Poincaré's Odds

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Laurent MAZLIAK
Laboratoire de Probabilités et Modèles Aléatoires
Université Pierre et Marie Curie
Paris, France

Abstract. This paper is devoted to Poincaré's work in probability. Though the subject does not represent a large part of the mathematician's achievements, it provides significant insight into the evolution of Poincaré's thought on several important matters such as the changes in physics implied by statistical mechanics and molecular theories. After having drawn the general historical context of this evolution, I focus on several important steps in Poincaré's texts dealing with probability theory, and eventually consider how his legacy was developed by the next generation.

Introduction

In 1906, Poincaré signed one of the most unusual texts of his scientific career [1], a report written for the Cour de Cassation in order to eventually close the Dreyfus case. In 1904, ten years after the arrest and the degradation of the unfortunate captain, the French government decided to bring an end to this lamentable story which had torn French society apart for years, and obtained the rehabilitation of the young officer who had been so unjustly martyred. As is well known, Dreyfus was convicted of treason in 1894 in the absence of any material proof, during a rushed and unbalanced trial orchestrated by the military hierarchy, whose aim was to identify a culprit as soon as possible. The only concrete document submitted as evidence by the prosecution was the famous *bordereau*, found in a wastepaper basket in the German Embassy in Paris and briefly scrutinized by several more or less competent experts. Among them, Alphonse Bertillon played a specially sinister role and became Dreyfus' most obstinate accuser. He built a bizarre, scientific-sounding theory of self-forgery of the *bordereau*, purporting to prove Dreyfus' guilt. Bertillon became trapped by his own theoretical construction, more because of overweening self-confidence and stupidity than by a real partisan spirit. When the Affair broke out at the end of the 1890s, and the political plot became obvious and Dreyfus' innocence apparent, Bertillon continued to elaborate his theory, rendering it more and more complex. This frenzy of elaboration brought him an avalanche of trouble, and nearly put an end to his career in the Paris police department. However, for the trial in the *Cour de Cassation*, in order to silence the last dissenting voices which might be raised, it was decided not to ignore Bertillon's rantings, but to ask incontestable academic authorities to give their opinion about the possible value of the self-proclaimed expert's conclusions. Three mathematicians were called for that purpose, Paul Appell, Gaston Darboux and Henri Poincaré, who jointly signed the report for the Cour de Cassation in 1906. Nevertheless, it was well known that

only the latter worked on the document, a tedious task Poincaré that undertook with integrity, but somewhat grudgingly. Besides, this was not Poincaré's first intervention in the Dreyfus case: during the judicial review of 1899 in Rennes of the decision of 1894, Painlevé read a letter of Poincaré criticizing Bertillon's work for its lack of scientific foundation.

This story is well known and has been narrated in detail many times ([52], [53], [81]) and I shall not dwell on it further. However, one may ask why it was Poincaré who was called upon to carry out this task. The first answer that comes to mind is the following: in 1906, Poincaré, who was then fifty-two years old, was without a doubt the most prominent among French scientists. He had, moreover, another characteristic: his name was familiar to a sizeable audience outside the scientific community. It was well known that he had been awarded the important prize of the Swedish king Oscar II in 1889; the publication of popular books on the philosophy of science increased his name recognition, and he was also famous due to several essays published in newspapers, for instance during the International Congress of Mathematicians in Paris in 1900, over which Poincaré presided. To seek out such a person in order to crush the insignificant but noisy Bertillon was therefore a logical calculation on the government's part. However, a second and more hidden reason probably played a part as well. The report for the *Cour de Cassation* [1] opens with a chapter whose title was quite original in the judicial litterature: *Notions on the probability of causes*; it contains a brief exposition of the principles of the Bayesian method. Bertillon had indeed pretended to build his system on the methods and results of the theory of probability, and answering him was only possible by confronting the so-called expert with his own weapons. It was therefore necessary not only to seek out a scientific celebrity for the job, but also someone whose authority in these matters could not be challenged. In 1906, Poincaré was regarded by everyone as the leading specialist in the mathematics of randomness in France. While by then he was no longer the Sorbonne Chair of Calculus of Probability and Mathematical Physics, he had held the Chair for some ten years (it was in fact the first position he obtained in Paris), had accomplished an impressive amount of work in mathematical physics during this period (we shall return at length to this subject later), and had published in 1896, shortly before leaving the chair, a treatise on the Calculus of Probability which, in 1906, was still the main textbook on the subject in French. Moreover, several texts had presented his thoughts on the presence of randomness in modern physics to a large cultivated audience, in particular his *Science and Hypothesis* ([71]), which went through several editions. It was therefore as a specialist in the calculus of probability that Poincaré was sought out by the judicial authorities, in order to dispose of the Dreyfus case once and for all.

If we go back in time fifteen years before this event, we notice a striking contrast. Since 1886, although Poincaré held the aforementioned Sorbonne chair, he had probably mainly seen in the chair's title the words *Mathematical Physics*. In 1892, he published an important textbook on Thermodynamics [64] based on the lectures he had read at the Sorbonne several years before. A Poincaré publication would of course not go unnoticed, and one attentive reader had been the British physicist Peter Guthrie Tait (1831-1901). Tait had been very close to Maxwell and was one of the most enthusiastic followers of his work. He wrote a review of Poincaré's book for the journal *Nature* ([87]); the review was quite negative despite the obvious talent Tait recognized in his young French colleague.

Poincaré, wrote Tait, introduced beautiful and complex mathematical theories in his textbook but often to the detriment of the physical meaning of the situations he studied. The most important reproach of the British physicist was the fact that Poincaré remained absolutely silent about the statistical theories of thermodynamics, leaving in the shadows the works of Tait's friend and master Maxwell. Tait wrote:

> 'But the most unsatisfactory part of the whole work is, it seems to us, the entire ignoration of the true (i.e. the statistical) basis of the Second Law of Thermodynamics. According to Clerk-Maxwell (Nature, xvii. 278) "The touch-stone of a treatise on Thermodynamics is what is called the Second Law." We need not quote the very clear statement which follows this, as it is probably accessible to all our readers. It certainly has not much resemblance to what will be found on the point in M. Poincaré's work: so little, indeed, that if we were to judge by these two writings alone it would appear that, with the exception of the portion treated in the recent investigations of v. Helmholtz, the science had been retrograding, certainly not advancing, for the last twenty years.'

Poincaré wrote an answer and sent it to *Nature* on 24 February 1892. It was followed, during the first semester of 1892, by six other letters between Tait and Poincaré[1]; they are rather sharp, each sticking to his position. On March 17th, Poincaré wrote the following comment about the major criticism made by Tait:

> 'I left completely aside a mechanical explanation of the principle of Clausius that M. Tait calls "the true (i.e. statistical) basis of the Second Law of Thermodynamics." I did not speak about this explanation, which besides seems to me rather unsatisfactory, because I desired to remain absolutely outside all the molecular hypotheses, as ingenious they may be; and in particular I said nothing about the kinetic theory of gases.'[2]

One thus observes that Poincaré, in 1892, had a very negative view of statistical mechanics, the very science of matter in which probabilities played the greatest role. However, Poincaré would not have been Poincaré if, once shown a difficulty, he did not take the bull by the horns and to try to tame it. A first decision was the resolution, during the next academic year, 1893, to lecture on the kinetic theory of gases. And indeed, in the Sorbonne syllabus for that year, we see that Poincaré had transformed his lectures into *Thermodynamics and the kinetic theory of gases*.

In 1894 Poincaré's first published paper on the kinetic theory of gases [67], to which we shall return later. If one still observes a good deal of skepticism in it, or at least reservation about these novelties in randomness, a change of attitude was taking place. This same academic year 1893-1894, Poincaré eventually decided for the first time to teach a course on the calculus of probability at the Sorbonne, which subsequently became the basis of his book of 1896, the same year when he exchanged

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[1] Accessible freely on the website of the Archives Poincaré in Nancy at the address http://www.univ-nancy2.fr/poincare/chp/hpcoalpha.xml

[2] 'J'ai laissé complètement de côté une explication mécanique du principe de Clausius que M. Tait appelle "the true (i.e. statistical) basis of the Second Law of Thermodynamics." Je n'ai pas parlé de cette explication, qui me paraît d'ailleurs assez peu satisfaisante, parce que je désirais rester complètement en dehors de toutes les hypothèses moléculaires quelque ingénieuses qu'elles puissent être; et en particulier j'ai passé sous silence la théorie cinétique des gaz'.

his chair for that of celestial mechanics. Moreover, in the following years, reflections on the mathematics of randomness became more frequent in his writings, up to the publication of his more philosophical works, which acknowledged the integration of the theory of probability among Poincaré's mathematical tools. By 1906, as already noted, the transition had been completed, especially as new elements, such as Einstein's just published theory of Brownian motion, made even more necessary the increasing presence of probability in scientific theories.

The present paper concerns the probabilistic aspects of Poincaré's enormous production, aspects of which remain somewhat limited in size. A non-negligible challenge in dealing with this subject is that probability penetrated Poincaré's work almost by force, forcing his hand several times, his main achievement consisting in building dykes so that the mathematician might venture with a dry foot on these rather soft lands. We shall see later that his successor Borel had a somewhat different attitude toward choosing to apply probability theory in many domains, reflecting the fact that Borel had encountered probability in a more spectacular way than Poincaré.

Despite this limited contribution, Poincaré succeeded in leaving a significant heritage which would later prove important. Above all, his most decisive influence may have been to allow probability theory to regain its prestige in France again, after occupying a rather miserable position in the French academic world for more than half a century.

The aim of the present paper is therefore threefold, and this is reflected in its three parts. In the first part, we focus on Poincaré's evolution in the fifteen years we have just pointed to, in order to define how this progressive taming of the mathematics of randomness occurred. In the second section, I examine in detail several of Poincaré's works involving probability considerations in order to give an idea not only of Poincaré's style, but also of the intellectual basis for the evolution in his thinking. And finally, in the last section, I comment on the persons who recovered the heritage of Poincaré in this area, and on how they extended it.

Many of the topics discussed in this paper have been covered previously. In particular, I shall refer several times to Sheynin's text ([84]), to von Plato's publications ([90], [91]) as well as to several papers by Bru, some published and some not ([20], [23]). Let us me also mention the nice book [36] where the author deals with Poincaré's works on probability with an epistemological approach.

Acknowledgement

may remain in the text. After the presentation of this paper at the Institut Henri Poincaré on 24 November 2012, I had an interesting echange with Persi Diaconis; I hope he will find the paper improved by his comments. And finally, I feel it necessary to say how much Bernard Bru would have deserved to write this chapter. I dedicate this work with gratitude to the memory of an honest man, our master and friend Marc Barbut, whom we miss so much since a cold morning of December 2011...

1   First part: the discovery of probability

In the beginning was the Chair. For, as we will see, it was not only by a fortuitous scientific interest that Poincaré first came to be involved in probability theory, but there was also a very specific academic situation in which Hermite, the major mathematical authority in 1880s France, crusaded for the career of his three *mathematical stars*: Paul Appell (his nephew), Émile Picard (his son-in-law) and of course Henri Poincaré, the latter considered by him to be the most brilliant, though he did not belong to his family (to the great displeasure, so he wrote to Mittag-Leffler, of Madame Hermite). A remarkable change took place at the Sorbonne at this time when, in just a few years, almost every holder of the existing chairs in mathematics and physics died: Liouville and Briot in 1882, Puiseux in 1883, Bouquet and Desains in 1885, Jamin in 1886, leaving an open field for a spectacular change in the professorships. At the end of the whirlwind, the average age of the professors of mathematics and physics at the Sorbonne had been reduced by eighteen years! It was therefore clear for everyone, and first of all Hermite, that there was a risk that the situation might become fixed for a long time, and that it was therefore necessary to act swiftly and resolutely in favor of his protégés. The three of them were, indeed, appointed in Paris during these years. This episode is narrated in detail in [2] and I here touch only upon the most important aspects insofar as they relate to our story.

