1926:

Ich fand Moskau zwar sehr interessant, aber ich wollte dort nicht mein Leben verbringen. Ich wusste nicht, was aus Russland unter Stalin werden würde …

A hundred million peasants are freed from the knout and millions of workers may look with proud hope on the first attempt at realizing sociallism [with a brutish face].

Serfdom was abolished in Russia in 1861 and workers looked in that manner only before ca. 1928.

Gumbel was lucky in that his later (in 1932) attempt to find a position in Moscow failed (Vogt 1991, p. 29), otherwise he would have likely perished, cf. Note 15, or at least been re-educated in the Gulag.

Notes

1. Gumbel began his academic career in Heidelberg in 1924 and only became außerordentlicher Professor in 1930 (Jansen 1991, pp. 385 and 387).

Here is a newspaper account (Anonymous 1931) of one of the pertinent episodes:

Prof. Gumbel sei von jungen Studenten in der übelsten un-akademischen Weise in seiner Lehrtätigkeit behindert worden … Prof. Albert Einstein mahnte, die inkriminierten [political] Bücher Gumbel zu lesen, er habe aus ihnen gelernt. Prof. Gumbel nannte den Kampf gegen ihn eines Kampf des Faschismus gegen die Republik.

A long Editorial (1931) which I also mention below was mostly devoted to defending Gumbel from the rightists. This proves that he was indeed one of their main opponents.

2. See § 3.3, No. 7 but especially [26].

3. The appearance of Gumbel's biography in their book certainly honoured his memory.

4. Pinl (1972) listed several of Gumbel's writings lacking in Jansen's bibliography.

5. I cite the letters by date and the provide five-digit numbers. In two cases, I mention the Pearson Papers kept at University College London.

6. Gumbel studied economics (Jansen 1991, p. 10). In 1923, Einstein (§ 3.1.2, No. 1) recommended him as an economist to a foreign university and in 1926 Gumbel read *Gastvorlesungen über Mathematik für Nationalökonomen* in Hamburg (Pinl 1972, p. 158).

7. A Professor der Rechts, and, at the time, the Reichsjustizminister. In Note 43 to § 2.4.2 I refer to one of his letters published in Bd. 18 (!) of his *Gesamtausgabe*. Below, I also mention Emil Lederer, a promi-nent economist (Jansen 1991, p. 18) and several mathematicians and physicists who are certainly remembered at least by the appropriate specialists.

8. See Note 1.

9. In 1924 Gumbel presided at a meeting commemorating the beginning of the world war and "in einem improvisierten Schluss-wort" recalled those perished: "Ich will nicht sagen – auf dem Felde der Unehre gefallen aber doch auf grässliche Weise ums Leben kamen" (Jansen 1991, p. 19). He used "diese Formel" once more in 1924 (Ibidem, p. 364; Note 107). In 1932, in another public speech, Gumbel (Ibidem, p. 35) proposed "als Denkmal des Krieges … eine große Kohlrübe" because in 1917/1918 swede had become the staple food for the Germans.

I also note that in 1927 Gumbel [8, p. 117) suggested that the "wahre Symbol" of Soviet Russia was not the Hammer and Sickle, but the bureaucrat's abacus. A bit later a Soviet citizen found guilty of suchlike blasphemy, even if whispered privately, would have landed in a labour camp.

10. The predecessor of the present *Max-Planck-Gesellschaft zur Förderung der Wissenschaften.*

11. He continued: “und er [der Weg] muss, wenn integral angewandt, dazu führen”!

12. Gumbel listed three reasons: the dissociation of those elected from the working population; the ideological influence of the capitalists; and the resistance of other institutions to the parliament. He failed to notice that under socialism the top people might be no less separated from the man in the street (Note 42) whose interests were hardly taken into account (Note 14).

In 1918, Gumbel [11, p. 194] thought that the transition to socialism should be achieved peacefully: “Schritt um Schritt baue man den Kapitalismus ab”.

