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# *Volterra's Prolusione as a source for Borel's interest in probability<sup>1</sup>*

Antonin DURAND<sup>2</sup> et Laurent MAZLIAK<sup>3</sup>

## **Abstract**

*In this paper, we study the influence of a paper by Vito Volterra on Borel's interest for probability. We try to prove that the reading of this article was for Borel an occasion of discovering new applications for the mathematics of randomness (such as biology or economics) and of measuring their importance as a tool provided to the citizen in order to face choices in social life.*

## **Introduction**

When Emile Borel met Vito Volterra, eleven years his senior, in 1897 at the International Congress of Mathematicians in Zurich, he was a young brilliant mathematician who had, extremely quickly, risen to the post of associate professor at the university of Lille, but whose international reputation had been hitherto overshadowed by elder mathematicians like Poincaré and Hermite who dominated the French school at that time. Volterra, despite being older and already well regarded by his peers, had not yet reached the watershed in his career that would increasingly involve him in the building of the Italian scientific institutions. The subsequent friendship of the two men, which lasted until the death of Volterra in 1940, was the basis for a large series of letters, most of which have been conserved at the Academia dei Lincei in Rome and at the Académie des Sciences in Paris.

The dialogue between these two personalities in the field of mathematics from their respective countries was often institutional in nature (organisation of conferences, wartime cooperation). Mathematics was not always at the center of their correspondence or, more precisely, their particular interests never concurred.

Letters that testify to their mutual simultaneous research are far from dominate in number, and it is often difficult to deduce any reciprocity in the issues motivating their efforts. Yet it is this very point which we wish to address in turning to Emile Borel's seminal work of 1905 on Probability which alludes significantly from Vito Volterra's text on "The Application of Mathematics in Biology and Economics". This paper, initially delivered as the inaugural lecture of the 1901-1902 university term in Rome, appears again in 1906 in the *Revue du Mois* founded by Borel the previous year.

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This was the French mathematician's first encounter with the text with which he would later open the first edition of his new review. The decision to publish this text endows the paper with the importance of a whole program of research and we believe it to show the very real influence of Volterra's work on Borel, whose career and scientific interests were then undergoing significant development. We do not wish to overstate this influence over the developments occurring in 1905, a particularly productive year for Borel : rather to place it as a new factor in a complex series of events.

The questions addressed in our paper are therefore: How Borel read Volterra's text? What did he find there to modify his understanding of probability and of the possibilities of application in new domains as natural sciences or economics? Borel's early interest for mathematics of randomness has been examined in several studies, e.g. (Knobloch, 1987), (von Plato, 1994), (Callens, 1997) or (Bru, 2003). However, to our best knowledge, Volterra's *Prolusione* was never directly related to this probabilistic turn in the aforementioned studies. Neither it was in those concerning Volterra such as (Guerraggio and Nastasi, 2005), (Goodstein, 2007) or (Guerraggio and Paoloni, 2008).

This is why we shall first consider the importance of the year 1905 in Borel's intellectual and personal development, and then move on to the paper by Borel on the "Application of Mathematics in Biology and Economics," published in the inaugural edition of the *Revue du Mois*, its context in Italy, in 1901, and in France in 1905. Finally we will evaluate the extent to which the text is developed and echoed in the thoughts and works of Borel at the time and in the long term.

## I- Emile Borel 1905

Émile Borel was thirty-four years old in 1905. For several years he had been an associate professor at the Ecole Normale Supérieure and gained the recognition of his peers by receiving the Grand prix des sciences mathématiques (1898), the prix Poncelet (1901) and then the prix Vaillant (1904). The year 1905, however, seems like a key moment in the work and development of this multitalented man. It was the year of the renewal if not complete change of his scientific work moving increasingly towards probability following the appearance of his first article on the subject in 1905 in the *Bulletin de la Société mathématique de France*. He moved well beyond his circle of academic mathematicians most notably publishing articles with reflections on mathematics in philosophical magazines, which, eventually, lead him to show more fully his social ambitions by the creation of the *Revue du Mois* whose first edition came out in January 1906

These three disparate elements simultaneously came together around the year 1905. Their coexistence, however, inevitably became problematic. It is interesting to separate these various elements concurrent during that pivotal year. Naturally, their disparity and interdependence shall be the focus of our attention.

### *The Discovery of the Mathematics of Chance*

We cannot understand the importance for Borel of his discovery of the problem of quantifying chance elements unless we start by stressing that for Borel this was principally a way in which he could bind together his various strands of scientific study in a mathematical endeavour. As so often happens, this meeting seems, if not entirely fortuitous, at the very least the unexpected development of a body of research into another field. This is what comes

out in the analysis that Borel himself wrote in *The real and imaginary in mathematics and physics*<sup>4</sup>: Here he gives an account of all his work dating from his thesis (1898) until the publication, in 1917, *Of Uniform monogeneous functions of complex variables*<sup>5</sup> which presented an approach towards the existence and construction of such functions the possibility of which was denied by Weierstrass<sup>6</sup>.

Even if Borel is largely regarded as the inventor of the theory of denumerable probability, he is not the sole initiator. Measure theory which stems from his work is the starting point, but the idea of using the techniques applied to quantifiable sets, based more or less directly on the legacy from Cantorian mathematics, on the formulation of a theory of probability would have otherwise taken longer to come out. Borel himself places its first appearance in the work of the Swedish mathematician, Anders Wiman<sup>7</sup> (1865-1959), author of a paper dating from 1901 with the title « Über eine Wahrscheinlichkeitsaufgabe bei Kettenbruchentwicklungen » (Concerning the question of probability relating to continued fractions), making the first steps in this direction:

*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 is sufficient to use the theory of those sets I called "measurable," and which Mr. Lebesgue had previously named "measurable (B)"; the use of this theory of measurable sets to the calculation of probability was first made, to my knowledge, by Mr. Wiman.<sup>8</sup>*

Wiman's linking probability with the theory of continued fractions which became, in Borel's mind, a few years later, a paradigm for quantified chance, is very significant and is worth examining more closely.