The Chair of Calculus of Probability and Mathematical Physics, occupied until 1882 by Briot, had been created some thirty years before, after numerous unsuccessful attempts by Poisson, but in a form different than the one pursued by the latter. The joining of mathematical physics to probability had been decided in order to temper the bad reputation of the theory of probability in the 1840s in France, notably due to some of the work of Laplace and, above all, of Poisson himself dealing with the application of probability in the judicial domain. The book of Poisson [79] ignited a dispute in the Academy in 1836, when Dupin and Poinsot harshly contested Poisson's conclusions, and the philosophers led by Victor Cousin made a scene *in the name of the sacred rights of liberty against the claims advanced by the mathematicians in order to explain how events occur in social issues*. John Stuart Mill summed up the situation by qualifying the application of probability to judicial problems as the *scandal of mathematics*. This polemical contretemps tarred the calculus of probability in France, leaving it with a most dubious reputation.

Although Lippmann was nominated in 1885 for the chair, Jamin's unexpected demise freed the chair of Research in physics of which Lippmann took hold immediately, leaving a place for Poincaré. One might initially be surprised by this choice. First, because Poincaré was preferred to other candidates, of course less gifted, but whose area of research was closer to that of the position. One would rather have expected to see Poincaré in one of the chairs in Analysis, for instance. In 1882, when Briot died and the great game began, Poincaré had no published work in physics

(let alone the calculus of probability). And yet, among the candidates who were eliminated, there was for instance Boussinesq who had to his credit significant contributions in that domain, and who openly stated his intention of revitalizing the teaching of probability which seemed to him to be in a poor state. Not without irony, Boussinesq later became Poincaré's successor in this same chair when the latter changed over to the Chair of Celestial Mechanics.

Thus Poincaré was nominated in 1886, without any real title for the position, and one may think that it had really been chance which led him there. However, as shown in Atten's fine analysis [2], a number of indications demonstrate that Poincaré had indeed really desired this particular position. Examining his lectures in 1887-1888 shows that he already had a profound knowledge of physical theories and, moreover, several passages in his correspondence at this time show his sustained interest in questions of physics. This shows that his attitude had not been purely opportunistic and that the chair pleased him. Also, Hermite, apart from the desire of supporting his protégé, seems to have made a thoughtful bet in nominating him for this somewhat unexpected position. Knowing Poincaré's acute mind, it was not unreasonable to anticipate spectacular achievements by him in this position. What followed, as we know, bore out Hermite's expectation. . .

The theory of probability, as I said, did not seem to concern the new professor at the beginning of his tenure, and he taught courses on several different physical theories. In 1892, he published his lectures on thermodynamics, delivered in 1888-1889 [64], producing a book, as noted earlier, that was sharply criticized by Tait. Poincaré therefore decided to look into the questions raised by the kinetic theory of gases, in particular because he had just read a communication by Lord Kelvin to the Royal Society containing several fundamental criticisms on Maxwell's theory [47]. Perhaps Poincaré had been especially eager to read this paper because it might provide a powerful argument in his controversy with Tait. However, the affair took another direction, revealing the mathematician's profound scientific honesty. At the beginning of the paper [67], published in 1894, though he again expressed some skepticism, Poincaré, whose conventionalism was formed during these years, seemed already half convinced of the possible fecundity of Maxwell's theory.

> 'Does this theory deserve the efforts the English devoted to it? One may sometimes ask the question; I doubt that, right now, it may explain all the facts we know. But the question is not to know if it is true; this word does not have any meaning when this kind of theory is concerned. The question is to know whether its fecundity is exhausted or if it can still help to make discoveries. And admittedly we cannot forget that it was useful to M. Crookes in his research on radiant matter and also to the inventors of the osmotic pressure. One can therefore still make use of the kinetic hypothesis, as long as one is not fooled by it.'[3]

We shall come back in the second section to this article of 1894 and to the new formulation of the ergodic principle it contains, in which Poincaré introduced the

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[3]'Cette théorie mérite-t-elle les efforts que les Anglais y ont consacrés ? On peut quelquefois se le demander; je doute que, dès à présent, elle puisse rendre compte de tous les faits connus. Mais il ne s'agit pas de savoir si elle est vraie; ce mot en ce qui concerne une théorie de ce genre n'a aucun sens. Il s'agit de savoir si sa fécondité est épuisée ou si elle peut encore aider à faire des découvertes. Or, on ne saurait oublier qu'elle a été utile à M. Crookes dans ses travaux sur la matière radiante ainsi qu'aux inventeurs de la pression osmotique. On peut donc encore se servir de l'hypothèse cinétique, pourvu qu'on n'en soit pas dupe.' ([67], p.513)

restriction of exceptional initial states. Let us only mention here the following point: Poincaré seemed to have found this idea in his previous work on the three body problem for which he had obtained the prize of the King of Sweden in 1889. In the memoir presented for the prize, he had indeed proved a recurrence theorem concerning the existence of trajectories such that the system comes back an infinite number of times in any region of the space, no matter how small. The next year, for his paper in *Acta Mathematica*, Poincaré added a probabilistic extension of his theorem where he showed that the set of initial conditions for which the trajectories come back only a finite number of times in the selected region has probability zero ([62], p.71-72); this passage was included some years later in his treatise of new methods for celestial mechanics [65] in a section simply called 'Probabilities'.

During the academic year 1893-1894, Poincaré prepared his first course of probability for his students of mathematical physics ([69]), published in 1896 and transcribed by Albert Quiquet, a former student of the École Normale Supérieure who entered the institution in 1883, before becoming an actuary, and who had probably been an attentive listener of the master's voice. In the manner of the standard probability textbooks of the time, it does not present a unified theoretical body but rather a series of questions that Poincaré tried to answer (the main ones, which occupy the bulk of the volume, concern the theory of errors of measurement - a subject we shall return to in the next section). The book, in its edition of 1896, is a natural successor to Bertrand's textbook [9], which up to then was the standard textbook (see [21]). In comparison to Bertrand's book, Poincaré's consolidates the material in several interesting ways, and this aspect is even more obvious in the second edition [77], completed by Poincaré some months before his death in July, 1912.

The mathematician seemed now convinced that it was no longer possible to get rid of probability altogether in science, so he decided instead to make the theory as acceptable as possible to the scientist. Poincaré decided to devote considerable effort towards that end, especially by writing several texts lying half-way between popularization (with the meaning of writing a description of several modern concepts using as little technical jargon as possible) and innovation. Two texts are of particular importance: that of 1899 ([70]) - reprinted as a chapter of [71] - and that of 1907 ([75]) - reprinted again as [76] and then as a preface to the second edition of his textbook [77] - two texts marking Poincaré's desire to show off his new probabilistic credo. But one has to realize that Poincaré wanted to convince himself above all and this leads one to ask the question Jean-Paul Pier ironically used as the title for his paper [61]: did Poincaré believe in the calculus of probability or not? Without pretending to give a final answer, one may however observe that Poincaré very honestly sought for a demarcation of the zone where it seemed to him that using probability theory did not create a major problem. Hence the attempt to tackle some fundamental questions in order to go beyond the defects that Bertrand had ironically illustrated with his famous paradoxes: When is it legitimate to let randomness intervene? What definition can be given of probability? Which mathematical techniques can be developed in order to obtain useful tools for physics, in particular for the kinetic theory of gases? Borel, as we shall see in the third section, would later remember Poincaré's position.

In his 1907 text, Poincaré accurately defined the legitimate way to call upon the notion of randomness. He saw essentially three origins for randomness: the ignorance of a very small cause that we cannot know but which produces a very important

effect (such as the so-called Butterfly Effect), the complexity of the causes which prevents us from giving any explanation other than a statistical one (as in the kinetic theory of gases), the intervention of an unexpected cause that we have neglected. This was not too far from the Laplacian conception, which should not surprise us very much since Poincaré, born in 1854, was a child of a century for which Laplace was a tutelary figure. However, Poincaré knew well the accusations accumulated against Laplace's theory and he proposed several ways to adjust it: randomness, even if it is connected to our ignorance to a certain extent, is not only that, and it is important to define the nature of the connection between randomness and ignorance. The conventionalist posture on which we have already commented naturally made things easier, but Poincaré did not seek facility. As he wrote in 1899:

> 'How shall we know that two possible cases are equally probable? Will it be by virtue of a convention? If we state an explicit convention at the beginning of each problem, everything will be fine; we will just apply the rules of arithmetic and algebra, and pursue the computation to the end, such that our result leaves no room for doubt. But, if we want to apply it anywhere, we will have to prove the legitimacy of our convention, and we will find ourselves in front of the difficulty we thought we had eluded.'[4]

In a remarkable creative achievement, Poincaré forged a method allowing the objectification of some probabilities. Using it, if one considers for instance a casino roulette with alternate black and red sectors, even without having the slightest idea of how it is put into motion, one may show it is reasonable to suppose that after a large number of turns, the probability that the ball stops in a red zone (or a black zone) is equal to 1/2. There are thus situations where one can go beyond the hazy Laplacian principle of (in)sufficient reason as a necessary convention to fix the value of the probability. The profound *method of arbitrary functions*, which is based on the hypothesis that at the initial time the distribution of the position where the ball stops is arbitrary and shows that this distribution reaches an asymptotic equilibrium and tends towards the uniform distribution, was certainly Poincaré's most important invention in the domain of probability and we shall see later the spectacular course it took.

To conclude this survey, let us mention that in 1906, when Poincaré was completing his report for the Cour de Cassation in the Dreyfus case, despite his place as the pre-eminent French authority in the theory of probability, he was, as far as his scientific thought was concerned, somewhat in the middle of the ford between a completely deterministic description of the world and our modern conceptions where randomness enters as a fundamental ingredient. Poincaré kept this uncomfortable position until the end of his life. It is besides noticeable that at the precise moment when Borel was - so strikingly - taking over, Poincaré did not seem to have been particularly interested in the enterprise of his young follower. He showed similarly disinterest for the fortunate experiments of the unfortunate Bachelier: he had written, it is true, a benevolent report on his thesis [3] and had sometimes helped him to obtain grants, but the two men had no further scientific contact [29]. Even more

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[4]'Comment saurons nous que deux cas possibles sont également probables? Sera-ce par une convention? Si nous plaçons au début de chaque problème une convention explicite, tout ira bien; nous n'aurons plus qu'à appliquer les règles de l'arithmétique et de l'algèbre et nous irons jusqu'au bout du calcul sans que notre résultat puisse laisser place au doute. Mais, si nous voulons en faire la moindre application, il faudra démontrer que notre convention était légitime, et nous nous retrouverons en face de la difficulté que nous avions cru éluder.' ([70], p.262)

suprising, Poincaré seems to have thoroughly ignored the Russian school's works (Chebyshev, Markov, Lyapounov...) and this explains why he became never conscious of some connections with his own works. Poincaré's probabilistic studies leave therefore a feeling of incompleteness, partly due probably to his premature death at the age of 58, but also to the singular situation of this last giant of Newtonian-Laplacian science who remained on the threshold of upheavals that arrived after his death.