13. Suchlike declarations are heard even now. The do-gooders still preach communism just like the believers in perpetual motion persisted in dreaming about the paradise they will be offering to mankind. Cf. Gorky’s warning (1930b, p. 3) addressed abroad: “You will also have to deal with traitors of the same brand”.

14. Anyway, the Soviet Union moved towards a planned economy suppressing its own New Economic Policy (§ 2.2.3). And experience showed that, apart from the impossibility of predicting the requirements for each commodity (including, for example, nails of every type and size) and the respective capacities, the plans were always geared to the needs of the state (as understood by the Party) rather than to the vital requirements of the population.

Horrible housing conditions in Moscow (Note 37) is an appropriate example. Late in life Gumbel [25, pp. 337 – 338] described the situation in the German Democratic Republic:

An einem Tag gibt es kaum Kartoffeln, aber Milch im Überfluss. An anderem Tag gibt es genug Kartoffeln, aber keine Milch.

15. Schaxel himself was invited by the Soviet Academy of Sciences and moved to Moscow. There, he came out against the notorious high-ranking humbug Lysenko, was imprisoned and then died, in 1943, “under obscure circumstances” (*Dictionary* 1983, p. 1026).

16. Later this institution was called the Marx – Engels – Lenin Institute, then Stalin’s name was added to it, – a fact shyly passed over in silence in the GSE (vol. 10, 1972/1976, pp. 301 – 302).

17. A petty mathematician and a diehard top communist (1892 – 1979) who eventually lost faith in the Soviet system and fled the country. Demidov &Tokareva (1995) published a letter of an eminent historian of mathematics, G. F. Rybkin, who edited Kolman’s manuscript of a booklet on Lobachevsky. He listed many glaring mistakes contained there and added that Kolman never blushed.

18. He repeated this statement twice: in the published text of the MSS (Marx 1983, p. 226) and in his last contribution (Kolman 1982, p. 172). In the later instance he, as noticed by Vogt (1991, p. 22), had shamelessly called Gumbel a “mediocre mathematician”. Vogt put on record some more information about Kolman; also see Vogt (1983). Thus, in 1931, at the International Congress of Mathematicians in Zurich, he reported on the preparation of the Marx MSS for publication without mentioning Gumbel.

19. The BSE (1st ed., vol. 19, 1930, p. 799) carried Gumbel’s biography. It described his scientific work and political activities in Germany and stated that “for some time” he had lived in Moscow “preparing Marx’ mathematical heritage for publication”. At the time, the Chief Editor of the BSE was Schmidt which likely explains why Gumbel was entered there, cf. beginning of § 2.2.2 and Note 22.

20. In a letter of 1901 to his father, an eminent statistician of the old, non-mathematical school, Chuprov (Sheynin 1990/2011, pp. 33 – 34) expressed his dissatisfaction with the “arithmetical manner of exposition” of vol. 2 of *Das Kapital*.

21. In 1881, Pearson thought about translating *Das Kapital* but it seems that Marx rejected his trial attempt (Porter 2004, p. 69ff). Pearson was critical of Bolshevism. He (1978, p. 243) remarked that [in 1924] Petersburg [actually, Petrograd] “has now for some inscrutable reason been given the name of the man who has practically ruined it”.

22. From 1927 (until?) he was member of the Presidium, and (from?) to 1930, head of its section on natural sciences; during 1939 – 1942, Vice-President of the Soviet Academy of Sciences.

23. His book [24] was translated into Russian in 1965. In the Foreword, B. V. Gnedenko properly stated that Gumbel had written the only monograph on its subject, which, moreover, will be easily understood by a broader circle of specialists, but that he had restricted his attention to studying independent trials. One of Gumbel's scientific papers [7] was translated in 1928 by Youshkevich who later became the most eminent Soviet historian of mathematics.

24. The present situation proves that Gumbel was wrong.

25. In a letter of 1915 to Markov, Chuprov (Sheynin 1996/2011, p. 130) remarked that "the figures now published by the Central Statistical Committee exaggerate the population [of Russia] by five if not ten million".