There was a regain in interest in the decomposition in continued fractions by mathematicians at the end of the 19th century. Let us quote the fundamental work by Stieltjès (1856-1894) on this question where he introduced Riemann's generalization of the integral which bears his name<sup>9</sup>. What interests us here more closely is the manner in which certain questions concerning continued fractions were put in the form of probabilistic terms. The origins of this seem to lie in the work of the Swedish astronomer Hugo Gylden (1841-1896) with his study of planetary movement around the Sun. To approximate this motion represented by a quasi-periodical function, Gylden considered Lagrange's techniques of approximation by continued fractions (this fundamental approximation technique was developed some years later by a student of Hermite, French mathematician Henri Padé (1863-1953), is known today as Padé approximants<sup>10</sup>. A smooth (analytical) function can be represented

$$f(t) = a_0 + \frac{t^{n_1}}{a_1 + \frac{t_2^n}{a_2 + \dots}}$$

4 L'imaginaire et le réel en mathématique et en physique

5 Fonctions monogènes uniformes de variables complexes

6 On this aspect see (Callens, 1990)

7 Some details on Wiman, are found in (Garding, 1991) p. 156-164. See also necrology of (Nagell, 1960)

8 « 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. »

9 See (Hawkins, 1975) pp.179 and seq.

10 See (Baker and Graves-Morris, 1996)

where the quotients  $a_n$  are nonnegative integers and Gylden was therefore led to study the structure of the decomposition in continued fractions of a real number  $x$  to which he devoted three papers dated 1888 (including 2 excerpts from letters to Hermite published by the latter as notes in the CRAS).<sup>11</sup> In one of the papers, Gylden chose a probabilistic approach in which he tried to specify the probability distribution of the quotients  $a_n$  for a number  $x$  drawn at random from  $[0,1]$ . More precisely, Gylden proved that the probability that  $a_n$  equals  $k$  is "more or less" inversely proportional to  $k$  which allowed him to deduce a property of convergence with probability 1 of some series describing the perturbations of planetary movements. This is an elegant result of almost certain convergence before the letter<sup>9</sup>.

In 1900 two astronomers from Lund, Tortsen Broden (1857-1931) and Wiman, resumed Glyden's work. In order to obtain the asymptotic probabilistic expression  $a_n=k$  in the form

$$\frac{1}{\ln 2} \ln \frac{1+1/k}{1+1/k+1}.$$

Wiman used Lebesgue's  $\sigma$ -additivity of measure (without, however, mentioning explicitly its use) and it is this use that Borel picks up on<sup>12</sup>. Unfortunately we do not know how Borel was informed of Wiman's work. There is no trace of any correspondence between them. There are several likely hypotheses. Firstly, one could believe that Wiman had sent his paper directly to Borel, perhaps via Mittag-Leffler (1846-1927). The latter had a number of exchanges with Borel during the year 1900 about contributions to the International Congress of Mathematicians in Paris, to which Borel had been appointed secretary. Another possible source is another member of the international network in the establishment where Borel found himself at that time. On 2 January 1904 his friend, the Scandinavian mathematician Ernst Lindelöf (1870-1946), wrote to introduce him his young colleague, the astronomer in Helsingfors, Karl Sundman (1873-1949) who had since found himself in Paris and wanted to make himself known to the French Mathematical Society. Now Sundman, as Lindelöf mentioned, was in charge of editing the unfinished works of Gylden. We have not been able to find any trace of a meeting between Sundman and Borel, but this must certainly have occurred and the young Finn would have been able to give a first hand account of the work of Wiman.

Borel's first incursion into the field of the mathematics of chance was modest in ambition limited to the treatment of "certain questions concerning probability" in five pages making six points.<sup>13</sup> After making clear the link he makes between probability and measure, Borel continues by proving the null probability of taking at random a rational number between 0 and 1 making use of Lebesgue's integral. Then, after reminding us of the basis of the notion of measurability he shows its value for examining several other probabilistic problems, incalculable by classical methods of integration

As we have already stated above, from the moment when he discovered these things, Borel went on to consider the example of continued fractions as a basic source when thinking about probability. A few years later, in 1909, in his famous resounding article for the review, "Il Circolo matematico di Palermo" Borel presented his application

<sup>11</sup> (Gylden, 1888a) and (Gylden, 1888b)

<sup>12</sup> For more details on the work of Gylden and Broden, see (von Plato, 1994), pp.29-31.

<sup>13</sup> (Borel, 1905)

of denumerable probability to decimal representations and continued fractions of real numbers and thereby achieves their most expansive development. In this article Borel introduces the notion of almost sure convergence and an initial version of the strong law of large numbers by which initiating the proof of the existence of an object by its value of 1 in probability. This kind of exposition became a central element in Borelian reasoning and stems directly from the manner in which Borel, fifteen years earlier, had introduced the practice of the measure of sets in his thesis; this consisted of showing the existence of an arc of a circle in which a particular series were converging uniformly and showing that he could choose the center of such an arc in a set of which he had proved that the measure of the complementary was null<sup>14</sup>. Following this, right from the start of his life as a probabilistic mathematician Borel uses the notion of a probabilistic occurrence 1 as a proof of existence. A good example of this can be found in the second chapter of the 1909 article (section13) where Borel comments the proof that almost each real number is absolutely normal<sup>15</sup>:

*"In the present state of science, the effective determination of absolute normal numbers seems to be the most difficult of problems; it would be interesting to resolve this either by creating an absolutely normal number, or by proving that amongst the group of numbers that could be effectively ascertained not one is absolutely normal. However paradoxical this suggestion may seem, it is absolutely not incompatible with the fact that the probability of a number being absolutely normal be equal to 1."*<sup>16</sup>

The strangeness of this kind of proof for existence compared to the classical methods was the reason for the strong law of large numbers and denumerable probability “caught the mathematicians by surprise”, as Von Plato amusingly expressed it. This kind of semi-intuitionism, “half-axiomatic, and half-constructivist” as Brouwer described it, became one of Borel’s mathematical trademarks but was received with a certain amount of skepticism<sup>17</sup>. Moreover, this greatly rekindled and increased the controversy that even Borel’s presentation of the measure of sets, had already aroused, in particular Schoenflies’ 1900 celebrated survey on the theory of sets<sup>18</sup>. For Schoenflies, measure theory as proposed by Borel had an overly subjective character because the definition of measurable sets seemed to be introduced *ad hoc* only in order to achieve his specific goal. For example the property of  $\sigma$ -additivity, which lies in the core of Borelian thought, could not, for Schoenflies, result legitimately from a simple definition. He wrote: “above all, it only has the nature of a postulate because we cannot decide if a property which can be verified by a finite sum, can be extended over an infinite number of terms by an axiom, but by deep

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<sup>14</sup> See (Hawkins, 1975) Section 4.2

<sup>15</sup> Let us recall that a real number is said to be *normal* if each figure between 0 and 9 appears with frequency 1/10 in its decimal decomposition. The number is said *absolutely normal* if the same is true for the representation in base d (with a frequency 1/d) for each integer d.

<sup>16</sup> « *Dans l'état présent de la science, la détermination effective d'un nombre absolument normal semble être le plus difficile des problèmes; il serait intéressant de le résoudre soit en construisant un nombre absolument normal, soit en prouvant que parmi les nombres qui peuvent être effectivement définis, aucun n'est absolument normal. Quelque paradoxale que cette proposition puisse paraître, elle n'est absolument pas incompatible avec le fait que la probabilité pour un nombre d'être absolument normal soit égale à 1.* » (Borel, 1909b) p.261.