2   Second part: construction of a probabilistic approach

In this second part, I would like to present some steps which have marked the progressive entry of questions on probability in Poincaré's works. Even if it is not possible to see a perfect continuity in the chain of his research, a kind of genealogy can be traced which allows one to better understand how the mathematician gradually adopted a probabilistic point of view in several situations. I feel a need to mention that my aim in this section is to describe Poincaré's contribution and not necessarily to comment its originality. A particular problem with our hero is that he practically never quotes his sources so that it is difficult to make a statement about what he had read or not. I have tried as much as was possible to make each subsection of this part independent of the others, which may sometimes result in brief repetition.

2.1   The recurrence theorem and its 'probabilistic' extension

In anticipation of the sixtieth birthday of King Oscar II of Sweden in 1889, a mathematical competition was organized by Mittag-Leffler. The subject concerned the three-body problem: Was the system Earth-Moon-Sun stable? Periodic? Organized so that it will always remain in a finite zone of space? Many of these questions were fundamental ones that had challenged Newtonian mechanics from the 18th century. Poincaré submitted in 1888 an impressive memoir, immediately selected for the award by a jury including Weierstrass, Mittag-Leffler and Hermite. While correcting the proofs of the paper for *Acta Mathematica*, Phragmen located a mistake, leading Poincaré to make numerous amendments before resubmitting a lengthy paper the following year, published in volume 13 of *Acta Mathematica*.

This story, well known and well documented (see in particular [7]), is of interest for us only as far as one difference between the version submitted for the prize and the published version in 1890 is the appearance of the word *probability*, certainly for the first time in the French mathematician's works. I shall closely follow Bru's investigation [23] on the way in which countable operations gradually established themselves in the mathematics of randomness.

In the first part of the memoir submitted for the competition, Poincaré studied the implications of the existence of integral invariants on the behavior of dynamical systems. A simple example of this situation is given by the case of an incompressible flow for which the shape of the set of molecules changes, but not its volume, which remains constant in time. However, Poincaré had in view a much more general situation. He showed in fact on p.46 *and seq.* of [62] that with a proper choice of coordinates in the phase space of position and velocity, the general situation of a mechanical system may be expressed by such an integral invariant. Poincaré then

expounded a version of his recurrence theorem in the following form: let E be a bounded portion of the space, composed of mobile points following the equations of mechanics, so that the total volume remains invariant in time. Let us suppose, writes Poincaré, that the mobile points remain always in E. Then, if one considers r_0 a region of E, no matter however small it may be, there will be some trajectories which will enter it an infinite number of times.

Poincaré's proof is a model of ingenuity and simplicity. One discretizes time with a step of amplitude τ. Let us call with Poincaré

$$r_1, r_2, \ldots, r_n, \ldots$$

the "consequents" of r_0, that is to say, the successive positions of the different points of the region r_0 at times

$$\tau, 2\tau, \ldots, n\tau, \ldots$$

In the same way, the "antecedent" of a region is the region of which it is the immediate consequent. Each region r_i has the same volume; as they remain by hypothesis inside a bounded zone, some of them necessarily intersect. Let two such regions be r_p and r_q with $p < q$, with intersection the region s_1 having nonzero volume: a point starting in s_1 will be back in s_1 at time $(q-p)\tau$. Going back in time, let us call r_0^1 the sub-region of r_0 whose p-th consequent is s_1. A particle starting from r_0^1 will again enter this region at time $(q-p)\tau$. We now start again the process by replacing r_0 by r_0^1, and thus build a decreasing sequence (r_0^n) of sub-regions of r_0 such that each point starting from r_0^n returns n times at least. Considering a point in the intersection of the r_0^n (whose non-emptiness is taken for granted by Poincaré), a trajectory starting from such a point will pass an infinite number of times through r_0.

In this form, the theorem is therefore completely deterministic. What then compelled Poincaré to believe it necessary, in the new version published in 1890, to rewrite his result in a probabilistic setting ([62], pp.71-72)? At first glance indeed, the appearance of the word probability in these pages may seem surprising in the chosen framework of the mechanics of Newton, Laplace and Hamilton. In fact, as Bru remarks ([23]), one must not be misled by this use of probability by Poincaré, viewing it as a sudden revelation of the presence of randomness having the ontic value we spontaneously give it today. Poincaré himself wrote: *je me propose maintenant d'expliquer pourquoi [les trajectoires non récurrentes] peuvent être regardées comme exceptionnelles.* What Poincaré was thus looking for was a convenient way of expressing the rarity, the thinness of a set. He was writing before the decisive creation of the measure theoretic tools and particularly of Borel's thesis which would, four years later, prove that a countable set has a measure equal to zero. For a long while, astronomers in particular employed the concept of probability with the meaning of practical rarity and certainly one should not seek for a more sophisticated explanation to justify the presence of the word coming from Poincaré's pen. It was a convenient way of speaking, whose aim was almost entirely to hide the *obscure instinct* mentioned by Poincaré in his 1899 paper ([70], p.262), almost as an apology, because we cannot do without it if we want to do scientific work.

Poincaré begins by expounding the following 'definition': if one calls p_0 the probability that the considered mobile point starts from a region r_0 with volume v_0 and p_0' the probability that it starts from another region r_0' with volume v_0', then

$$\frac{p_0}{p_0'} = \frac{v_0}{v_0'}.$$

In particular, if r_0 is a region with volume v, used as a reference, the probability that the mobile point starting from r_0 starts from a sub-region σ_0 with volume w is given by $\dfrac{w}{rv}$. Equipped with this notion, the mathematician wants to prove that the initial conditions in r_0 such that the trajectory does not reenter r_0 more than k times form a set with probability zero, no matter how large the integer k.

Earlier in his paper, Poincaré had proved that if $r_0, \ldots, r_{n-1}$ were n regions with the same volume v included in a common region with volume V, and if $nv > kV$, then it was necessary that there were at least $k + 1$ regions whose intersection was nonempty. Indeed, if one supposes that all the intersections taken $k + 1$ by $k + 1$ were empty, one may write (in modern notation) that $\displaystyle\sum_{i=0}^{n-1} \mathbb{1}_{r_i} \leq k$, hence $nv \leq kV$ by integrating over the volume V.

Let us still suppose valid the hypothesis of the previous theorem which asserts that the mobile point remains in a bounded region, in a portion of the space with volume V, and let us again take the discrete step τ in time. Let us next choose n sufficiently large so that $n > \dfrac{kV}{v}$. One may then find, among the n successive consequents of a region r_0 with volume v, $k + 1$ consequents, denoted

$$r_{\alpha_0}, r_{\alpha_1}, \ldots, r_{\alpha_k}$$

with $\alpha_1 < \alpha_2 < \cdots < \alpha_k$, having a nonempty intersection denoted by s_{α_k}. Let us now call s_0 the α_k-th antecedent of s_{α_k} and s_p, the p-th consequent of s_0. If a mobile point starts from s_0, it will enter the regions

$$s_0, s_{\alpha_k-\alpha_{k-1}}, s_{\alpha_k-\alpha_{k-2}}, \ldots, s_{\alpha_k-\alpha_2}, s_{\alpha_k-\alpha_1}, s_{\alpha_k-\alpha_0}$$

which, by construction, are all included in r_0 (as for each $0 \leq i \leq k$, the α_i-th consequent of $s_{\alpha_k-\alpha_i}$ is in s_{α_k} and therefore in r_{α_i}). One has therefore shown that there are, in the considered region r_0, initial conditions of trajectories which pass at least $k + 1$ times through r_0.

Let us eventually fix a region r_0 with volume v. Let us consider, writes Poincaré, σ_0 the subset of r_0 such that the trajectories issued from σ_0 do not pass through r_0 at least $k + 1$ times between time 0 and time $(n-1)\tau$; denote by w the volume of σ_0. The probability p_k of the set of such trajectories is therefore $\dfrac{w}{v}$.

By hypothesis a trajectory starting from σ_0 does not pass $k + 1$ times through r_0, and hence not through σ_0. From the previous result, one has necessarily that $nw < kV$, such that

$$p_k < \frac{kV}{nv}.$$

No matter how large k may be, one may choose n large so that this probability can be made as small as wanted. Poincaré, tacitly using the continuity of probability along a non-increasing sequence of events, concludes that the probability of the trajectories issued from r_0 which do not pass through r_0 more than k times between times 0 and ∞ is zero.

2.2   Kinetic theory of gases

As observed in the introduction, in 1892 Poincaré was not favorably disposed towards the statistical description of thermodynamics. His polemics with Tait, from

which I quoted several passages, was closely tied to the mechanist spirit in which
Poincaré had been educated. Statistical mechanics, and in particular the kinetic
theory of gases, could not therefore pretend to be more than an ingenious construc-
tion with no explanatory value. An important text revealing Poincaré's thoughts
on the subject was published immediately afterwards, in 1893, in one of the first
issues of the *Revue de Métaphysique et de Morale* [66]. With great honesty, Poincaré
mentioned the classical mechanical conception of the universe due to Newton and
Laplace, but also the numerous problems it encounters when it tries to explain
numerous practical situations of irreversibility, as in the case of molecular motion
in thermodynamics. Poincaré mentioned that the kinetic theory of gases proposed
by the English is the attempt *la plus sérieuse de conciliation entre le mécanisme
et l'expérience* ([66], p.536). Nevertheless, he stated that numerous difficulties still
remained, in particular for reconciling the recurrence of mechanical systems (*"un
théorème facile à établir"*, wrote the author, who may have adopted a humouristic
posture) with the experimental observation of convergence towards a stable state.
The manner in which the kinetic theory of gases pretends to evacuate the problem,
by invoking that what is called a stable equilibrium is in fact a transitory state in
which the system remains an enormous time, did not seem to convince our hero.
However, at least, the tone adopted in [66] is obviously calmer than that of the
exchanges with Tait. Another point which can be observed is that here, as in other
works by Poincaré we shall comment on, Boltzmann was nowhere to be found. This
absence, difficult to imagine as involuntary, remains unexplained, including for Von
Plato in [90], p.84.

In 1892, Lord Kelvin presented a note [47] to the Royal Society (of which he
was then the president) with an unambiguous title. The note presented an *ad hoc*
example demonstrating, in a supposedly *decisive* way, the failure of equipartition
of kinetic energy following Maxwell and Boltzmann's theory. The two physicists
had indeed deduced the equipartition of kinetic energy as a basic principle of their
theory: the average kinetic energies of several independent parts of a system are in
the same ratio as the ratio of the number of degrees of freedom they have. This
result was fundamental for the establishment of a relation between kinetic energy
and temperature.

In his short paper, Kelvin imagined a mechanical system including three points
A, B, C, which are in motion in this order on a line KL, such that B remains almost
motionless and only reacts to the shocks produced by A and C on one side and
the other, whereas the mechanical situation on both sides is different because of a
repulsive force F acting on A and pushing it towards B (in the zone KH of the
scheme) while C can move freely.

