CORRESPONDENCE
REGARDING THE ART OF CONJECTURING

GOTTFRIED LEIBNIZ AND JAKOB BERNOULLI

Leibniz and Jakob Bernoulli discussed the theory of probability in their correspondence. Leibniz himself had written the treatise *de Arte combinatoria* in 1666 as boy. He knew about the works of Pascal and Huygens and, in fact, he knew Huygens personally. In the year 1676, he had visited Johannes Hudde in Amsterdam who, in turn, had also carried on correspondence with Huygens in 1665 regarding his (Huygen's) treatise. He was in the possession of de Witt's treatise concerning annuities. In addition, Caspar Neumann, deacon at St. Mary Magdeline in Breslau, Silesia, who examined the records which had been kept in Breslau concerning age, sex, year and month of death for many years, sent his observations to Gottfried Leibniz.

Leibniz provided the stimulus for the discussion in a letter written in April of 1703. It is likely that the version of the *Ars Conjectandi* which has come down to us was completed at this time.

Post Script to Letter XI from Leibniz to Bernoulli.
April, 1703 at Berlin. *Math. Schr.*[1] p. 71

"**P.S.**: I hear that the subject of estimating probabilities — which I consider important — has been not a little developed by you. I would like someone to treat mathematically the various kinds of games (in which there are beautiful examples of this subject.) This task would be both pleasant and useful, and it would not be unworthy of you or any very serious mathematician. I have seen some of your stated theses and only a few discussions of them. However, I would like to have them all."[2]

In the letter dated 3 October 1703, Jakob replied to Leibniz, giving a description of his work and the main proposition. In this letter he informed Leibniz that his theorem had been shown to his brother Johann twelve years previously. Jakob asked Leibniz for legal situations which would help in completing the work and also about the treatise of de Witt concerning annuities. Indeed, it will be seen that Jakob repeatedly insisted on obtaining a copy of de Witt's work for the purpose of obtaining statistics.

Date: Between April 1703 and April 1705.

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[1] *Translator's note*: These letters may be found in *Leibnizens Mathematische Schriften herausgegeben von C.I. Gerhardt.* Erste Abteilung, Band III, Hille.

[2] Translation by Bing Sung, *Translations from James Bernoulli*, Technical Report No. 2, Harvard University, Department of Statistics, 1966.

Extract from Letter XII from Jakob Bernoulli to Leibniz
3 October 1703 at Basel. *Math. Schr.* pp. 77-78.

"I would gladly like to know, most honorable sir, from whom you know that I have been working on the subject of estimating probabilities. It is true that for many years past I have taken much pleasure in explorations of this sort, since I scarcely think that anyone else has thought more than I about these matters. I even had a mind to write a tract about this subject; but I have often put it off for years at a time, because my natural laziness — which the weakness of my health, as an accomplice, has increased so much more — caused me to approach the writing very feebly. I often wish I had a secretary who could fully divine my thoughts when they were gently hinted to him, and could put them down in writing. Nevertheless, I have already completed the larger part of a book, but with an important part still missing, in which I show how to apply the principles of the art of estimation to civil, moral, and economic affairs. I will finish the book after I have solved a singular problem, which has a not small commendation of difficulty and a very large commendation of usefulness, and which has remained before my brother for twelve years, although he, when asked about the same problem some time ago by Marquis de l'Hospital, concealed the truth because of his eagerness to devalue my research. I will briefly tell you what the problem is: it is a known fact that the probability of any event depends on the number of possible outcomes with which the event can or cannot happen; and so, it occurred to me to ask why, for example, do we know with how much greater probability a seven rather than an eight will fall when we roll a pair of dice, and why indeed do we *not* know how much more probable it is for a young man of twenty years to survive an old man of sixty years than for an old man of sixty years to survive a young man of twenty years; this is the point: we know the number of possible ways in which a seven and in which an eight can fall when rolling dice, but we do not know the number of possible ways which prevail in summoning a young man to die before an old man, and which prevail in summoning an old man to die before a young man. I began to inquire whether what is hidden from us by chance *a priori* can at least be known by us *a posteriori* from an occurrence observed many times in similar cases — i.e., from an experiment performed on many pairs of young and old men. For had I observed it to have happened that a young man outlived his respective old man in one thousand cases, for example, and to have happened otherwise only five hundred times, I could safely enough conclude that it is twice as probable that a young man outlives an old man as it is that the latter outlives the former. Moreover, although — and this is amazing — even the stupidest man knows, by some instinct of nature *per se* and by no previous instruction, that the more observations there are, the less danger there is in straying from the mark, it requires not at all ordinary research to demonstrate this fact accurately and geometrically. But this is not all that I want: in addition, it must be inquired whether the probability of an accurate ratio increases steadily as the number of observations grows, so that finally the probability that I have found the true ratio rather than a false ratio exceeds any given probability; or whether each problem, so to speak, has an asymptote — that is, whether I shall finally reach some level of probability beyond which I cannot be more certain that I have detected the true ratio. For if the latter is true, we will be done with our attempt at finding out the number of possible outcomes through experiments; if the former is true, we will investigate the ratio between the numbers of possible outcomes *a posteriori* with as much certainty as if it were known to us *a priori*. And I have found the former condition is indeed the case; whence I can now determine how many trials must be set up so that it will be a hundred, a thousand, ten thousand, etc., times more probable (and finally, so that it will be morally certain) that the ratio between the numbers of possible outcomes which I obtain in this way is legitimate and genuine. The following suffices for practice in civil life: to formulate our conjectures in any situation that may occur no less scientifically than in game of chance; I think that all the wisdom of a politician lies in this alone. I do not know, most honorable sir, whether anything of substance appears to you to be in these speculations; in any case, you will make me grateful if you could supply me with any legal situations which you think could be usefully applied to these matters. Recently, I found that a certain tract which had been unknown to me was cited in the printed Monthly Excerpts of Hanover: Pensionarius de Wit's **von Subtiler Ausrechnung deſ valoriſ der Leib-Renten**. Perhaps he has something doing here; whatever it is, I would very much wish to obtain his source from somewhere."