<sup>17</sup> Voir (von Plato, 1994), p.57 and (Mazliak, 2009)

<sup>18</sup> (Schoenflies, 1900)

examination alone<sup>19</sup>.” In addition, Schoenflies criticised Borel for having no applications of his definition of a measure of sets other than the cases of analytical extensions dealt with in order to introduce them. This could equally explain the enthusiasm with which the French mathematician looked for an application of his theories in a field with such high potential for development as the measurement of chance and risk assessment.

Of course, it was the rapid development in the physics of that period that initially propelled Borel, as it had propelled Poincaré a few years earlier, towards the study of probability. Today we do not always appreciate the full measure of profound upheaval experienced by the scientific community, at the turn of the 20<sup>th</sup> century, created by the mathematics of chance expanding and entering into the theories of physics. A lecture by Paul Langevin dated 1913<sup>20</sup> gives its full measure, placing the introduction of probability in parallel to a radical change in the understanding of the structural laws of matter. Poincaré, somewhat grudgingly, was resigned to accepting a presence of randomness as a partially arbitrary convention leaving the scholar free to choose the one best suited to his calculations. He had shown how these random elements had the good sense to be asymptotically glossed over by the virtue of large numbers and did not damage the objective interpretation of results. The whole of chapter XI of *Science and Hypothesis* is dedicated to describing this manner of observation, by way of the famous case of the small planets in the zodiac and, more generally, the exposition of the method of arbitrary functions<sup>21</sup>. Poincaré concludes chapter XI with: “In order to take on any calculation in probability and even to make any sense of it, it is necessary to accept as a starting point a hypothesis or a convention which includes a certain arbitrariness. In choosing the convention we can only be guided by the principle of sufficient reason. (...) The probability calculations can be applied and put to good use with those problems that have results which are independent of the original hypothesis (...).”<sup>22</sup>

Borel’s 1905 article finishes by announcing a forthcoming note focused on questions concerning mathematics in physics. Indeed in 1906 there appeared in the *Annales scientifiques de l’École normale supérieure* the article “Concerning the principles of kinetic theory of gases<sup>23</sup>. While Borel immediately acknowledges his position of protégé under the patrician support of his illustrious elder, writing, “I really ought to quote from these inspiring pages [of “Science and Hypothesis”] at every occasion, the reading of which has proved very useful to me<sup>24</sup>” He does not fail to also point out how much his approach differed from Poincaré’s<sup>25</sup>. For Borel it was no longer a case of limiting himself to a convention. He had to satisfy the mathematicians’ thirst for intellectual rigour by showing how modern physics could manifest itself by being treated in a wholly

<sup>19</sup> “Sie hat zunächst nur den Charakter eines Postulats, da ja die Frage, ob eine Eigenschaft endlicher Summen auf unendlich viele Summanden ausdehnbar ist, nicht durch Festsetzung erledigt werden kann, sondern vielmehr der Untersuchung bedarf”, (Schoenflies, 1900), p.93

<sup>20</sup> (Langevin, 1913)

<sup>21</sup> (Poincaré, 1902)

About arbitrary functions and Poincaré’s conception of randomness, as well as its direct influence in the history of ergodic studies and Markov chains, one can consult (Bru, 2003) and (Havlova, Mazliak and Sisma, 2005)

<sup>22</sup> « Pour entreprendre un calcul quelconque de probabilité, et même pour que ce calcul ait un sens, il faut admettre, comme point de départ, une hypothèse ou une convention qui comporte toujours un certain arbitraire. Dans le choix de cette convention, nous ne pouvons être guidés que par le principe de raison suffisante. (...) Les problèmes où le calcul des probabilités peut être appliqué avec profit sont ceux où le résultat est indépendant de l’hypothèse faite au début » in (Poincaré, 1902) , chap. XI, *Conclusions*.

<sup>23</sup> « Sur les principes de la théorie cinétique des gaz ».

<sup>24</sup> « Je ne puis citer à chaque instant ces pages suggestives [de la Science et l’Hypothèse de Poincaré], dont la lecture m'a été fort utile ».

<sup>25</sup> (Borel, 1906). p.11, note (2).

mathematical manner. The model of mathematical probability outlined by Borel could be based on geometric structures as was shown in the title of the book he published in 1914, where he included his 1906 article as having been the start of his approach<sup>26</sup>. The Borelian approach had far reaching consequences in the subsequent history of probability, through the works of Lévy and Wiener on Brownian Motion<sup>27</sup>.

### ***Mathematician as citizen***

*"My husband comes in, puts down his old tattered leather briefcase on the table and says to me, "I have just won the Petit-d'Ormoy prize.*

*Awarded by the Academy of Science for work of a high standard, this prize is worth 10,000 Francs (the equivalent of two million of our old francs).*

*"What shall we do with this money?"*

*Emile Borel says to me, "How would you like if we realised one of your dreams?"<sup>28</sup>*

This dream that Borel's wife, Marguerite, herself mentions in her book of memoirs, released in 1968, became the founding of the *Revue de Mois*. While Borel remained, first and foremost, a mathematician, we shall now see how Borel, in that year of 1905, showed a lively enthusiasm for the exposition of research beyond the tight inner circle of university academia.

The first edition of the *Revue du Mois* appeared one year after this episode, in January 1906<sup>29</sup>. The new publication had ramifications that went beyond the original scope of the Borel family's dream project. For Borel it was a decisive step: his entry into the intellectual world beyond the sphere of mathematics. The Petit-d'Ormoy prize, created in 1875 to reward the achievement of a complete body of scientific research, was not officially awarded until the weekly meeting of the Academy on 1<sup>st</sup> December 1905<sup>30</sup>. The committee in charge of awarding the prize was made up of people close to Borel; starting with Paul Appel, his father in law, Marguerite's father; Emile Picard, uncle on Marguerite's side; and long-standing friends like Paul Painlevé. Other members, in addition to the close intimates of Borel, were often linked, to varying degrees, to the salons of the Appels, or the Borels, having gained entry, prior to his marriage to the daughter of the house: Camille Jordan, Gaston Darboux, Georges Humbert, and Henri Poincaré, who was the referee.