The total energy of C is balanced by the energy of A, but, as the latter includes a
non-negative potential-energy term due to the repulsive force, Kelvin triumphantly
concluded that the average kinetic energy of A and C cannot be equal, as they
should have been following Maxwell's theory as the two points each have one degree
of freedom. Kelvin commented:

> 'It is in truth only for an approximately "perfect" gas, that is to say, an
> assemblage of molecules in which each molecule moves for comparatively
> long times in lines very approximately straight, and experiences changes of
> velocity and direction in comparatively very short times of collision, and
> it is only for the kinetic energy of the translatory motions of the molecules
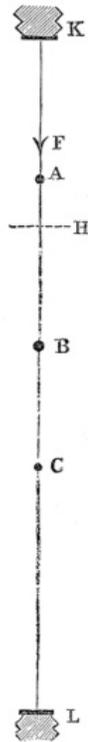
Figure 1: Kelvin's construction in [47].

of the "perfect gas" that the temperature is equal to the average kinetic energy per molecule.' ([47], p.399)

Reading this note encouraged Poincaré, as he noted, to reflect on the kinetic theory of gases, to understand whether Kelvin's objection was well-founded, and to draw his own conclusions on the subject. At this precise moment, when he had been attacked by Tait, the title of the note by such an authority as Kelvin had impressed him and he may have thought he would find there a decisive argument confirming his own skepticism. And so in 1894 Poincaré published his first paper on the kinetic theory of gases [67]. Poincaré began by presenting a long general exposition of the fundamentals of Maxwell's theory. This survey seemed necessary in the first place because the kinetic theory of gases had been much less studied by French physicists than by English physicists[5]. These fundamentals were in the first place the ergodic principle, called the *postulate of Maxwell* by Poincaré, which asserts that, whatever may be the initial situation of the system, it will always pass an infinity of times as close as desired to any position compatible with the integrals of the motion; from this postulate Maxwell drew a theorem whose main consequence was precisely the point contested by Kelvin: in a system for which the only integral is the conservation of the kinetic energy, if the system is made up of two independent parts, the long run mean values of the kinetic energy of these two parts are in the same ratio as their numbers of degrees of freedom.

Poincaré began by observing, as previously in [66], that the recurrence theorem of [62] contradicted Maxwell's postulate along the recurrent solutions. It was therefore

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[5]'[...] a été beaucoup moins cultivée par les physiciens français que par les anglais.' ([67], p.513)

necessary at least to add that the postulate was true except for certain initial conditions. [6] As von Plato comments [90] (p.84), we have here the formulation usually given today for the ergodic principle, in order to take into account the possibility of exceptional initial conditions. Once again, although this idea was also present in Boltzmann's works, the Austrian scientist was nowhere mentioned.

But it was above all the objection contained in Kelvin's paper that Poincaré desired to analyze in detail in order to check whether it contradicted Maxwell's results or not. From the situation of the system A, C, Poincaré built a representative geometric model: a point M in a phase space with three dimensions whose first coordinate is the speed of A, the second the speed of C and the third the abscissa of A. Using Kelvin's system conditions, he could define S, a solid of revolution, from which M cannot exit in the course of time. Naturally, two small regions included in S with the same volume can be entered a different number of times with the same total sojourn time because the speed in these volumes could be different. Poincaré introduced the notion of the *density of the trajectory* in a small element in S with volume v as the quotient $\dfrac{t}{v}$, where t is the total time spent by the trajectory in v ([67], p.519). Using this representation, Poincaré could define the average value of the kinetic energy for A as the moment of inertia of S with respect to the plane yz, for C as the moment of inertia with respect to xz, the 'masses' in S being distributed by the previously defined density. The solid S being one of revolution, these moments of inertia are equal: the fine analysis made by Poincaré therefore shows that one can recover the equipartition result by taking the average of the kinetic energies not uniformly over time but taking into account the phases of the motion and their duration.

Poincaré concluded his paper with a comment that may seem paradoxical in light of the result he had just obtained. While disputing the decisive character of Kelvin's arguments, Poincaré insisted that he nevertheless shared his colleague's skepticism. To give weight to his comment, he slightly transformed Kelvin's example in order to produce an *ad hoc* situation for which there *really is* a problem. In fact, some lines earlier, Poincaré had emphasized what was for him the fundamental point:

> 'I believe Maxwell's theorem really is a necessary consequence of his postulate, as soon as one admits the existence of a mean state; but the postulate itself must admit many exceptions.'[7]

For Poincaré, it was therefore the definition of the average states that posed a problem, and it was the search for a satisfactory definition that required attention from those those who wished to consolidate the bases of statistical mechanics. We shall see later that it was indeed in this direction that Poincaré, and later Borel, focussed their efforts.

2.3   Limit theorems

The textbook [69] published in 1896 constitutes the first of Poincaré's works dealing explicitly with the theory of probabilities. It was, as already mentioned, the result of lectures offered by Poincaré during the academic year 1893-94 at the Sorbonne,

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[6]'[...] sauf pour certaines conditions initiales exceptionnelles' ([67], p.518)

[7]'Je crois que le théorème de Maxwell est bien une conséquence nécessaire de son postulat, du moment qu'on admet l'existence d'un état moyen; mais le postulat lui-même doit comporter de nombreuses exceptions.' ([67], p.521)

redacted by a former student of the École Normale Supérieure, who became an actuary and who probably wished to learn with the master; it was published by Georges Carré. This first edition does not have a preface, and presents itself as a succession of 22 lectures, more or less connected to each other, probably reflecting Poincaré's actual lectures. It is 274 pages long, compared to the 341 pages of the second edition of 1912 [77], which provides an idea of the number of complements. In its initial form, Poincaré's book appears as a successor to Bertrand's treatise [9], the framework of which it follows. Nevertheless, these textbooks really have a different feel, and we must leave it at that for the moment. However, as Poincaré's textbook has been discussed by several commetators, especially in [84] and [25], I shall restrict myself to just a few remarks. Let us note that the authors just mentioned focused on the 1912 edition, which naturally benefited from reflections of Poincaré after that very important period of the mid-1890s, when he was beginning to investigate the mathematics of randomness, so that this may not accurately reflect the mathematician's state of mind in 1896. I choose here, in contrast, to focus on the original 1896 version.

An important part of Poincaré's textbook is devoted to the use of probability theory as a model of measurement error in the experimental sciences. In a commentary on his own works ([78], p.121), Poincaré wrote:

> 'The Mathematical Physics Chair has for its official title: Calculus of Probability and Mathematical Physics. This connection can be justified by the applications of this calculus in all physics experiments, or by those in kinetic gas theory. In any case, for one semester I dealt with probability and my lectures were published. The theory of errors was naturally my main goal. I had to erect explicit reservations concerning the generality of the 'law of errors', but I sought to justify the law via new considerations in the cases where it remains legitimate.'[8]

In [69], the analysis of the law of errors begins on page 147 and occupies part of the following chapters. Poincaré commented on the manner in which the Gaussian character of the error had been obtained:

> '[This distribution] cannot be obtained by rigorous deductions. Several of its putative proofs are awful, including, among others, the one based on the statement that gap probability is proportional to the gaps. Nonetheless, everyone believes it, as M. Lippmann told me one day, because experimenters imagine it to be a mathematical theorem, while mathematicians imagine it to be an experimental fact.'[9]

Much of Poincaré's treatment of the law of errors, in particular the convergence to a Gaussian distribution of a Bayesian approach of the measurement process, was in

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[8]'La Chaire de Physique Mathématique a pour titre officiel : Calcul des Probabilités et Physique Mathématique. Ce rattachement peut se justifier par les applications que peut avoir ce calcul dans toutes les expériences de Physique; ou par celles qu'il a trouvées dans la théorie cinétique des gaz. Quoi qu'il en soit, je me suis occupé des probabilités pendant un semestre et mes leçons ont été publiées. La théorie des erreurs était naturellement mon principal but. J'ai dû faire d'expresses réserves sur la généralité de la "loi des erreurs"; mais j'ai cherché à la justifier, dans les cas où elle reste légitime, par des considérations nouvelles.'

[9]'[Cette loi] ne s'obtient pas par des déductions rigoureuses; plus d'une démonstration qu'on a voulu en donner est grossière, entre autres celle qui s'appuie sur l'affirmation que la probabilité des écarts est proportionnelle aux écarts. Tout le monde y croit cependant, me disait un jour M. Lippmann, car les expérimentateurs s'imaginent que c'est un théorème de mathématiques, et les mathématiciens que c'est un fait expérimental.'([69], p.149)

fact already found by Laplace, at least in a preliminary form. Once again, it is not really possible to make precise what Poincaré knew. For details on what is today known as the Bernstein-von Mises theorem see [88] (p.140 *et seq.* and its references).

Let us consider observations of a phenomenon denoted by $x_1, x_2, \ldots, x_n$. The true measure of the phenomenon under study being z, the *a priori* probability that each of these n observations belong to the interval $[x_i, x_i + dx_i]$ is taken under the form

$$\varphi(x_1, z)\varphi(x_2, z) \ldots \varphi(x_n, z)dx_1 dx_2 \ldots dx_n.$$

Let finally $\psi(z)dz$ be the *a priori* probability so that the true value belongs to the interval $[z, z + dz[$.

Supposing that ψ is constant and that $\varphi(x_i, z)$ can be written under the form $\varphi(z - x_i)$, Gauss obtained the Gaussian distribution by looking for the φ such that the most probable value was the empirical mean

$$\overline{x} = \frac{x_1 + \cdots + x_n}{n}.$$

Poincaré recalled ([69], p.152) Bertrand's objections to Gauss' result; Bertrand had in particular disputed the requirement that the mean be the most probable value while the natural condition would have been to require it to be the *probable value* (which is to say the expectation).

Poincaré thus considered the possibility of suppressing some of Gauss' conditions. Keeping firstly the hypothesis that the empirical mean be the most probable value ([69], p.155 - see also the details in [84], p.149 *et seq.*), he obtained for the form of the error function
$$\varphi(x_1, z) = \theta(x_1)e^{A(z)x_1+B(z)},$$

where θ and A are two arbitrary functions, B being such that the following differential equation is satisfied: $A'(z)z + B'(z) = 0$.

Considering next Bertrand's objection, Poincaré looked next at the problem that arises when one replaces the requirement of the *most probable value* with that of the *probable value*. Here ([69], p.158) he gives a theorem he would use subsequently a number of times: if φ_1 and φ_2 are two continuous functions, the quotient

$$\frac{\int \varphi_1(z)\Phi^p(z)dz}{\int \varphi_2(z)\Phi^p(z)dz}$$

tends, when $p \to +\infty$, towards

$$\frac{\varphi_1(z_0)}{\varphi_2(z_0)},$$

where z_0 is a point in which Φ attains its unique maximum. Following his habit, which drove Mittag-Leffler to dispair, Poincaré's writing was somewhat laconic; he did not specify any precise hypothesis, or supply a real proof, presenting the result only as an extrapolation of the discrete case.

In any case, considering next

$$\Phi(x_1, \ldots, x_n; z) = \varphi(x_1, z)\varphi(x_2, z) \ldots \varphi(x_n, z),$$

Poincaré made the hypothesis that p observations resulted in the value x_1, p resulted in x_2, ..., p resulted in x_n, where p is a fixed and very large integer ([69], p.157).

The condition requiring the mean to be equal to the expectation can therefore be written as

$$\frac{\int_{+\infty}^{+\infty} z\psi(z)\Phi^p(x_1,\ldots,x_n;z)dz}{\int_{+\infty}^{+\infty} \psi(z)\Phi^p(x_1,\ldots,x_n;z)dz} = \frac{x_1 + \cdots + x_n}{n}.$$

Applying the previous theorem, under the hypothesis that Φ has a unique maximum in z_0, one has z_0 as the limit of the left-hand side, which must therefore be equal to the arithmetic mean $\overline{x}$. One is thus brought back to the previous question under the hypothesis that Φ should be maximal at $\overline{x}$. Under the hypothesis that φ depends only on the discrepancies $z - x_i$, Poincaré again obtained the Gaussian distribution. It is remarkable that the form of the *a priori* probability of the phenomenon ψ is not present in the result. This lack of dependence on the initial hypothesis might perhaps have been the inspiration for his method of arbitrary functions, described later.