Leibniz answered on 3 December 1703. In this letter he objected to the validity of probabilities computed *a posteriori*. He closed the letter with some remarks about de Witt's treatise.

Extract from Letter XIII to Jakob Bernoulli from Leibniz
3 December 1703[3] at Berlin. *Math. Schr.* pp. 83-84.

"The estimation of probabilities is extremely useful, although in several political and legal situations there is not much need for fine calculation as there is for the accurate recapitulation of all the circumstances. I remember learning for the first time *not* from your brother but from somewhere else that these matters had been dealt with by you. When we estimate empirically, by means of experiments, the probabilities of successes, you ask whether a perfect estimation can be finally obtained in this manner. You write that you have found this to be so. There appears to me to be a difficulty in this conclusion: that happenings which depend upon an infinite number of cases cannot be determined by a finite number of experiments; indeed, nature has her own habits, born from the return of causes, but only 'in general.' And so, who will say whether a subsequent experiment will not stray somewhat from the rule of all the preceding experiments, because of the very mutabilities of things? New diseases continually inundate the human race, but if you had performed as many experiments as you please on the nature of deaths, you have not on that account set up the boundaries of the world so that it cannot change in the future. When we investigate the path of a comet from any number of observations, we suppose that it is either a conic curve or another kind of simple curve. Given any number of points, an infinite number of curves can be found passing through them. Thus, I show the following: I postulate (and this can be demonstrated) that given any number of points, some regular curve can be found passing through these points. Let it be given that this curve has been found, and call it "A." Now, let another point be taken lying between the points given but outside of this curve; let a curve pass through this new point and the points given originally, according to the above postulate: this curve must be different from the

[3]Sylla claims this letter is dated 26 November 1703.

first curve, but nevertheless it passes through the same given points through which the first curve passes. And since a point can be varied an infinite number of times, there will also be an infinite number of these and other possible curves. Moreover, observed outcomes can be compared with these points, where the fixed underlying outcomes or their estimates inferred from observed outcomes, can be compared with the model curve. It may be added that, although a perfect estimation cannot be had empirically, an empirical estimate would nonetheless be useful and sufficient in practice. The person who composed the monthly Germanic excerpts of Hanover has been at my house. Pensionarius de Wit's article is flimsy when he uses that estimation known from the equal possibility of similar outcomes and hence shows that the problem of resurrections can be clearly solved by considering the fate of the Batavians.[4] And therefore, he has written in Belgic, so that he might appear to be on the same footing with the commoner."

Jakob wrote back on 20 April 1704. Here he responded to the objections of Leibniz. At the end he again spoke about the book of de Witt.