If we look more closely at the interval and chain of events that lead from Borel winning the Petit d'Ormoy prize to the setting up of the *Revue du Mois*, one cannot fail to

<sup>26</sup> (Borel, 1914)

<sup>27</sup> On this see (Mazliak, 2010)

<sup>28</sup> « Mon mari rentre, pose sur la table sa vieille serviette de cuir noir fatiguée et me dit : "Je viens de recevoir le prix Petit-d'Ormoy". Décerné par l'Académie des sciences pour des travaux de haute valeur, ce prix est de 10 000 francs (ce qui équivaut à deux millions de nos anciens francs). [...] "Que ferons-nous de cet argent ?" [...] Emile Borel me dit : "Veux-tu que nous réalisions l'un de tes rêves ?" » in (Marbo, 1968) p. 84

<sup>29</sup> Sur la création de la *Revue du Mois*, cf. (Mazliak, 2007)

<sup>30</sup> *Comptes rendus hebdomadaire des séances de l'Académie des Sciences*, Paris, 1905, semestre 2, 141, séance du 1<sup>er</sup> décembre 1905, p. 1140.

be struck by the speed of Borel's reaction. The committee in charge of awarding the prize was elected during the session of 1<sup>st</sup> May 1905<sup>31</sup>, and, as we have seen, the decision to award the prize to Borel was not made public until December. However, by 2<sup>nd</sup> June 1905 Borel made it known to Volterra his intention to launch a new review and had already sent him a pamphlet giving a main outline of the project<sup>32</sup>. Once we realise the links between Borel and several members of the committee, it is not impossible to surmise his knowledge of their choice prior to its announcement. Nevertheless, it is, admittedly, astonishing that in the space of a month the committee could meet, make its choice, inform Borel of their choice, and for him to have taken the decision and planned the project. Such speed tends to back up Camille Marbo's assertion of an old dream coming true. This could also lead to raising doubts as to the attribution of the *Revue du Mois* with the winning of the Petit d'Ormoy, to a less fortuitous explanation of its inception, and for an idea that began well before the winning of the prize.

The aims of the review were set out in a short text accompanying the first edition.

*"The number of problems that can be treated adopting scientific methods grows every day. It seemed possible to us to imagine a review which focused on these methods, not as a specialist publication but rather by aiming at the general development of ideas, and the exposition and critical appraisal of the advances in Knowledge and the resultant spread of ideas*

*The Revue du Mois attempts to be this review. It claims, above all, to be a review containing free discussion, allowing the free unhampered expression of opinions based on science. The titles of the articles that follow this statement testify to the breadth of its scope; the names of the authors are an assurance of the seriousness with which its remit shall be fulfilled."<sup>33</sup>*

While the articles chosen to be published were supposed to be treated "adopting a scientific method" and the expressed opinions were to be "based on science," a simple glance at the contents of the first few editions will amply reveal the breadth of interests of the review which dealt as much with literature<sup>34</sup> as with international relations<sup>35</sup>.

For Borel this meant finding a vehicle capable of conveying his widening interests and greater presence in public life. His letters moreover are evidence of his precocious appetite for politics, but the path that would eventually lead him to a deep involvement in national affairs was very gradual. With reference to the Dreyfus Affair; his closeness to such well known Dreyfusards like Painlevé or Appel<sup>36</sup>, as well as his letters, are concise,

<sup>31</sup> *Comptes rendus hebdomadaire des séances de l'Académie des Sciences*, Paris, 1905, semestre 1, 140, séance du 1<sup>er</sup> mai 1905, p. 1169.

<sup>32</sup> Lettre d'Emile Borel à Vito Volterra, 2 juin 1905

<sup>33</sup> "Le nombre et l'importance des questions qui peuvent être traitées par la méthode scientifique s'accroissent chaque jour. Il nous a semblé qu'on pouvait concevoir une publication dont cette méthode serait le principe, publication n'ayant rien de spécialement technique et prenant comme but essentiel de contribuer au développement des idées générales par l'exposition et l'étude critique des progrès réalisés dans la connaissance des faits et des mouvements d'idées qui en sont la conséquence.

La Revue du Mois tente d'être cette publication. Elle se propose avant tout d'être une Revue de libre discussion, admettant à s'exprimer en pleine indépendance toutes les opinions à base scientifique. L'étendue de son programme apparaît nettement à la lecture des titres d'articles ci-après ; les noms des auteurs sont une garantie que ce programme sera sérieusement rempli."

<sup>34</sup> (Van Tieghem, 1906)

<sup>35</sup> (Bourgeois, 1906)

<sup>36</sup> Evidence of the involvement of the two mathematicians with each in a letter from Charles Hermite to Götta Mittag-Leffler dated 7 march 1899 (reprinted in the *Cahiers du séminaire d'histoire des mathématiques*, 10, 1989, p. 58) : « Et puis l'Affaire Dreyfus qui s'y joint, comme par une véritable fatalité. Painlevé et Appell sont

even ample, evidence, and leave little doubt of the Dreyfusard leanings of the young mathematician. Thus Borel wrote to Volterra in march 1899, several weeks before the trial in Rennes :

*"With regard to national politics, I believe that the death of Felix Faure has cleared up matters and that, finally, the end of the Affaire is in sight. Nobody here doubts that the statement of the Cour de Cassation (Highest court of appeal) will be similar to the Criminal Court's and it will undoubtedly act with more authority on the war council, to whom the case will be handed."*<sup>37</sup>

Borel's closeness to radicalism, which did not establish itself officially until after the First World War, adds another feature to the year 1905, when the law separating the church and state was put into effect. In the first edition of the *Revue du Mois*, placed in second position, the article by Alfred Croiset, the historian of ancient Greece, on "The secular teaching of morality"<sup>38</sup>, set the desired tone of the review.

In addition it is the question of teaching and, in particular, secondary education, which brought Borel to overtly take a position on the political scene. His initial involvement took the form of textbooks for secondary school pupils, published by Armand Colin, from 1903 until 1910. In 1905 he had already published two books covering the first two years focusing on algebra (1903), an elementary course in trigonometry (1904) and geometry (1905)<sup>39</sup>. From 1904, accompanying this involvement in secondary education, there appeared, in parallel, his theoretical thoughts on the subject.

The lectures Borel gave at the *Musée pédagogique* in 1904 was an important step towards the politicisation of his work. Borel clarified and justified this, at length, in his introduction alluding to the fact that these matters were of "general interest and (touched on) problems that were current and vital"<sup>40</sup>. The concept of practical mathematics coming out of "mathematical laboratories", which he had already defended before, was echoed by Volterra in his opening article in the *Revue du Mois*, dedicated to "mathematics in the biological and social sciences".

## II- *The Prolusione, Italian and French contexts*

When Borel approached Volterra to participate in his new review, he gave him practically an open remit, which would tend to suppose that he gave as much importance to having the name of his illustrious correspondent on his review as to the very subject matter.