Poincaré examined next the general problem by suppressing the constraint that φ depends only on the discrepancies, and obtaind the following form for φ

$$\varphi(x_1, z) = \theta(x_1)e^{-\int \psi(z)(z-x_1)dz}$$

where $\int \psi(z)(z - x_1)dz$ is the primitive of $\psi(z)(z - x_1)$ equal to 0 in x_1.

He argued ([69], p.165) that the only reasonable hypothesis was to take $\psi = 1$ as there was no reason to believe that the function φ, which depends on the observer's skillfulness, would depend on ψ, the *a priori* probability for the value of the measured quantity. For θ, on the contrary, there was no good reason to suppose it constant (in which case the Gaussian distribution would be again obtained). Poincaré took the example of the meridian observations in astronomy where a *decimal error* had been detected in practice: the observers show a kind of predilection for certain decimals in the approximations.

Poincaré gave a somewhat intricate justification for focusing on the mean because it satisfies a practical aspect: as the errors are small, to estimate $f(z)$ by the mean of the $f(x_i)$ was the same as estimating z by the mean of the x_i, as immediately seen by replacing $f(x)$ by its finite Taylor expansion in z,

$$f(z) + (x - z)f'(z).$$

In any case, the major justification was given in the following chapter (Quatorzième leçon, [69], p.167), where the consistency of the estimator $\overline{x}$ was studied using an arbitrary law for the error, based on the law of large numbers. After having recalled the computation of the moments for the Gaussian distribution, Poincaré implemented the method of moments in the following way. Suppose that y, with distribution φ, admits the same moments as the Gaussian distribution. One then computes the probable value of $e^{-n(y_0-y)^2}$, where n is a given integer. Decomposing

$$e^{-n(y_0-y)^2} = \sum_{p=0}^{\infty} A_p y^{2p},$$

one obtains

$$\int_{-\infty}^{+\infty} \sqrt{h/\pi}\, e^{-hy^2} e^{-n(y_0-y)^2} dy = \sum_{p=0}^{\infty} A_p \, \mathbb{E}(y^{2p}),$$

letting $\mathbb{E}(y^{2p})$ denote the expectation of y^{2p} (the odd moments are naturally equal to zero), and h a positive constant. The same decomposition is valid by hypothesis if φ replaces the Gaussian distribution in the integral. One has therefore

$$\frac{\int_{-\infty}^{+\infty} \sqrt{h/\pi} e^{-hy^2} e^{-n(y_0-y)^2} dy}{\int_{-\infty}^{+\infty} \varphi(y) e^{-n(y_0-y)^2} dy} = 1,$$

and, using his theorem on the limits again, Poincaré could obtain, letting n tend to infinity,

$$\sqrt{h/\pi} e^{-hy_0^2} = \varphi(y_0).$$

Now, let us consider again that n measures

$$x_1, \ldots, x_n$$

of a quantity z are effectuated, and let us denote by $y_i = z - x_i$ the individual error of the i-th measure. Let us suppose that the distribution of an individual error is arbitrary.

Poincaré began by justifying the fact of considering the mean

$$\frac{y_1 + \cdots + y_n}{n}$$

of the n individual errors as error. Indeed, he explained that the mean becomes more and more probable as the probable value of its square is

$$\frac{1}{n}\mathbb{E}(y_1^2),$$

and so, when n becomes large, the probable value of

$$\left(\frac{y_1 + \cdots + y_n}{n}\right)^2$$

tends towards 0 in the sense that the expectation

$$\mathbb{E}[(z - \overline{x})^2]$$

tends towards 0 (this is the L^2 version of the law of large numbers). As Sheynin observes ([84], p. 151), Poincaré made a mistake when he attributed to Gauss this observation as the latter had never been interested in the asymptotic study of the error. Poincaré then used his method of moments in order to prove that the distribution of the mean is Gaussian when the individual errors are centered and do not have a significant effect on it.

In the 1912 edition [77], Poincaré significantly added a section ([77], n°144 pp. 206-208) devoted to a proof of the central limit theorem and obtained a *justification* a posteriori *de la loi de Gauss fondée sur le théorème de Bernoulli*. Poincaré introduced the characteristic function as

$$f(\alpha) = \sum_x p_x e^{\alpha x}$$

in the *finite* discrete case, and

$$f(\alpha) = \int \varphi(x) e^{\alpha x} dx$$

in the case of a continuous density. In his mind, α was a real or a complex number and neither the bounds of the sum or the integral, nor the issue of convergence were mentioned. Poincaré's considerations on the Fourier and Laplace transforms can in fact already be largely found in Laplace, though the denomination *characteristic function* is Poincaré's. The Fourier inversion formula was used by Poincaré since his lectures on the analytical theory of heat [68] (see in particular the Chapter 6, p.97). As usual, there is no mention of Laplace in Poincaré's text. Plausibly, Laplace introduced characteristic functions for probability distributions after having studied Fourier's treatise (see [22]). In the absence of proof that Poincaré knew Laplace's method, one may speculate that he had the same kind of illumination as his predecessor.

Thanks to Fourier's inversion formula, Poincaré stated that the characteristic function determined the distribution. He could thus obtain simply that a sum of independent Gaussian variables followed a Gaussian distribution and, by means of a heuristic and again quite laconic proof, that the error resulting from a large number of very small and independent partial errors[10] was Gaussian. It seems difficult to award the status of a proof of the central limit theorem to these few lines, a proof published some ten years later in works by Lindeberg and Lévy ([51], [50]). Besides, in this intriguing but rather hasty complement, Poincaré showed his complete ignorance of the Russian research on limit theorems (Čebyčev, Markov et Lyapunov) which produced some well established versions of the theorem.

In the sixteenth chapter of [69], (n°147 of the second edition [77] p.211), Poincaré the physicist still had reservations about what would be an indiscriminate use of the theories he had just described, which depended so heavily on a mathematical idealization (the absence of systematic errors, overly smooth hypotheses...). He wrote, not without irony: 'I've argued the best I could up to now in favor of Gauss' law.'[11] He then focused on the study of exceptional cases and completed his textbook wit a detailed examination of the method of least squares; on these subjects, I refer the interested reader to the already quoted paper by Sheynin ([84]).

2.4   The great invention: the method of arbitrary functions

Although Poincaré is rightly considered to be the father of conventionalism in scientific philosophy, it would be simplistic to think that this position covers all Poincaré's philosophy of research. Admittedly, from the very beginning, the latter had repeated that any use of probability must be based on the choice of a convention one had to justify. Thus, if one throws a die, one is generally led to take as a convention the attribution of a probability of 1/6 for each face to appear. However all the arguments used to justify this convention do not have the same value, and choosing well among them is also part of a sound scientific process. We cannot indeed be satisfied with common sense: Bertrand amused himself, when he constructed his famous paradoxes about the choice of a chord in a circle, in showing that the result depended so closely on the chosen convention that it lost meaning, and that the calculus of probability in such situations was reduced to a more or less ingenious arithmetic. The risk was to condemn the calculus of probability altogether as a vain science and conclude that our *instinct obscur* had deceived us ([70], p.262).

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[10]'[...] résultante d'un très grand nombre d'erreurs partielles très petites et indépendantes.' ([77], p.208)

[11]'J'ai plaidé de mon mieux jusqu'ici en faveur de la loi de Gauss.'

And yet, wrote Poincaré, without this obscure instinct science would be impossible. How can one reconcile the irreconcilable?

Until then, common practice was to cite Laplace and use the principle of insufficient reason as a supporting argument. A dubious argument in fact, as in practice it amounts to assigning a value to the probability only by supposing that the different possible cases are equally probable, since we do not have any reason to assert the contrary. How could a scientist such as Poincaré, who was looking for a reasonably sound basis for using the mathematics of randomness, be satisfied with such a vicious circle?

Let us observe in passing that he was far from the first to deal with such a question. And besides, we have already seen that after Laplace's death, several weaknesses of his approach had been underlined: the vicious circle of the definition of probability by possibility, the absence of an answer to the general question of the nature of the probabilities of causes when applying Bayes's principle, let alone the confusions in ill-considered applications, in particular the judicial ones that we have already mentioned.... A substitute was sought for Laplace's theory. This problem of defining the *natural* value of probabilities had in particular obsessed German psychologists and physiologists throughout the second half of the 19th century ([46]). Von Kries in particular succeeded, a good ten years before Poincaré, in constructing the foundations of a method allowing one to justify the attribution of equal probabilities to the different outcomes of a random experiment repeated a large number of times ([45]). Poincaré, without question, completely ignored these works, all the more because they did not belong *stricto sensu* to the sphere of mathematics.

The question thus for Poincaré was to show that in some important cases, one may consider that the equiprobability of the issues in a random experiment as the result not only of common sense but also of mathematical reasoning, and thereby to avoid the criticism of Laplace's principle.

The idea developed by Poincaré, as earlier by von Kries, was that the repetition of the experiment a large number of times ends in a kind of asymptotic equilibrium, in a compensation, so that the hypothesis of equiprobability becomes reasonable even if one absolutely ignores the initial situation.

As early as the 1780s, Laplace observed that in many cases the initial distribution asymptotically vanishes when one repeatedly applies Bayes' method. For complements, the interested reader may consult Stigler's translation and commentary of Laplace ([86]), and also [20], p.144. At the end of his paper [63], Poincaré briefly mentioned that the convention he had adopted to define the probability concept in the study of the stability of the three-body problem was by no means necessary: the result (a probability equal to zero for non-recurrent trajectories) remains true whatever the convention. Poincaré's aim in subsequent papers was to show that in more general situations, the knowledge of the probability distribution at the origin of time is not needed, as this distribution is not present in the final result. Two examples serve repeatedly as an illustration in the various texts ([70], [75] especially) in which he discussed his method: the uniform distribution of the so-called small planets on the Zodiac and the probabilities for the red and black slots of a roulette wheel. In [70], p. 266, Poincaré observed the proximity of these two situations. The systematic exposition of *arbitrary functions* as a fundamental method of the theory of probability seems due to Borel ([15], p.114) but the expression *méthode des fonctions arbitraires* was later generally used in the context of Markov chains (see

in particular [35]).

Let us first follow Poincaré's comments on the second, and simpler case of roulette ([70], p.267). The ball, thrown with force, stops after having turned many times around the face of a roulette wheel regularly divided into black and red sectors. How can we estimate the probability that it stops in a red sector?

Poincaré's idea is that, when the ball travels for a large number of turns before stopping, any infinitesimal variation in the initial impulsion can produce a change in the color of the sector where the ball stops. Therefore, the situation becomes the same as considering that the face of the game is divided into a large number of red and black sectors. I make, said Poincaré, the convention that the probability for this angle to be fall between θ and $\theta + d\theta$ equals $\varphi(\theta)d\theta$, where φ is a function about which I do not know anything (as it depends on the way the ball moved at the origin of time, an *arbitrary* function). Poincaré nevertheless asserts, without any real justification, that we are *naturally* led to suppose φ is continuous. The probability that the ball stops in a red sector is the integral of φ estimated on the red sectors.