Extract from Letter XIV from Jakob Bernoulli to Leibniz
20 April 1704 at Basel. *Math. Schr.* pp. 87-89.

"Various questions about Certainty, Resurrections, Endowed Agreements, Conjectures,[5] and other matters show me that the subject of estimating probabilities in legal affairs requires not only the recapitulation of circumstances but also the same computation and calculation which we are accustomed to use in reckoning the outcomes of games of chance; I will show how to do this clearly for each situation. Moreover, the difficulty you found with my empirical method in determining the ratio between the numbers of possible outcomes requires more examples, not those in which it is impossible by any means to agree upon the numbers themselves, but rather those in which the numbers can be learned *a priori*. In addition, I said that I could, in these examples, provide for you a demonstration (which my brother saw twelve years ago and approved of). In order that you may really understand more clearly what I think, I give you an example: I place in an urn several hidden pebbles, black and white ones, and the number of white ones is twice the number of black ones; but you do not know this ratio, and you wish to determine it by experiment. And so, you draw one pebble out after another (replacing the pebble which you drew out in each single choice before you draw the next one, so that the number of pebbles in the urn is not diminished,) and you note whether you have picked a white or a black one. Now, I claim (assuming that you have two estimates of the two-to-one ratio which are, though quite close to one another, different, one being larger, the other being smaller — say 201 : 100 and 199 : 100) that I can determine scientifically the necessary number of observations so that with ten, a hundred, a thousand, etc. times more probability, the ratio of the number of drawings in which you choose a white pebble to the number of drawings in which you

[4] *Bing Sung's note: My interpretation of this is the following:*
Problem: What is the possibility of resurrection?
Solution: Look at the proportion of Batavians who have been resurrected. (De Wit himself was a Batavian.) [A Batavian is a Hollander –RJP.]

[5] *de Assecurationibus, de Reditibus ad vitam, de Pactis dotalibus, de Praesumptionibus*: Sylla renders these as Insurance, Annuities, Dowry contracts and Presumptions.—RJP

choose a black pebble will fall within, rather than outside of, these limits of the two-to-one ratio: 201 : 100 and 199 : 100; and so I claim that you can be morally certain the ratio obtained by experiment will come as close as you please to the true two-to-one ratio. But if now in place of the urn you substitute the human body of an old man or a young man, the human body which contains the tinder of sicknesses within itself as the urn contains pebbles, you can determine in the same way through observations how much nearer to death the one is than the other. It does no good to say that the number of sicknesses to which each is exposed is infinite; for let us grant this; it is nevertheless known that there are levels in infinity, and that the ratio of one infinity to another infinity is still a finite number, and can be expressed either precisely or sufficiently precisely for practical use. If sicknesses are multiplied by the passage of time, then new observations, in any case, must be set up: and it is certain that he who thinks that the investigations of our ancient forefathers concerning the end of life be settled by the daily customary observations made in London, Paris, or elsewhere will grossly err from the truth. The example of investigating the trajectory of a comet from several of its observed positions is, in this situation, almost apropos; I would never use it to demonstrate a proposition: although, in a limited way, I can find an application, since it cannot be denied that if five points have been observed, all of which are perceived to lie along a parabola, the notion of a parabola will be stronger than if only four points had been observed: for although there are an infinite number of curves which may pass through five points, there is nevertheless beyond this infinite number another infinite number — rather, an infinitely times more infinite number — of curves which may pass through only the first four points and not through the fifth point, all of which are excluded by this fifth observation. And yet, I admit that every conjecture which is deduced by observations of this sort would be quite flimsy and uncertain if it were not conceded that the curve sought is one of the class of simple curves; this indeed seems quite correct to me, since we see everywhere that nature follows the simplest paths. I perceive from your description that the Belgian tract of Jean de Wit contains such things which serve my point very well. And so I ask as strongly as possible that you, most honorable sir, send to me your copy of the book on any convenient occasion, since I have sought for it in vain in Amsterdam. I shall return it faithfully on the next market day in Frankfurt together with the fourth and fifth part of my publications concerning infinite series, the latter of which has been recently published and circulated." [6]

The next letters of Leibniz, possibly two or three, are lost. In the letter of 2 August 1704, Jakob again reminded Leibniz of the writing of de Witt. Jakob pointed out that there do exist ratios which cannot be determined in finitely many iterations, an example being the Ludolphine[7] constant .