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du même bord dans cette affaire, j'y suis d'un autre, mais tous nous sommes également préoccupés et inquiets d'un mal général dont souffre la France, et dont nous ne voyons pas le terme ». For further information on the involvement of mathematicians in the Dreyfus Affair, consult (Duclert, 1999) and equally (Mansuy and Mazliak, 2005)

<sup>37</sup> « Quant à notre politique intérieure, je crois que la mort de Felix Faure l'a beaucoup éclaircie et que la fin de l'Affaire arrivera enfin. Personne ne doute ici que l'arrêt des chambres réunies de la Cour de Cassation ne soit dans le même sens qu'avait été celui de la chambre criminelle et il aura sans doute plus d'autorité sur le Conseil de Guerre auquel l'affaire sera renvoyée » Letter from Emile Borel to Vito Volterra, 3rd march 1899, Accademia dei Lincei, Archivio Volterra.

<sup>38</sup> (Croiset, 1905)

<sup>39</sup> See the complete chronological table of text books published by Armand Colin in (Guiraldenq, 1999), p. 58.

<sup>40</sup> « d'intérêt général et [touchaient] à des questions actuelles et vitales » (Borel, 1904), p. 107.

*"I would be very grateful if you would submit an article to us, either on a scientific subject, explained in a manner accessible to those with only a general knowledge of science (for example, engineers), or on questions concerning education; a study on the relationships between the technical schools and universities in Italy would certainly arouse a lot of interest."<sup>41</sup>*

Enclosed in the letter was a general pamphlet outlining the editorial strategy of the new review. The answer from Volterra arrived a few days later, and the text he offered was not completely new since it was the text of the lecture that he gave to the University of Rome in 1901.

*"Some time ago, I published in Italian the lecture I gave at the University of Rome on the new application of mathematics in the biological and social sciences. The paper did not travel beyond Italy and, if I say so myself, was received with some success by our biologists who were interested in these issues. Do you think the article could be translated into your magazine?"<sup>42</sup>*

The revisions to the original text in the French translation were minimal: the end of the lecture which maintained the Italian preeminence in the application of mathematics in these new fields was removed, being considered as too closely tied to the original circumstances in which the lecture was given.<sup>43</sup> The amendment referring to Anatole France, was equally reduced, a general presentation of the winner of the Nobel prize for literature being seemed no longer necessary in order to address the French readership. But setting these two details aside, the paper that was translated into the first edition of the *Revue du Mois* was practically identical to the one delivered to the University of Rome in 1901.

The acclaim that Volterra was flaunting came in two waves. While his lecture was only heard by lecturers and students in Rome, it was immediately transcribed into the *Annuario* (the yearbook) of the University of Rome and reprinted in the *Giornale degli economisti*<sup>44</sup>. The success amongst the Italian biologists that Volterra was mentioning happened later with the reprint of the article in the *Archivio di fisiologia*<sup>45</sup> in 1906, several weeks before it appeared in French. Thus the same text would have been reprinted not less than four times in four differing reviews: a university review, two technical reviews and a review of general interest. This would leave no doubt about the importance Volterra

<sup>41</sup> Je vous serais personnellement très reconnaissant si vous pouviez nous donner un article, soit sur un sujet scientifique exposé de manière à être accessible à ceux qui ont seulement une instruction scientifique générale (comme les ingénieurs), soit sur des questions d'enseignement ; une étude sur les relations entre les écoles techniques et les universités en Italie, intéresserait certainement beaucoup Letter from Emile Borel to Vito Volterra, 2nd June 1905, Accademia dei Lincei, Archivio Volterra, 72.

<sup>42</sup> J'ai publié en Italien il y a quelque temps une lecture que j'ai faite à l'Université de Rome sur les applications nouvelles des mathématiques aux sciences sociales et biologiques. Cet article n'a été répandu qu'en Italie mais j'ose dire qu'il a eu quelque succès auprès de nos naturalistes qui se sont intéressés à la question. Croyez-vous que l'article peut se traduire pour votre journal ? Draft of a letter from Vito Volterra to Emile Borel, dated 6th May 1905 but seemingly written on the 6th June 1905, Accademia dei Lincei, Archivio Volterra,

<sup>43</sup> Thus Volterra writes to Borel : "I have removed the final passages because I thought that while it is all right to finish a lecture with a wish it is not appropriate to do so in an article." « J'ai retranché complètement les dernières périodes parce que j'ai pensé que si l'on pouvait finir un discours en émettant un vœu, on ne pouvait pas le faire dans un article », letter dated 5th November 1905, Accademia dei Lincei, Archivio Volterra.

<sup>44</sup> *Giornale degli economisti*, série 2, novembre 1901, p. 436-458 : this version reprints the text of the Roman *prolusione*, including the passages excluded in 1905 (cf. previous note )

<sup>45</sup> (Volterra, 1906)

attached to the article, and about his willingness to diffuse it to a wider public than that of the mathematicians and universities.

The approach adopted around this paper mirrors the industry that had absorbed him since 1906: the renaissance of the Italian society for the improvement of sciences (SIPS). In this instance also, the strategy put into use by the mathematician consisted of bringing the sciences, in the widest sense possible, together, into the one body, of which mathematics was the spine. Both these projects had the dual effect of opening the different disciplines to each other centred on their common bases in mathematics.

The programmatic dimension of the text, which runs through the paper, and is fully expressed with its position at the opening of the review, is to be linked with the original circumstances in which it was delivered. Indeed, it first took the form of a *prolusione*, an inaugural discourse, delivered at the start of the academic year at a university by a newly appointed professor. It had the seriousness of that moment in the career of a lecturer when he spells out the scope of his program, and, at the same time, took part in the solemn formalities beginning the academic year.

While being named to deliver the inaugural lecture, the *prolusione*, was considered as an honour, Volterra accepted it as an additional task, as can be seen in a letter he wrote to Giovanni Vailati, his former assistant at the University of Turin.

*"Regardless of how much I have scorned it I have not managed to avoid the painful task of giving the inaugural lecture of the next academic year of the university. What can I do to maintain the interest of those who are involved in fields far from mathematics? I was considering doing something on the attempts to apply mathematics in biology and the social sciences. What do you think about this line of thought? Do you think it appropriate or would you replace it with something else? Just in case, what useful books do you know? Given that you know just about everything and are very up to date in sociology, and are able to give me the best advice, I would be eternally grateful to you if you could write to me on this"<sup>46</sup>.*

Borel had initially suggested an article concerning the relations between theoretical teaching in universities and technical education. Volterra was, actually, at that time, involved in the setting up of a *Politecnico* in Turin, having been appointed by the Senate to join together the museum of industry and the training school of engineering. The Italian mathematician avoided this suggestion, promising, however, to deliver an article on this, later on, which he finally never did. The change in subject does not seem to have bothered Borel who seemed pleased enough in his reply dated 8<sup>th</sup> June 1905, where he wrote that he would be "very happy to publish a translation [...] of the very interesting lecture"<sup>47</sup>. A somewhat cool, neutral reply which raises questions of whether Borel, who had limited comprehension of Italian, had actually understood the text.