Let us denote by ε the length of a sector on the circumference, and let us consider a double interval with length 2ε containing a red and a black sector. Let then M and m be respectively the maximum and the minimum of φ on the considered double interval. As we can suppose that ε is very small, the difference $M - m$ is very small. And as the difference between the integral on the red sectors and the integral on the black sectors is dominated by

$$\sum_{k=1}^{\pi/\varepsilon}(M_k - m_k)\varepsilon,$$

(where M_k and m_k are respectively the maximum and the minimum on each double interval k of the subdivision of the face with length 2ε), this difference is small and it is thus reasonable to suppose that both integrals, whose sum equals 1, are equal to 1/2.

Once again, Poincaré's writing is somewhat sloppy. He emphasized the importance of the fact that ε was small with respect to the total swept angle,[12] but without giving much detail on how to interpret this fact. His brevity probably came from the parallel with the other example we shall now present - but which he studied earlier in his text.

The expression *small planets* designates the asteroïd belt present between Mars and Jupiter which had been gradually explored until the end of the 19th century. The first appearance of questions of a statistical type about these planets seems to go back to the twelfth chapter of the 1896 textbook ([69], p. 142), where Poincaré asked how one can estimate the probable value of their number N. For that purpose, he implemented a Bayesian method using the *a priori* probability for an existing small planet to have been observed, this probability being supposed to have a density f. It allowed him to carry out the computation of the *a posteriori* expectation of N.

In [70], Poincaré was interested in a remarkable phenomenon: the almost uniform distribution of the small planets in the different directions of the Zodiac. Poincaré looked for arguments justifying this fact ([70], p. 265 *et seq.*). We know, said Poincaré, that the small planets follow Kepler's laws, but on the contrary we absolutely ignore their initial distribution.

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[12]'[...] par rapport à l'angle total parcouru.'

Let then b be the longitude of a small planet at the initial time, and a its mean motion. At time t, its longitude is therefore $at + b$. As already noted, one knows nothing of the initial distribution and we suppose it is given by an arbitrary function $\varphi(a, b)$, once more assumed regular in some way: Poincaré wrote that it was *continuous* but in the sequel used it as a function of class C^∞.

The mean value of $\sin(at + b)$ is given by

$$\int \int \varphi(a, b) \sin(at + b) da db.$$

When t becomes large, this integral becomes close to 0. Poincaré used successive integrations by part, with the derivatives of φ, whereas he could have used only continuity and Riemann-Lebesgue's lemma, but, as we have already seen, Poincaré did not regard the refinement of his hypotheses as a major concern. *A fortiori*, for every nonzero integer n, the integrals

$$\int \int \varphi(a, b) \sin n(at + b)\, da db, \text{ and } \int \int \varphi(a, b) \cos n(at + b)\, da db$$

are also very small for a large fixed t. Therefore, if one denotes by ψ the probability density of the longitude at time t, one has for every $n \geq 1$,

$$\int_{[0,2\pi[} \psi(u) \sin nu\, du, \text{ and } \int_{[0,2\pi[} \psi(u) \cos nu\, du$$

very close to 0. The Fourier expansion of ψ leads to the conclusion that ψ is almost constant, that is to say, that the longitude of a small planet is roughly uniformly distributed on the Zodiac.

2.5 Cards shuffling

If the example of the small planets illustrates sensitivity to initial conditions, that of kinetic gas theory is connected with the complexity of causes. The number of molecules is so large, and they collide in so many ways, that it is impossible to consider the system they form as simply describable by classical mechanics. In 1902 the first textbook was published expounding the basic principles of statistical mechanics, written by Gibbs ([37]). It developed two main applications for the new theory: in addition to the kinetic theory of gases, it introduced the situation of mixing two liquids (a drop of ink put in a glass of water) in order to present the evolution of a system towards equilibrium. Hadamard, in 1906, had written a review of Gibb's book for the *Bulletin des sciences mathématiques* ([38]). In order to illustrate this mixing situation, he invented the ingenious metaphor of the shuffling of a pack of cards by a gambler evolving towards an equal distribution of the possible permutations of the cards. Hadamard however did not propose any mathematical treatment of the question and it was Poincaré, in the paper he published in 1907 in Borel's journal ([75] republished later in [76] and [77]), which first analyzed the problem. He restricted himself in fact to the simplest case, that of two cards. Let us suppose, said Poincaré, that one has a probability p that after one permutation, the cards are still in the same order as before the permutation, and $q = 1 - p$ that their order is reversed. Let us consider there are n successive permutations and that the gambler who shuffles the cards earns a payoff S equal to 1 franc if the order after these n

permutations is unchanged, and -1 franc if it is reversed. A direct computation of the expectation shows that

$$\mathbb{E}(S) = (p - q)^n,$$

as, in a modern formulation, S can be written as

$$\prod_{i=1}^{n} X_i$$

with the $X_i = \pm 1$ independent with distribution (p, q) representing the fact that the i-th permutation has changed or not the order of the cards. Hence, except in the trivial cases $p = 0$ or 1, $\mathbb{E}(S) \to 0$ when n tends towards infinity, which amounts to saying that the two states $+1$ and -1, and therefore the two possible orders, tend to become equiprobable. It is interesting that Poincaré was inspired, for the recursive computation of the expectation without first looking for the distribution of S, by several computations of expectations he found in Chapter III of Bertrand's book [9].

As mentioned by Poincaré, the tendency to uniformity remained true whatever the number of cards but the *démonstration serait compliquée*. One may suppose that in 1907 Poincaré already had the idea of the proof for the general case of n cards but he wrote it only for the second edition of his textbook of probability in 1912, in the first section of a chapter added to the book, entitled *Questions diverses*. Curiously, Poincaré's method of proof, contrary to what he had done in the case of two cards, was not inspired by probabilistic reasoning but was connected to the theory of groups. Though the Perron-Frobenius theory was already available (but probably unknown to Poincaré), Poincaré referred to older works by Frobenius published in the *Sitzungsberichte* of Berlin Academy between 1896 and 1901, and by Elie Cartan [24] that he had himself extended in his paper [72] (consult [83] and [25] for details). We shall see in the next part that this non-probabilistic aspect did not escape Borel, who proposed an alternative approach. As for card shuffling, it enjoyed a spectacular renewed interest in the 1920s.

3   Third part: an uneven heritage

We now tackle the heritage of Poincaré's ideas about randomness and probability. This is an intricate question. Indeed, Poincaré cannot be considered as a full probabilist in the spirit of mathematicians of later generations like Paul Lévy and Andrei Nikolaevich Kolmogorov. As we have already mentioned, these studies on probability constitute only a very small island in the ocean of the mathematician's production. Moreover, it is rather difficult to locate a very precise result, a theorem, concerning the theory of probability which can be specifically credited to Poincaré. His primary goal was to refine already existing results or to explore new aspects and new questions while not feeling compelled to give them a complete structure. We should again repeat here that in this domain more than any other, it was above all Poincaré whom Poincaré sought to convince, and therefore his works dealing with probability, including his philosophical texts, often take on a rambling tone, written following his train of thought, often slightly verbose, illustrating Picard's opinion (as reported by K. Popoff): he ignored the adage *pauca sed matura*[13]. As Bru notes

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[13]'Il ne connaissait pas l'adage *pauca sed matura*.' ([80], p. 89)

([20], p. 155), everyone at that time had read Poincaré. But one has the impression that few understood what he wrote about probability.

3.1   Borelian path

Émile Borel was unquestionably the main exception. Not only did he read and understand Poincaré, but he was poised to make the subject his own in a spectacular way, so much so that he may be regarded as the first French probabilist of the twentieth century. We shall examine how this passing of the baton took place between the master and his young disciple.

It must firstly be said that this probabilistic turn of Émile Borel was one of the most singular changes one could observe in a mathematician around 1900. After initiating a profound transformation of methods in the theory of functions, Borel became a star of mathematical analysis in France. Nothing seemed to predispose him to take the plunge and to devote important efforts to study, refine and popularize the calculus of probability whose dubious reputation in the mathematical community - on which we commented above - might have led to rebuke. The context of Borel's turn since 1905, the date of publication of his first work in this domain, has been studied in detail several times, for instance in the papers [32] and [59]. The main difference which can be found between the discovery of probability by Poincaré and by Borel is that, for the latter, it arose from reflections within the mathematical field and more specifically by considerations on the status of mathematical objects - in particular about real numbers. In Borel, during the years just preceding 1900, we note indeed a greater and greater distance from Cantorian romanticism and its absolutist attitude, as emphasized by Anne-Marie Décaillot in her beautiful book on Cantor and France ([31], p. 159). Borel gradually replaced this idealistic vision, which no longer satisfied him, by a realism colored by a healthy dose of pragmatism: the probabilistic approach appeared then to Borel as an adequate means by which to confront various forms of reality: mathematical reality first and then physical reality and practical reality. . . .

The best synthesis summing up Borel's spirit about the quantification of randomness can be found in Cavaillès' text [27] published in the *Revue de Métaphysique et de Morale*; it should be seen, at least in part, as a commentary on Borel's essay on the interpretation of probabilities [18] that completed the great enterprise of the *Traité du Calcul des Probabilités et de ses Applications* begun in 1922. As Cavaillès lyrically put it ([27], p.154), probabilities appear to be the only privileged access to the path of the future in a world which is no longer equipped with the sharp edges of certainty, but presents itself instead as a hazy realm of approximations. Borel, at the moment of his probabilistic turn thirty years earlier, expressed himself similarly when he asserted that a coefficient of probability constituted the clearest answer to many questions, an answer which corresponded to an absolutely tangible reality, and when he was ironic about reluctant spirits who preferred certainty, and who would perhaps also prefer that 2 plus 2 make 5.[14]

I refer the reader to the aforementioned studies for precise details on these questions. What I would like to consider here is how Borel had combined his research on calculus of probability with the considerations of his predecessor. From his very first paper, Borel announced that he adopted the conventionalism of Poincaré ([13],

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[14]'[. . .] peut-être aussi que 2 et 2 fissent 5.'

p.123). But his aim was to illustrate the role that the (then novel) Lebesgue integral and measure theory could play, after he discovered with amazement their use in [95] by the Swedish mathematician Anders Wiman (on this subject, see [32]).

> 'The methods adopted by Mr. Lebesgue allow us to examine [...] questions of probability that appear inaccessible to the classical methods of integration. Moreover, in the simpler cases, it suffices to use the theory of sets I called 'measurable', and which Mr. Lebesgue later termed 'measurable (B)'; the first use of this theory of measurable sets for the calculation of probability is due, I believe, to Mr. Wiman.'[15]

I shall not deal here with the radical transformations the Lebesgue integral brought to analysis at the beginning of the twentieth century. For a broad overview, one consult [41]. Nevertheless, for the sake of completeness, let us say at least a few words about Borel's role in the elaboration of this theory.

In his thesis dealing with questions of the extension of analytic functions, Borel invented a new concept of analytic extension, more general than that of Weierstrass, using a great deal of geometric imagination. In the course of his proof, he proved that a countable subset of an interval can be covered by a sequence of intervals with total length as small as one wants. This was probably the first appearance of a σ-additivity argument for the linear measure of sets. In subsequent years, Borel considerably fleshed out his construction, in particular in his work [12], by introducing the notion of measurable set and of measure based on σ-additivity. These concepts had however a limited extension for Borel as he considered only explicit sets obtained by countable unions and complementary sets, forcing him to make the shaky suggestion that one should attribute a measure *inferior to* α to any subset of a measurable set with measure α.