26. See p. 10 of the original Russian edition to which I refer when the pertinent statement is missing, or omitted in the German version (abridged by Jansen). The page numbers of the two versions greatly differ and it is not difficult to distinguish between them. Even when Gumbel foresaw that sex criminality will persist under socialism (p. 19), the Editor(s) of the Russian edition disagreed!

27. Gumbel [10] said a few words about the study of conjuncture made at Harvard University. Then, he published a short review [14] on Konjunkturforschung without however mentioning Kondratiev, see Note 29.

Elsewhere, he [15, p. 110] stated that Konjunkturkunde was a new statistical discipline.

28. On 25 March 1931 Einstein wrote two letters, one to Gumbel (46527), the other one, which I also quote in § 3.3, No. 1, to Radbruch (46529). In each of them, he stated that he was glad to have read the latter's article and in the second one he added:

Ich freue mich, dass in diesem Lande noch aufrechte und rechtliche Männer gibt, wie Sie einer sind. Ihr Artikel war mir eine wirkliche Freude.

The paper in question was likely Radbruch (1929 – 1930) where the author condemned political murders substantiated by la raison d'Etat. Einstein hardly knew about Gumbel's pertinent pronouncement to the contrary.

29. In 1922 Chuprov (Sheynin 1990/2011, p. 22) stated that

The intrinsic contradictions of capitalism are great and deep, but at present the ability to manage them is still greater.

In 1923, Kondratiev predicted the crisis of the capitalist system (although not its starting point). His fate was tragic (Ibidem, pp. 29 – 30). In 1952 Gumbel [20, p. 161] formulated another "fundamentale Frage":

Ob die neue Gesellschaft einen humanitären Sozialismus oder eine totalitäre und vielleicht sogar theokratische Struktur bringen wird. Die russische Regierung ähnelt heute der Ecclesia [general assembly] Militans … (Das älteste Beispiel für die Übereinstimmung beider Ziele war die kommunistische Regierung der Jesuiten in Paraguay.)

This passage is extremely interesting. First, it anticipated the idealistic phrase *Socialism with a humane face*. Second, in the 1980s, the eminent Soviet mathematician (and notorious anti-Semite) Shafarevich declared that socialism was defined by an appropriate ideology rather than by social ownership of the means of production. Accordingly, he argued that the Inca state (a slave-holding despotic state) was a socialist country.

30. Which does not really exist, as he himself stated on the same page! And how about the necessary conditions for the transition to socialism (§ 2.1) which were never fulfilled in Russia?

31. But was the civil war necessary? Also see § 2.3. In 1927 the GPU (more correctly, the OGPU) acquired the right to arrest and even to execute citizens without trial (Stetsovsky 1997, vol. 1, p. 244).

32. On p. 91 Gumbel mentioned the "present communist government" and added a curious remark: "so far as [it] … really has communist tendencies".

After Khroushchev, Soviet leaders hardly believed in a communist future. They kept pretending to their faith so as to continue in absolute power and instantly abandoned this attitude after the downfall of the Soviet Union.

33. This restriction was later abandoned.

34. Gumbel hardly realized that in 1921 – 1922 several thousand clergymen, monks and nuns of the Orthodox Church were executed on false charges, – alleged refusal to give up the Church valuables necessary for saving the starving population (Courtois et al 1997, p. 140ff), cf. Note 59. The BSE (1st ed., vol. 46, 1940, p. 665) even accused the Church of "espionage, treason and betrayal", although its later editions dropped this charge. The second antireligious wave occurred in 1929 – 1930; Flügge (1930) made public additional horrible facts concerning Mennonites and Baptists.

35. Zinoviev was expelled from the Party in 1927, 1932 and 1934 (he was twice re-admitted) and executed in 1936. Trotsky was exiled from the country in 1929 and assassinated by a Stalinist agent in Mexico in 1940. About 1934, Gumbel (Jansen 1991, p. 67) denounced Trotsky's exile.