[6] Translated by Bing Sung.

[7] Ludolph von Ceulen computed π to 20 digits in a paper written in 1596 by extending the method of Archimedes. He ultimately computed π to 35 digits.

Extract from Letter XV from Jakob Bernoulli to Leibniz
2 August 1704 at Basel. *Math. Schr.* pp. 91.

"I shall receive shortly from Father Varignon two copies of the *Histoire de l'Académie des Sciences* for the year 1701 that must be sent to you and to my brother: I will arrange to be added for you the fourth and fifth part of my theses on Infinite Series; conversely, I am expecting again on this market day from you the composition of the Pensioner de Witt, to which if only you are able to add what you have written formerly concerning agreements. I also would like that you make available to me any example of the conditional legacy; likewise you may show by an example anything you understand by way of annuities which are constituted on many lives; for not at all with study in judicial matters have I applied the mind according to the aforesaid. We can determine the ratio between the numbers of deaths although infinite by finite experiments but not precisely, but what amounts in practice is sufficient to be approaching constantly nearer until the error becomes insensible; with respect to which indeed in Geometry itself it is common, thus the ratio of the diameter to the circumference, although it cannot be determined precisely except through the Cyclic numbers of Ludolphus continued into infinity, nevertheless is fixed by Archimedes within the limits 7 : 22 and 71 : 223 sufficiently constricted to use. I exhibit a specimen of the art of conjecture in some games of chance, particularly regarding games of tennis, which I treat in detail; but in the majority of card games I do not advance, much less in games of draughts, on account of the immense variety of combinations, how many repeated throws of game stones they are able to undertake."

On 15 November 1704, Jakob complained that he awaited the copy of de Witt's treatise on market day in vain.

Extract from Letter XVI from Jakob Bernoulli to Leibniz
15 October 1704 at Basel. *Math. Schr.* pp. 92.

"Seeing that I understand from your last letters, my response to your previous letters has not been delivered to you, I send a copy of it with this. What concerns Mr. Hermann, he will respond to you himself on it. I expected your Mr. de Wit treatise from you on this market day, but in vain. Mr Mencke would be able perhaps to be commissioned at the moment of market day of Leipzig as intermediary of merchants."

Leibniz responded on 28 November that he possessed the short work of de Witt's concerning annuities, but could not find it.

Farther below Leibniz noted Jakob's comparison with the number π and objected to the analogy. While the Ludolphine method leads to π more and more closely, one certainly does not come nearer to the truth through increased observations in the case of illnesses.

Some remarks about different games follow, from those Leibniz, it seems to me, lets understand, that Jakob's attempt to illuminate everyday problems not only with healthy common sense, but also with the help of exact calculations, that he is not made particularly happy. Talented players, so Leibniz summarized, decide what is better, almost like what happens in the military or in the medical arts, in that they need considerations, this is more in many different ways than deep, whatever of the art own.

Extracts from Letter XVII to Jakob Bernoulli from Leibniz
28 November[8] 1704 at Berlin. *Math. Schr.* pp. 93-94.

"The dissertation of Pensioner Witt, or rather the printed paper concerning annuities on a life, reasonably brief, certainly exists among my books, but since I wished to send it to you, I have not yet found it. I shall nevertheless surrender the work when found, where in the first place it will be permitted to be brought to light being hidden somewhere at home. It contains nothing besides, which can be very new to you.

My double dissertation on the Conditions was printed by the Academy of Leipzig, if I myself remember well, in 1665. Two years after, corrected, it was refused, thus as certain others of my small juridical reflections through that of Nuremberg where I had permitted that it may go in peregrinations to Altorf, but the exemplars were lost and from this fact it is with difficulty that thereafter I have obtained one in Germany, reported by chance through a friend. I have the intention to prepare a new edition one day.

In certain things collected insufficiently (for our ability certainly) there is no certainty, with increased number of data just as with new years being added to the observations of death we approach nearer to mean truth in the whole, although good sense decrees things to be accepted so, but in fact in series, such as in the Ludolphine, by being continued the truth is always approached. In games of pure reason (just as chess and ramparts[9]) or in half chance, as of cards which Spanish men call *Hombre* or of games ⟨of dice⟩ which our conversations call (**Verkehren**) although it is not easily to define by calculation, by how much one more fitting of being about to be chosen than another is to hope of victory, for the most part yet they can be defined by reason whether how much it is more fitting it is permitted to judge what is right from data. Whence we see the clever gamesters just as in military or medical matters, what is more decided, of skills in considerations more multiplied than profound, because it is likewise of the art."