One had to wait for Lebesgue's thesis and the publication of his Note [49], in which he introduced a new conception for integration, for the notion of measurable set to reach its full power, on which is based the remarkable flexibility of the integral exploited by Borel in his paper [13]. There he showed in particular how the use of Lebesgue's integral can allow one to give meaning to some questions formulated in a probabilistic way. One of the most simple of these is that of assigning zero probability to the choice of a rational number when drawing a real number at random from the interval [0,1]. Let us insist on the fact that for Borel, it was more critical that the Lebesgue integral give a question meaning, than to provide its answer. We see here Borel being absolutely in line with Poincaré's conventionalism, but the choice of the convention (identifying the probability with the measure of a subset of [0,1]) is based on mathematical considerations.

However it is above all in his long paper of 1906 on the kinetic theory of gases ([14]) that Borel would fit in Poincaré's heritage, at the same time introducing new considerations showing that he was also striking out on his own. He often insisted on the difference between his approach and that of Poincaré ([14], p.11, note 2).

Borel's aim was to provide a genuine mathematical model for Maxwell's theory in order to satisfy mathematicians.

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[15]'Les méthodes de M. Lebesgue permettent d'étudier [...] des questions de probabilités qui paraissent inaccessibles par les procédés d'intégration classique. D'ailleurs, dans les cas particuliers les plus simples, il suffira de se servir de la théorie des ensembles que j'avais appelés mesurables et auxquels M. Lebesgue a donné le nom de mesurables (B); l'application de cette théorie des ensembles mesurables au calcul des probabilités a été, à ma connaissance, faite pour la première fois par M. Wiman.' ([13], p. 126)

considered by Poincaré, Borel challenged it as being questionable, the planets having clearly mutual influences ([14], p. 12), and so he sought to forgo this hypothesis. Progressively enlarging his initial problem, Borel arrived at the following asymptotic formulation ([14], p. 15):

> Problem C. - Given the mean motions of n small planets to within ε and their exact initial positions, one denotes by $\overline{\omega}$ the probability that, at a time t chosen at random in an interval a, b, every corresponding point P is in C_1. What is the limit to which $\overline{\omega}$ tends when the interval a, b increases indefinitely?[17]

Borel could then implement a method of arbitrary functions in dimension n without supposing the initial independence of the motions of the planets, and prove that asymptotically the desired probability was $1/2^n$, a type of ergodic theorem which showed an asymptotic independence he would also show in the case of his model for the kinetic theory of gases having Gaussian distributions as limits. A rather curious detail is that the result given by Borel, proving the convergence of the uniform distribution on a sphere with radius $\sqrt{n}$ in n dimensional space to independent Gaussian variables, is today called *Poincaré's Lemma*, even though it is entirely absent from the works of Poincaré (for complements about this strange fact, see [57] and the references therein).

I have not been able to make out clearly whether Poincaré was ever interested in the research and the work of his successor in the field of probability. The only sign which might indicate at least a passing interest is the fact that he agreed to write an article entitled "Le hasard" ([75]) for the *Revue du Mois*. But, to my knowledge, Poincaré did not comment on Borel's work, and what is even more surprising, if one remembers Poincaré's work at the beginning of the 1890s, he showed no interest in measure theory as applied to the mathematics of randomness. Poincaré, here again, stood poised on the threshold of a domain he helped to create, but did not enter.

3.2   Markovian descent

In order to complete this outline of Poincaré's probabilistic heritage, let us finally consider what may have been the most amazing consequence of his work: the dazzling development, since the end of the 1920s, of the theory of Markov chains and Markov processes. This story has already been set out in several texts and I shall again restrict myself to comments on only the most salient points, referring the reader elsewhere for more information.

We have already evoked Poincaré's investigations of card shuffling and the fact that in his proof of the convergence to the uniform distribution in [77], he used an algebraic method with limited exploitation of the probabilistic structure of the model. Borel, an attentive reader, immediately realized this and wrote a note, asking Poincaré to present it to the Academy for the *Comptes-Rendus de l'Académie des Sciences* (the only letter from Borel placed online on the website of the Archives Poincaré [18]).

Borel wrote to his colleague on 29 December 1911:

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[17] 'Problème C. - Connaissant à ε près les moyens mouvements des n petites planètes et connaissant exactement leurs positions initiales, on désigne par $\overline{\omega}$ la probabilité pour qu'à une époque t choisie arbitrairement dans un intervalle a, b tous les points P correspondants soient sur C_1. Quelle est la limite vers laquelle tend $\overline{\omega}$ lorsque l'intervalle a, b augmente indéfiniment?' ([14], p. 15)

[18] http://www.univ-nancy2.fr/poincare/chp/

'I have just read the book you kindly sent me; I do not need to tell you how much the new sections interested me, in particular your theory of card shuffling. I tried to render it accessible to those unfamiliar with complex numbers, and it seems to me that in doing so I reached a slightly more general proposition. If it is new, and if you find it interesting, I would ask you to communicate the attached note.[19]

Poincaré acted immediately and the note was presented on 2 January 1912. Borel's method in [16] was in fact an extension of the elementary one used by Poincaré to treat the case of two cards, where one looks at the evolution of the successive means in the course of time. This method later became the standard in proofs of the exponential convergence of an irreducible finite Markov chain towards its stationary distribution (see for instance [10], p.131). Here the stationary distribution is uniform due to the reversible character of the chain. Borel even gave himself the satisfaction of introducing a dependence on time (the chain becoming inhomogeneous).

He considered the regular case where there exists an ε such that, at every moment, the transition probabilities of one permutation to another at a subsequent time are all greater than some ε. In Borel's notation, let $p_{j,n}$ be the probability of the j-th possible permutation of the cards before the n-th operation. Denoting by $\alpha_{j,h,n}$ the probability for A_h to be replaced by A_j during the n-th operation, one has

$$p_{j,n+1} = \sum_{h=1}^{h=k} \alpha_{j,h,n} p_{h,n}$$

with the constraint $\sum_{h=1}^{k} \alpha_{j,h,n} = 1$ where k denotes the number of possible permutations. Let us immediately observe that P_n and p_n, the largest and the smallest of the $p_{j,n}$, form two sequences, respectively nonincreasing and nondecreasing. Let P and p denote their limits. For a given $\eta > 0$, one may choose n for which $P_n \le P + \eta$, and therefore the $p_{j,n}$ are inferior to $P + \eta$. After N operations one can write

$$p_{j,n+N} = \sum_{h=1}^{h=k} \beta_{j,h,n} p_{h,n} \ , \ \sum_{h=1}^{h=k} \beta_{j,h,n} = 1$$

where the $\beta.$ are the transition probabilities between time n and time $n + N$, each being greater than ε by hypothesis.

Let us consider the smallest of the $p_{h,n}$, $p_{h_0,n}$ so that $p_n = p_{h_0,n} \le p$. For the sake of simplicity, let us denote by β its coefficient $\beta_{j,h_0,n}$; by hypothesis, $\beta \ge \varepsilon$. Let us observe that $\sum_{h=1,h\ne h_0}^{h=k} \beta_{j,h,n} = 1 - \beta$. Therefore, one can write, by choosing j such that $p_{j,n+N}$ is superior or equal to P,

$$P \le p_{j,n+N} \le \beta p + (1 - \beta)(P + \eta) = P + (1 - \beta)\eta - \beta(P - p)$$

and hence

$$P - p \le \frac{1 - \beta}{\beta} \eta \le \frac{1 - \varepsilon}{\varepsilon} \eta.$$

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[19]'Je viens de lire le livre que vous avez eu l'amabilité de me faire envoyer; je n'ai pas besoin de vous dire combien les parties neuves m'ont intéressé, en particulier votre théorie du battage. J'ai essayé de la mettre à la portée de ceux qui ne sont pas familiers avec les nombres complexes, et il m'a semblé que j'obtenais ainsi une proposition un peu plus générale. Si elle est nouvelle, et si elle vous paraît intéressante, je vous demanderai de communiquer la note ci-jointe.'

η being arbitrary small, one concludes that $P = p$ and therefore that asymptotically the $p_{j,n}$ become all equal to $1/k$. Let us observe in passing that in these blessed years when it was permitted to publish mistakes, Borel erred in writing his inequality, considering ε instead of the number we called β, a fact which naturally did not change anything in the final result.

Nobody seemed to have paid attention to Borel's note: when these results were rediscovered by Lévy and then Hadamard in the 1920s, neither of them had the slightest idea of its existence (on this subject see the letters from Lévy to Fréchet [6], pp.137 to 141).

We must next skip five years and cover several hundreds of kilometers east in order to see a new protagonist coming on stage, the Czech mathematician Bohuslav HOSTINSKÝ. Moreover, as if it were not enough that we must invoke an unknown mathematician, we must first say a few words about an unknown philosopher. Indeed the man who may have been, together with Borel, the most attentive contemporary reader of Poincaré's texts on probability was another Czech, the philosopher Karel VOROVKA (1879-1929), whose influence on Hostinský was decisive.

It is not possible here to discuss this singular figure in detail and I shall therefore restrict myself to giving some elements explaining how he had got involved in this melting pot. An interesting and very complete study on Vorovka was published in Czech some years ago [60] and hopefully it will become more accessible in a more widely known language. Some complements can also be found in [56] and the references therein. Two reasons explain this general ignorance of Vorovka: the fact that his works, mostly in Czech, were never translated, and also that his early death precluded the collection of his ideas in a large-scale work. Placing himself in the tradition of Bernhard Bolzano (1771-1848), the major figure of the philosophical scene in Prague during the 19th century, Vorovka sought an approach combining both his strong mathematical education and a rather strict religious philosophy, an original syncretism of empiricism and idealism which had close links with the thought of the hero of the Czechoslovak independence, T.G. Masaryk, and with American pragmatist philosophy, in which he had much been interested.

Vorovka's discovery of Poincaré's philosophical writings at the beginning of the 20th century was a real revelation: he drew from them the conviction that the scientific discoveries at the end of the 19th century, especially in physics, compelled a reconsideration of the question of free will. Vorovka showed a real originality in that he did not content himself with principles, but closely studied the mathematical problems raised by the theory of probability. He was a diligent reader of Bertrand's textbook, of Borel's texts, but also of Markov's works, publishing several works inspired by papers of the Russian mathematician (see [54], [92], [93], [94]). At the time when he was granted tenure at the Czech University in Prague, around the year 1910, Vorovka met the mathematician Bohuslav Hostinský, who had just returned to Bohemia after a period of research in Paris. In Hostinský's own words (see [44]), it is through the discussions he had with Vorovka that he learned about Poincaré's works, and he began to reflect upon the calculus of probability, a domain somewhat remote from his original field of research (differential geometry).

Following Jiří Beránek, who was one of the last assistants to Hostinský after the Second World War at the University of Brno, another source of the latter's interest in the calculus of probability is found in his reading of the 1911 paper by Paul and Tanya Ehrenfest on Statistical Mechanics for the *Encyklopädie der Mathematischen*

Wissenschaften, translated and completed by Borel for the French version of the *Encyclopédie des Sciences Mathématiques* [33].