<sup>46</sup> "Per quanto me ne sia schernito, non mi è riuscito di dispensarmi dal pesantissimo incarico di fare il discorso inaugurale al prossimo anno scolastico all'Università. Che fare per non riuscire privo di interesse fra cultori delle altre discipline fuori dalle matematiche? Avrei pensato di fare qualche cosa sui tentativi di applicazione delle matematiche alle scienze biologiche o sociologiche. Che dice di un argomento simile? Le va o lo sostituirebbe con qualche altro? Nel caso quali libri sono a sua conoscenza che mi potrebbero riuscire utili? Ella che sa tutto e che è fortissimo nelle scienze sociologiche mi potrà dare degli ottimi consigli e le sarò gratissimose mi vorrà scrivere qualche cosa in proposito" Letter from Vito Volterra to Giovanni Valati, 1er juillet 1901, quoted in (Guerragio and Paoloni, 2008), p. 86.

<sup>47</sup> « très heureux de publier une traduction [...] de l'intéressant discours » Letter from Emile Borel to Vito Volterra, 8 June 1906, Accademia dei Lincei, Archivio Volterra, 43.

The translation was given to Ludovic Zoretti<sup>48</sup>, doctor in mathematics and former principal librarian at the Ecole normale supérieure, who presented a first draft of the French text in mid-August 1906. The abundance of correspondence between Borel and Volterra during the second half of 1905 testifies to the degree with which the Italian mathematician followed each tiny detail of the work, up until his letter of 19<sup>th</sup> November 1905 where he consented to the printing of the article without further amendment<sup>49</sup>.

The article began with an assertion from Anatole France: “Never ask a wise man for secrets of the universe that do not appear in his shop window. It’s of no concern to him”<sup>50</sup>. So the article was an appeal to scientists, and mathematicians especially, to show more curiosity about things and to get more involved in the application of their calculations, in physics but also in economics and biology. The application of mathematics in these disciplines would correspondingly render them more capable of being appraised as sciences<sup>51</sup>. Thus Volterra described an interpretation of the history of science which correlated the advance of sciences with their mathematisation: the more a discipline lent its methods and results to mathematics, and the more it integrated these results into its procedures, the greater its scientific value. Mechanics appeared to typify and exemplify perfectly this development. Volterra cited the example from the biomechanical school of Hermann Helmholtz (1821-1894) where mathematics was used in experiments of human physiology in order to obtain laws in biophysics (most notably in acoustics). The results were essentially descriptive in nature, which, for him, showed that this school had not yet achieved a complete state of mathematisation. In the article Volterra placed economics with its recent developments first<sup>52</sup>, followed by a number of instances of mathematics being applied to biology. Taking each discipline in turn, he systematically went through each branch of mathematics from probability, through geometry and differential equations, and showing what each had to offer to the applied sciences.

The theory of probability did not occupy a great deal of space in the article but the arguments used were very strong. Right from the beginning there was “Galton’s attempts to measure numerically elements from the theory of organic evolution such as evolution and heredity,”<sup>53</sup> underlining how much Galton borrowed from Quetelet. Even if Volterra admitted that, “there is a lot to modify in what he has done,” he identified in this first application of statistics to biometry, “the dawn of a new day”<sup>54</sup>. But, above all, towards the end of the article Volterra devoted particular attention to probability where he presented it as one of the most promising field for mathematics:

*“That is where lies the most particular and striking branch of mathematics. If*

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<sup>48</sup> The path taken by Ludovic Zoretti (1880-1948) who was then a young doctor in mathematics is, moreover, particularly interesting. Having obtained first place in the Agrégation competitive examination in 1902, he engaged in a university career at Grenoble, then later in Caen. Member of the SFIO, activist in the general union of teachers (la Fédération générale de l’enseignement), he took a clear stance against the French entry into the war in 1939, which excluded him from the movement after the congress of Nantes. He then joined the “Rassemblement national populaire,” the collaborationist party of Marcel Déat, which caused him to be condemned to death, in absentia, and imprisoned until he died in 1948.

<sup>49</sup> Letter from Vito Volterra to Emile Borel, 19th November 1905, Académie des Sciences de Paris, archives personnelles d’Emile Borel, dossier Revue du Mois, n° 3.

<sup>50</sup> il ne faut jamais demander à un savant les secrets de l’univers qui ne sont pas dans sa vitrine. Cela ne l’intéresse point » (Volterra, 1906b), p. 1.

<sup>51</sup> This is set out clearly in 1906 in Volterra’s critical study of Pareto’s new manual of political economy: (Volterra, 1906c), reprinted in *Opere matematiche*, vol. III, p. 142.

<sup>52</sup> On the role of economics in the *proluzione* de Volterra, cf. the studies of (Guerraggio and Paoloni, 2008), p. 89-92.

<sup>53</sup> (Volterra, 1906b)

<sup>54</sup> « l’aube d’un jour nouveau », (Volterra, 1906b), p. 6.

*we examine what lies behind any personal decision we will always find, more or less hidden, a calculation in probability. One can say that, to some extent, the most ordinary man, who expects the sun to rise in the morning, owes his faith in the dawn to a unwitting application of Bernoulli's law of large numbers.*<sup>55</sup>

It would be very tempting to link Volterra's case for increasing the horizons of mathematicians, with his own personal forays, in the following years, notably in the field of biology. It is important to stress the gap in time that separates the 1901 lecture and Volterra's initial work in ecology and biology which date to the mid 1920s<sup>56</sup>. Volterra himself recognized this when he was editing the lecture where he, "went beyond the boundaries of [his] work"<sup>57</sup>. Similarly, Volterra had never directly practised the theory of probability, and, despite the interest he claimed to have had in their results, he never actually increased the application of the researches that he so much vaunted.

We must suppose, then, that the people he met and the meetings he attended during the editing of his lecture lead Volterra towards a field barely familiar to him. In this regard, Giovanni Vailati, Volterra's former assistant in Turin, who was then teaching in college in Bari, seems to have played a determining role. Starting with the choice of subject: Judith Goodstein quotes a letter from Volterra to Vailati where Volterra reveals his hesitations about finding a subject, "which would not bore the specialists in disciplines outwith mathematics"<sup>58</sup>. Then, later on, in the documents, when Volterra asked Vailati for further assistance in finding bibliographical references: Vailati's reply, arriving in July 1901, contained most of the references included in Volterra's lecture

*"Amongst the foreign non-mathematician writers most seriously interested in the scientific treatment of their subjects are; Galton (Francis), author of "Hereditary Genius" and some very particular studies on heredity; Pearson (Karl), whose "Grammar of Science" which I haven't yet read came out this year in a new edition; and Venn (John), a classic, whose authority is worthy of trust."*<sup>59</sup>.