36. Read: All issues are subordinated to Marxist philosophy. The attitude towards relativity theory was not at all established. For example, Kolman (1939) believed that velocities can exceed 300,000 km/sec.]Not exactly true]. The contrary statement, he declared, went against dialectical materialism. Then, a certain Vislobokov (1952), writing in a leading ideological journal, denied the theory. Even in the 1970s a (state) publishing house in Moscow rejected a manuscript describing Einstein's life and work, because, as the reviewer claimed, he was a Zionist. I heard about this from the author herself.

37. Was it so difficult to foresee the impending breakdown of the housing? The powers that were had hardly done anything at all not to mention that, in 1933 – 1934, because of their possible anti-Soviet inclinations, *undesirable elements* were forced to leave Moscow (60 thousand during two months of 1934) as well as several other cities (Courtois et al 1997, Chapter 9 of pt. 1).

Gumbel published photographs depicting the ugly conditions of housing in Moscow but did not dare disclose his authorship or even to let them appear in Germany (Jansen 1991, p. 16).

38. Gumbel believed, naively or otherwise, that young workers were being sent to rural areas "to examine the feelings" of the peasants rather than to organize a ruthless struggle against the kulaks. A few years later two million of these poor wretches were exiled and six million of peasants died of starvation (Courtois et al 1997, p. 164).

39. This would have been tantamount to intervention. Again, Gumbel's text hardly tallies with his belief [12, p. 174] in the sincerity of contemporary Russian proposals for disarmament.

40. Actually, the Soviet military force was not sufficient for preventing these nations from securing independence.

41. When comparing this statement with Gumbel's own previous report (§ 2.2.3) on the value of the rouble, it occurs that the Russian currency experienced a downfall which apparently meant that a large portion of the population was impoverished.

42. Their salaries were low as compared with their Western counterparts. However, fringe benefits had been (and still are) so diverse and considerable that the "poor top people" constitute an altogether separate population. Some time ago it became generally known that for several decades they had been buying foodstuffs (and other goods?) at prices existing in 1926. And some of them were even being serviced by clandestine state-maintained brothels.

43. Radbruch provided a related testimony. In a letter of 1949 to a certain Hugo Marx he (1995, p. 316) wrote:

Schrieb mir Gumbel über seine jetzige Ansicht vom Marxismus, sehr abgewogen Zustimmung und Kritik und ganz in dem mir richtig erscheinenden Sinne. Sogar er scheint weiser geworden zu sein.

44. He [21, p. 284] mentioned this fact already in 1952, although in passing. In 1925 he [5] did not say anything about it.

45. Gumbel mentioned Leonhard (1956). On p. 723 she cited Einstein's statement "kein Ziel ist so hoch dass es unwürdige Methoden rechtfertigen könnte" choosing it

as an epigraph to one of her chapters. Following a nasty tradition, she had not indicated the exact source. Bearing in mind Russian communists, she could have well written "… unwürdige [much less cannibalistic] Methoden …"

46. The handwritten draft of this letter has No. 46547 and Einstein wrote it beneath Montel's letter to him dated 5 Dec. 1931 (46546). Montel mentioned Gumbel and stated that "ce [?] serait pour lui naturellement la meilleure de recommandation". Montel's answer to letter 50110 (see §3.1.2, No. 5) had the letterhead *Ecole Municipale de Physique et de Chimie Industrielles* whose director was then Langevin, and Montel indeed mentioned him. He apparently substituted for Langevin.

47. Gumbel again informed Einstein about the German refugees in France on 10 Jan. and 18 Nov. 1935 (50134 and 50135).

48. Jansen (1991, p. 12) reported that in 1915/1916

Neben mathematischen und naturwissenschaftlichen Vorlesungen und Übungen, darunter auch die Einsteins [cf. the text of this letter 43810], hörte er [Gumbel] den bekannten und angefeindeten Pazifisten Georg Friedrich Nicolai.

Jansen added that Nicolai had written a foreword to one of Gumbel's political notes.

49. A copy of this letter is also kept among the Pearson Papers (709), but the words "um die Gerechtigkeit" inserted by hand are absent there.