On 28 February 1705, Jakob reminded Leibniz again of the treatise of de Witt. Jakob ended the letter with an abrupt comment on the main proposition.

Extracts from Letter XVIII from Jakob Bernoulli to Leibniz
28 February 1705 at Basel. *Math. Schr.* pp. 95-98.

"You will receive immediately the *Histoire de l'Acadmie des Sciences de Paris* together with the fourth and fifth parts of the Propositions on Series included, the whole from Mr. Varignon himself, unless by chance you have already received them. I beg again you remember to pass to me the treatise of Mr. de Witt, if ever meanwhile it will have fallen into your hands; for no matter what it contains, they are not able to not be to me completely new things; for just as your, whatever you have published at some time, will always be highly desirable to me, if you deem

[8]*Translator's note*: Sylla claims October.
[9]*Translator's note*: The text reads "velut scaccorum et aggerum."

worthy to deliver of certain things to my possession; I have nothing of them except *de Arte combinatoria* and new *Hypothetical Physics*.

Because it considers the Appearances of Truth,[10] and of them the evidence on behalf of an evidently enlarged number of observations, the thing has itself entirely as I have written, and I am certain a pleasing demonstration to you , when I will have published."

In April 1705, Leibniz reported that he has not yet had the opportunity to locate de Witt's work. The end of the letter contains an indication that the book of de Witt is still promised:

Extracts from Letter XIX of Leibniz to Jakob Bernoulli
April 1705 at Hannover . *Math. Schr.* pp. 98-103.

"I count for nothing those of my writings, of which you said to me that you have, de Arte combinatoria and Hypothetical Physics; indeed it is nearly of naivetćomposed during the first youth of which the first will have appeared in 1666 and the last, I think, in 1670. These are in truth said diverse arguments of philosophy and of Mathesis that I have extracted from my journal. It was not yet permitted to seek the writing of Pensioner de Witt adequately among the books; I do not doubt nevertheless, but that I will be about to discover it at last, whenever there will be time. But scarcely anything new will occur to you in it, it stands with the same foundation everywhere, by which not only some learned men had used besides, but also Pascal on the Arithmetic Triangle, and Huygens in the dissertation on Chance, certainly as the arithmetic mean may be supposed among equally uncertain events; on which foundation now too countrymen enjoy, when they estimate the prices of farms, and managers of monetary affairs, when mean principles establish the revenues of prefectures, when a contractor offers himself.

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"And by this way furthermore the smaller packets are able to be procured by me, not always expected on market day. And by the same method you will receive the book of Witt from me, when it will be permitted to bring to light in the first place."

On 16 August 1705 Jakob Bernoulli died. We close with a letter to in which Leibniz makes two interesting remarks. First, he errors in thinking that the a twelve is as likely as eleven in the cast of two dice. Secondly, he takes credit for engaging Bernoulli in the pursuit of the study of probability.

Extract of a letter from Gottfried Leibniz to Louis Bourquet
22 March 1714 at Vienna
Die Philosphischen Schriften Band III, pp. 569–570

"The art of conjecturing is founded on that which is more or less easy, or else more or less feasible, because the Latin *facilis* derived by *faciendo* wishes to say *feasible* word by word: for example, with two dice, it is as feasible to cast twelve points, as to cast eleven, for both are able to be made only in one way alone; but it is three times

[10] *Translator's note*: "Verisimilitudo,"that is, Plausibility.

more feasible to case seven from them, because that is able to be made by casting 6 and 4, 5 and 2, and 4 and 3; and one combination here is as feasible as the other. The Chevalier de Méré (Author of the book of *Agrèments*) was the first who gave occasion to these meditations, that Messers Pascal, Fermat and Huygens pursued. Mr. the Pensioner de Wit and Mr. Hudde have also worked there above next. The late Mr. Bernoulli has cultivated this matter on my exhortations. One regards still the probabilities *a posteriori*, by experience, and one must have recourse to the error of the reasons *a priori*: for example, it is equally probable that the infant who must be born is a boy or girl, because the number of boys and the number of girls is found very nearly equal in this World. One is able to say that that which is done the most or the least is also the most or the least feasible in the present state of things, putting all the considerations together which must unite in the production of a fact."