Thus it is thanks to Vailati that Volterra discovered Galton and Pearson, and moreover, Vilfredo Pareto, referred to, at length, in the rest of the letter. Nor can we discount Volterra's own trips, and, in particular, his first stay in England in the summer of 1901, which gave him the opportunity to meet George Darwin, the youngest son of the author of *The Origin of Species*, allowing him to gain first hand information concerning the beginnings of biometry.

<sup>55</sup> « C'est là la branche la plus singulière et la plus curieuse des mathématiques. Si nous analysons un jugement quelconque de notre esprit, nous y trouverons toujours, plus ou moins dissimulé, un calcul de probabilité. On pourrait dire dans une certaine mesure, que l'homme le plus simple qui attend le matin le lever du soleil doit sa foi de voir surgir le jour à une application inconsciente de la loi des grands nombres de Bernoulli. » (Volterra, 1906b), p. 16.

<sup>56</sup> Volterra's interest in oceanography dates from around the end of the first decade with the setting up, in 1910, of the *Comitato talassografico italiano* (cf. (Linguetti, 2005) ), but Volterra's first articles on biological fluctuation appeared only in 1927.

<sup>57</sup> « Sort[ait] du cercle de [s]es travaux », in « Les mathématiques dans les sciences biologiques et sociales », *op. cit.*, p. 3.

<sup>58</sup> Letter from Vito Volterra to Giovanni Vailati, quoted in (Goodstein, 2007), p. 126.

<sup>59</sup> "Tra gli scrittori esteri, non matematici, che si sono occupati di tali argomenti con intenti più seriamente scientifici, conosco il Galton (Francis) autore dell'*Hereditary Genius* e di molte curiose ricerche sull'eredità [rietà]; il Pearson (Karl) di la cui *Grammar of Science*, che non ho potuto leggere finora, è uscito quest'anno [appunti] una nuova edizione, il Venn (John) classico da autorità degna di fiducia", Letter from Giovanni Vailati to Vito Volterra, Bari, 3rd July 1901, Accademia dei Lincei, Archivio Volterra, 20.

### III- Borelian reading and inheritance

Printing Volterra's text in the opening pages of the *Revue du Mois*, which had neither an introduction by the editors, nor a preface, was not an insignificant act. It testifies to Borel's willingness to continue himself, at least in part, the programmatical outline of the opening text. The symbolic stance of this position, moreover, can be shown, *a contrario*, by the criticism expressed by Henri Lebesgue, who always maintained a lot of skepticism towards the *Revue du Mois*<sup>60</sup>, and who, from 1906, concentrated his criticism on the preeminent position of Volterra's article.

*"I think it was a mistake to have started with Volterra's article. I do not deny that the article contains some things that are good, perhaps it is all good, but, and I am all the more convinced of my reproach because I am aware of often meriting it myself, Volterra, when he talks about things, presupposes a lot of prior knowledge from the reader, which is going too far. My fear is that, given that the articles in the 1st issue should allow the reader to see if the review is for him, Volterra's article has lead too many people to say that the review is not for them. Also, the article is a translation, and, despite all Zoretti's skills, it is still noticeable and this may put certain people off for whom, however, the review is made."*<sup>61</sup>

We can certainly suppose that the pride of place given to the article in the review owes something to protocol – the willingness of Borel to pay homage to his illustrious colleague, ten years his senior, and was a means to underline the international aspect of the review, respecting the diplomatic procedure of giving the first world to a foreigner dignitary. But Lebesgue's reaction points out that an editorial line has to be recognized by the reader as well as the editors, which allows everybody to know if the review “is for him”.

And we can certainly perceive in the article the initial sketch of what Stéphane Callens called the philosophy of “practical values”<sup>62</sup> in science initially articulated in the first article penned by Borel in the *Revue du Mois* in April 1906, with several hints present already in Volterra's article. Indeed, both authors shared an interpretation of mathematics existing in the sciences and in everyday life; an interpretation that is neither wholly instrumental nor utilitarian, no more than it is axiomatic. The object was to find a middle path between an absolute faith in the results of mathematics, consisting of an application

<sup>60</sup>See, for example, his scathing comment in a letter of 1909 to Borel: “In a nutshell, it is the Revue du Mois that I reproach you for. I understand well that for you it is the opportunity to invest your talents as a man of action and your administrative powers, but these are what I appreciate the least in you.” « Pour tout dire, je vous reproche la Revue du Mois. Je sais bien que vous trouvez là l'occasion de dépenser vos qualités d'homme d'action et vos ardeurs d'administrateur, mais c'est ce que j'estime le moins en vous », in (Dugac, 2007), letter 75, p. 159.

<sup>61</sup> *Je crois que c'est une maladresse d'avoir débuté par l'article de Volterra. Je ne méconnais pas que cet article ne contienne des choses très bien, peut-être est-il tout entier très bien, seulement, et je fais d'autant plus volontiers ce reproche que j'ai souvent conscience de le mériter, Volterra parle de beaucoup de choses en les supposant connues du lecteur, ce qui peut-être est exagéré. J'ai peur que, alors que les articles du n° 1 devraient permettre à chacun de voir si la revue est faite pour lui, l'article de Volterra ait conduit trop de gens à se dire que la revue n'était pas faite pour eux. Et puis l'article de Volterra est une traduction et, malgré l'adresse de Zoretti, cela se sent et pourra choquer certaines gens pour lesquelles, cependant, la revue est faite* Letter from Lebesgue to Borel, 16th January 1906, reprinted in *Cahiers du Séminaire d'histoire des mathématiques*, 12, 1991, p. 135.

<sup>62</sup> (Callens, 1997), p. 317-370.

without any judgment, and a skepticism consisting of positing a radical rupture between mathematical equations and problems of everyday life. This is the essence of a comment Borel makes in his article on “the practical value of probability calculations”:

*“Introducing a calculation into everyday decision-making too often is met with one of the following extreme judgments: for some it is absurd to interfere with calculations into decisions with elements that cannot be expressed in numbers, for others, numbers have a magical value giving infallible powers to those who employ them following the rules”*<sup>63</sup>.