Beránek wrote ([8]) that this paper, whose impact was considerable,

> 'put the emphasis on statistical methods in physics, along with geometrical methods, mainly in connection with the works of L. Boltzmann on kinetic gas theory. Boltzmann's work sustained discussions and controversies concerning the correctness and legitimacy of his mathematical methods. Hostinský, as he mentioned later, began to study Boltzmann's works in 1915, and to take an interest in the efforts made to provide precise mathematical bases for the kinetic theory. The central point of these efforts implied a reexamination of some fundamental questions of the theory of probability. Hostinský was especially impressed by the fundamental works of H. Poincaré on the bases of probability calculus which opened the way for new methods necessary for the improvement of kinetic theory. For this reason, around 1917, Hostinský began to study in earnest questions in the calculus of probability...'[20]

The fact that Hostinský began to deal seriously with probability in 1917 is attested by his own diary, kept in the archives of Masaryk's university in Brno. The diary's entries concern only comments on differential geometry until 1917. On 10 January 1917, Hostinský made some observations on the study of card shuffling by Poincaré, following [77], and on January 18th he took up problems of lottery. A first paper appeared some months later in the *Rozpravy České Akademie* dealing with the problem of Buffon's needle [43].

The problem of Buffon's needle is a classic of the calculus of probability and Hostinský began by expounding it:

> 'A cylindrical needle is thrown on a horizontal floor, on which are traced equidistant parallels; the distance $2a$ between two successive parallels is supposed larger than the length $2b$ of the needle. What is the probability that the needle meet one of the parallels?'[21]

Buffon had proposed a solution whose numerical result $\frac{2b}{\pi a}$, in which π was present, was a source of numerous propositions for an 'experimental' calculation of π. But, in fact, Buffon's proof was based on the hypothesis that the needle center could be located anywhere on the plane, and Hostinský, in a second critical part of his paper, mentioned the dubious nature of such an hypothesis, just as Carvallo had done before him in 1912. An experimental device could only take the form of a table of limited size, and it is then clear that, depending on the choice of a small square

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[20]'[...] mettait l'accent sur les méthodes statistiques en physique, à côté des méthodes géométriques, principalement en relation avec les travaux de L. Boltzmann sur la théorie cinétique des gaz. Sur ceux-ci furent menées discussions et controverses, au sujet de l'exactitude et de la légitimité des méthodes mathématiques employées. Hostinský, comme il l'a lui même mentionné, commença à partir de 1915 à étudier les travaux de Boltzmann et à s'intéresser aux efforts qui étaient faits pour donner à la théorie cinétique des bases mathématiques précises. Le point central de ceux-ci nécessitait un nouvel examen de certaines questions fondamentales de la théorie des probabilités. Hostinský fut particulièrement impressionné à ce sujet par les travaux fondamentaux de H. Poincaré sur les fondements du calcul des probabilités qui ouvraient la voie à de nouvelles méthodes nécessaires pour le perfectionnement de la théorie cinétique. Pour cette raison, vers 1917, Hostinský commença à s'occuper sérieusement de questions de calcul des probabilités...'

[21]'On lance une aiguille cylindrique sur un plan horizontal, où sont tracées des parallèles équidistantes; la distance $2a$ de deux parallèles voisines est supposée plus grande que la longueur $2b$ de l'aiguille. Quelle est la probabilité pour que l'aiguille rencontre l'une des parallèles?'

C_1 at the center of the table or another square C_2 on the edge of the table with the same area, the probability p_1 that the center of the needle belongs to C_1 and the probability p_2 that it belongs to C_2 cannot be the same: indeed, C_2 is strongly subject to the constraint that the needle does not fall from the table, but C_1 is very weakly so constrained, so that intuitively one should have $p_1 >> p_2$.

Hostinský therefore considered it indispensable to suppose unknown the *a priori* distribution of the localization of the needle. It is an *unknown* distribution (with density) $f(x,y)dxdy$. But, mentioned Hostinský, Poincaré also, in the resolution of several problems of probability, allowed the use of such an *arbitrary* density and observed that in some situations this function would not be present in the final result. Hostinský proposed to prove that if a domain A of the space is segmented in m elementary domains with the same volume ε, and containing each a white part with volume $\lambda\varepsilon$ and a black part with volume $(1-\lambda)\varepsilon$ (where $0 < \lambda < 1$), then for any sufficiently regular function $\varphi(x,y,z)$, the integral on the white parts will asymptotically (when m tends to infinity) be equal to λ times the integral of φ on A.

Hostinský then applied this result in order to propose a *new* solution to the problem of the needle. Instead of Buffon's unrealistic hypothesis, he supposed that the center of the needle is compelled to fall in a square with side $2na$, $n \in \mathbb{N}$, with a density of probability given by an unknown function φ (which he supposed to have bounded derivatives) and kept on the other hand the second hypothesis concerning the uniform distribution of the angle ω of the needle with respect to the parallels. This being set out, dividing the domain of integration $0 < x < 2na, 0 < y < 2na, 0 < \omega < \frac{\pi}{2}$ in n^2 subdomains (by partitioning the values of x and y with respect to the multiples of a), each small domain is itself divided into two parts (corresponding to the fact that the needle intersects [*white* part] or does not intersect [*black* part] the corresponding parallel). The ratio of their respective volumes to the total volume of the subdomain is constant and equal, for the white part, to $\frac{2b}{\pi a}$. An application of the previous theorem then allows one to assert that we obtain the desired probability, at least asymptotically when n tends towards infinity.

In the Spring of 1920, seeking to benefit from the sympathy of French public opinion towards the young Czechoslovakia, Hostinský had sent to Émile Picard the translation of his paper and Picard proposed immediately (18 April 1920) to include it in the Mélanges of the *Bulletin des Sciences Mathématiques*. This slightly revised version of the paper of 1917 was published at the end of 1920 and Maurice Fréchet, who had just arrived in Strasbourg and considered himself as a missionary [85] read it with attention, as he mentioned in a subsequent letter to Hostinský, dated 7 November 1920, in which he congratulated him on having obtained such a *positive result*.

As we have just explained, following Poincaré's example, Hostinský required that the function φ admit a uniformly bounded derivative in the domain A in order to obtain an upper bound for the difference between the maximum and the minimum of φ on each of the small domains. But Fréchet, when he read the paper, rightly realized that as only an estimation of the integrals of φ on these domains was needed, the simultaneous convergence of the superior and inferior Darboux sums towards the integral of φ allowed one to obtain the desired result with φ Riemann-integrable. This is what he wrote, together with the proof, to Hostinský on 7 November 1920.

It seems that the former letter refers to Fréchet's initial research on probabilistic

questions. It was subsequently published in a short note in 1921 ([34]). Hostinský answered on 22 December 1920, agreeing with Fréchet that the integration hypothesis was sufficient. He also mentioned that Borel had already suggested that Poincaré's hypothesis could be weakened, supposing only the function to be continuous. In his textbook on probability published in 1909 by Hermann [15], in which Borel devoted the whole of Chapter VIII to the introduction of arbitrary functions by considering both Poincaré's examples of the roulette wheel and of the small planets on the Zodiac, Borel noted that the hypothesis of continuity was sufficient to apply Poincaré's method. Fréchet was to include Hostinský's observation in his note in 1921 [34] (where he emphasized that it had been inspired by the latter after having read his paper on Buffon's needle). In [34], he mentioned Borel's work to emphasize that hypotheses of both continuity (Borel) and derivability (Poincaré) were useless and that Riemann-integrability was sufficient.

His friendly relationship with Fréchet encouraged Hostinský to continue his probabilistic studies, and this leads us eventually to the last step of this long journey, introducing Jacques Hadamard (1865-1963). The presence of this name in our story may seem quite strange, and, in fact, Hadamard was interested in probabilities only during one semester of the academic year 1927-1928. He had never considered them before, and would never do so again, showing even some irritation towards Lévy, one of the disciples of whom he was most fond, when he 'wasted' his mathematical talent in the 1920s and left the royal path of functional analysis for the calculus of probability.

Following Poincaré's example, Hadamard always kept in mind physical theories from which he intended to extract new mathematical problems. It was from this perspective that he had written the aforementioned review of Gibbs' book in 1906.

When Hadamard began writing up his course of analysis at the École Polytechnique in the 1920s (published by Hermann in two volumes in 1926 and 1930), he had to prepare some lectures in 1927 on probability theory, and he took up Poincaré's example of card shuffling. On this occasion, he recovered Borel's method of successive means and published in 1927 a note in the *Comptes-Rendus de l'Académie des sciences de Paris* [39]. Soon after that, Hostinský discovered Hadamard's note and sent an extension of it for publication in the *Comptes-Rendus* [42], which appeared in the first weeks of 1928. There, for the first time, before everyone - except for Bachelier, but, alas, who had ever read Bachelier among the mathematicians I write about! - and especially before Kolmogorov, Hostinský introduced a Markovian model in continuous time. At the Bologna congress in September 1928, it was realized that Poincaré's card-shuffling studies were in fact a special case of the model of variables in chain introduced by Markov in 1906, and developed in several of his posterior publications as well as by Bernstein in 1926, but which were largely ignored outside Russia. The attention this drew, in particular at the Congress in Bologna in 1928, inaugurated intense activity on these questions which continued throughout the 1930s, a story brilliantly recounted in [20] to which I refer the interested reader. This unexpected crowning of Poincaré's efforts seems to be a perfect moment to take leave of the master.

Conclusion

Poincaré lived during that very specific moment in the history of science when randomness, in a more and more insistent way, challenged the beautiful deterministic edifice of Newton's and Laplace's cosmology which had dominated scientific thinking for centuries. A conference by Paul Langevin in 1913 [48] shows the extent of this challenge, paralleling the introduction of probabilities and a drastic change in our comprehension of the structural laws of matter. Such a penetrating mind as Poincaré could not have lived this irruption otherwise than as a traumatic one, that he had to face with the means he had at his disposal. These means, as we saw, had not yet reached the degree of power necessary to deal with many problems raised by modern physics. Let us recall one of the master's apothegms:

> 'Physics gives us not only an occasion for problem solving: it helps us find the means of solution, and in two ways. It points us in the direction of the solution, and suggests how to reason our way there.'[22]

And the new physical theories with which Poincaré was confronted suggested developing the theory of probability in the first place - a suggestion which can be also found, but in a slightly different perspective, among the problems Hilbert expounded during the Paris Congress of 1900. Therein lies the apparent paradox which puzzled the mathematician at the turn of the century: the hesitation and reluctance in the face of problems raised by statistical mechanics, the somewhat uncertain attempts to give solid bases to the theory of probability, the seemingly limited taste for new mathematical techniques, in particular measure theory and Lebesgue's integration, though they could have provided decisive tools to tackle numerous problems. Poincaré, as we said, remained a man of the 19th century, maybe in the same way as Klein had mischievously presented Gauss as a scientist of the 18th century. Naturally, in Gauss's case, the irony came from the fact that he had lived two thirds of his life in the 19th century, whereas death surprised Poincaré at the beginning of the 20th century. But we may speculate - although *not* here! - on the manner in which our hero would have adapted to transformations in the scientific world picture. We have seen that, following the example of his glorious predecessor, Poincaré sowed widely, and the spectacular blossoms of many of his ideas inspired countless researchers after his death. As for probabilities, I think one can sum up the measure of his influence as follows: he began to extract the domain from the grey zone to which it had been confined by almost all French mathematicians, he initiated methods that flourished when they integrated more powerful mathematical theories, he convinced Borel of the importance of certain questions, to the study of which he eventually devoted an enormous amount of energy. For a rather marginal subject in Poincaré's works, such a contribution appears far from negligible.

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[22]'La physique ne nous donne pas seulement l'occasion de résoudre des problèmes; elle nous aide à en trouver les moyens, et cela de deux manières. Elle nous fait pressentir la solution; elle nous suggère des raisonnements.'([73], p. 152)

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