50. *The National Union Catalog, Pre-1956 Imprints*, vols. 1 – 754, 1968 – 1981 (vol. 269, pp. 595 – 596) lists *Annual Reports* of this American-based Board for 1924/25 and 1925/26.

51. Gumbel [20] published an obituary notice of Bortkiewicz. I can now add that Mises left a manuscript on mathematics in Nazi Germany (Sheynin 2003).

52. See below.

53. In 1924 Gumbel addressed a French – German peace meeting (Note 9) and published an appropriate paper [3]. Also see [4].

54. Einstein kept Barbusse's portrait in his study "next to the portrait of my [of his] late mother" (Einstein 1922). Later Barbusse (1935, p. 312) stated that Stalin was "the Lenin of today". Yes, of course; and the next ones in line were Mao Zedong and Pol Pot!

After Barbusse's death Stalin sent his condolences to *L'Humanité* (BSE, 2nd ed., vol. 4, 1950, p. 235). Feuchtwanger (1937, p. 109) echoed Barbusse's maxim: *If Lenin had been the Caesar of the Soviet Union, then Stalin is their Augustus*. Cf. Gide (1936 – 1937, p. 69): *Stalin is the raison of everything.*

55. In 1929, after a coup d'état, a militaristic-monarchic dictatorship was established in Yugoslavia.

56. Léon Blum, the then Prime Minister of France. And here is Einstein's later statement (letter to Maurice Solovine, the translator of some of his contributions into French, of 23 Dec. 1938; Einstein 1993, p. 93):

France's betrayal of Spain and Czechoslovakia is frightful. The worst part is that the consequences will be deplorable.

57. During the 1930s – 1940s, James Paul Warburg published quite a few books on foreign relations.

58. Which one? NATO was established in 1949; the Bundesrepublik joined it in 1955.

59. Thus, in 1921 – 1922 more than five million died of starvation whereas grain had been sold abroad (Stetsovsky 1997, vol. 1, p. 28), – apparently, in part, to finance revolutionary movements worldwide.

60. Russell (1920a, p. 119) reasonably feared the "revival of Jenghis Khan and Timur".

61. Russell (1920a, p. 7) also believed that "Socialism is necessary for the world" and Gumbel (Russell 1917, p. 102n) thought that he might be called an "antibolshevistic communist".

62. The main text of Rolland (1935 – 1938) could have been meant.

63. Feuchtwanger essentially drew on his talks with Soviet leaders, Stalin included! He possibly felt an instinctive thirst for replacing reality by desire. On the other hand, I ought to add that his collected works were published soon afterwards.

Feuchwanger's book (1937) on Russia also appeared in a Russian translation although it contained some criticism of the Soviet regime. Strange as it may seem, I have it on good authority that those who discussed it in public were being imprisoned (and the translated book withdrawn from libraries).

64. I have not seen a single reference to Dostoevsky's *Besy* (1873; several English translations from 1931 onward entitled either *The Devils* or *The Possessed*; French and German translations made at the end of the 19th century). This is a prophetic and destructive criticism of revolutionists. Neither did I see any mention of Russell (beginning of § 4).

65. I quote its two lines: *There is no other nation/ Where a man is breathing as freely as here*.

66. Vaksberg has only partly documented his account. In this case hard evidence is lacking. On p. 376 the author maintained in passing that Wallenberg was poisoned as well.

67. On p. 11 he called Charles Chaplin "sentimental and dismal"! Chaplin's films with a happy end for the man in the street in a capitalist society, – this was, as I suspect, the real cause of Gorky's remark.

68. How can a cruel people re-educate tens of millions of their compatriots? Another statement seems, however, partly true: Not the "atrocities" of the leaders of the revolution, but the cruelty of the people was solely responsible for the post-revolutionary events (p. 41; Gorky's own inverted commas).

69. Here is a literal translation of a troglodyte's contented statement (Smit 1931, p. 4)*:The crowds of arrested saboteurs are full of statisticians*. In a few years she became Corresponding Member of the Soviet Academy of Sciences …

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Abbreviation: Jansen = Jansen (1991); PZM = Pod Znamenem Marksisma; R = in Russian

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