The similarity of this with the work of Volterra in favour of an increased reasoned use of mathematics in the other sciences is confirmed, moreover, by the quotation he made, several months later, in his review of the recently published *Manual of economics* by Vilfredo Pareto, in the *Giornale degli economisti*<sup>64</sup>. This article appearing only a few months after Borel’s article on “practical values,” witnessed the convergence in thought of the two mathematicians on the question of the application of mathematics and its relationship with the rest of human knowledge. In particular, we cannot help but be struck by the similarity in the way the two mathematicians take on the question of the role of probability in decision making. Volterra’s declaration that “if we examine what lies behind any personal decision, we will always find, more or less hidden, a calculation in probability”<sup>65</sup>, is echoed, almost word for word, by Borel, three months later with « probability calculations play a part, more or less subconsciously, in all our decisions»<sup>66</sup>. The reciprocity of influence stemming from these two founding articles was confirmed when Volterra, on his part, borrowed the concept of the uncomensurability of probability and certainty in his critical essay on Pareto:

*“Mathematical economics, by virtue of its thorough resolution of problems whose limits are clearly defined in the domain, can deliver a positive numerical base, on which it can influence the actions to be taken in reality. This leaves open, however, the internal debate concerning the big issues of moral and political character which must assimilate these results.”*<sup>67</sup>

This statement by Volterra echoes the fundamental idea present in Borel’s article according to which the job of probability calculations is not to make a decision but to clarify it. Borel and, in the same manner, Volterra, had taken pains, right from the introduction, of getting rid of any “moral reasons” which could discourage participating in a profitable game or encourage participating in a mathematically disadvantageous one.

<sup>63</sup> « L’intervention du calcul dans les décisions de la vie pratique donne trop souvent lieu à l’un de deux jugements extrêmes : pour les uns, il est absurde de mêler le calcul à une décision dont certains éléments ne sont pas exprimables en chiffres ; pour d’autres, les chiffres ont une vertu magique qui rend infallible tous ceux qui les emploient suivant les règles. » (Borel, 1906c), p. 431.

<sup>64</sup> (Volterra, 1906c)

<sup>65</sup> « Si nous analysons un jugement quelconque de notre esprit, nous y trouverons toujours, plus ou moins dissimulé, un calcul de probabilité » VOLTERRA Vito, « Les mathématiques dans les sciences biologiques et sociales », *op. cit.*, p. 16.

<sup>66</sup> « Le calcul des probabilités intervient d’une manière plus ou moins consciente dans toutes nos décisions » (Borel 1906c ), p. 437.

<sup>67</sup> L’économie mathématique, en résolvant rigoureusement des problèmes bien déterminés dans un champs dont les limites sont nettement défini, doit nous offrir une base de données positives, sur laquelle elle puisse appuyer sûrement ses jugements sur les voies à suivre en pratique. Mais cela laisse ouverte la discussion interne sur les grandes questions de caractère moral et politique, auxquels ces résultats devront s’appliquer, (Volterra, 1906c), p. 142.

Similarly, Borel when dealing with a fictional character who has to make a strategic decision, arrived at the following conclusion :

*"Should he do it or not? The calculation cannot answer this question, but only clarify by measuring the chances he is taking ; its up to him to then decide if it is appropriate to act."*<sup>68</sup>

The reciprocal borrowings, following the publication Volterra's first article in the *Revue du Mois*, shows the repercussions it had for the two mathematicians, who followed, in the subsequent months, the agenda outlined by the program in the text. While Volterra's interest concentrated, for a while, on economic mathematics, the application of the program lead Borel, more and more, into the study of probability and its vulgarization.

Thus the *Revue du Mois* became an esteemed vehicle dedicated to the popularization of and debates concerning the mathematics of chance. During its ten years' existence, until it was put to sleep in 1916, Borel's review became the natural place of exchange where the subject or new theories were discussed, often by Borel himself.

For example, the principal arguments of the controversy between Borel and the biologist Felix Le Dantec took place in the *Revue du Mois*<sup>69</sup>. The latter chose the review to release an article<sup>70</sup> that put into question the very concept of the law of chance. While Le Dantec did not deny the possible applications of the law of large numbers, he thought of it as experimental and of no general worth. Edited by a biologist, this article was primarily directed to supporting Darwinism placing it into a Lamarckian perspective that considerably reduced the position of randomness in the process of evolution<sup>71</sup>. Le Dantec went right to denying any legitimacy to the idea of probability of a singular event "a concept that has neither rhyme nor reason"<sup>72</sup>. While Borel initially responded with various allusions during the lectures he delivered at the Sorbonne in 1909<sup>73</sup>, and while the polemics continued elsewhere, notably in the *Revue philosophique*, Borel revealed his position most completely in the *Revue du Mois* in an article with the evocative title: "Probability and M.Le Dantec"<sup>74</sup>.

Similarly, when Alfred Binet published a work concerning "revelations in writing", thus launching the basis for graphology as a scientific research, Borel is enthoused by the subject, seeing a possible application of the practical values of probability in the new discipline. Binet intended indeed to base the scientific value of graphology on a statistical study of the reliability of its results. Binet delivered letters and envelopes, in sufficient quantities, to the scrutiny of experts asking them to infer the age, sex and intelligence of the writers. He gathered the information and compared it with what would have been the results obtained in a random response to the questions asked. The originality of this approach consisted of establishing the scientific basis for graphology, not by proving its certainty, but in showing that it is more reliable than pure chance. "It is clearly more than pure chance, but it is hardly brilliant."<sup>75</sup> Borel concluded cheekily, in his review of the book where he did find, nevertheless, many valid points in methodology. This is why he appealed to the readers to take part and increase the samples in order do make the results

<sup>68</sup> « *Doit-il le faire ou non ? Le calcul ne peut répondre à cette question, mais seulement l'éclairer en précisant la nature de la chance qu'il court ; c'est à lui de décider ensuite s'il juge opportun de la courir.* »

<sup>69</sup> About this argument see (Bru, Bru and Chung, 1999)

<sup>70</sup> (Le Dantec, 1907)

<sup>71</sup> (Bru, Bru and Chung, 1999)

<sup>72</sup> (Le Dantec, 1907)

<sup>73</sup> (Borel, 1909)

<sup>74</sup> (Borel, 1911)

<sup>75</sup> (Borel, 1907)

more valid. And, more importantly, this is why he appeals to Binet to make use of his own data to continue his studies sumoning the Laplacian concept of increasing the reliability of observations by increasing the number of observations. He wrote to him, in June 1906, asking to consult the information he had collected with his colleagues and to use it as the basis of a study

*"Not everything is false according to Laplacian calculation. We must only apply them to simple, clear conclusions and not to infinitely complex criteria to be dismissed or condemned. I have often thought about these things, but I cannot see the practical means to approach them experimentally. I am wondering if it would be possible to make use of the numerous findings that you and your colleagues have observed."*<sup>76</sup>

The *Revue du Mois* adventure became Borel's opportunity to discover new aspects in the study of chance. Amongst these, statistics occupies a special place. At the time of his discovery, there was no work in France related to probability calculations contrary to what was beginning to happen in other countries like Germany, Italy and, especially, United Kingdom, where Pearson was developing his studies in biometry. This is particularly significant when we consider that, fifteen years later, Borel was the major instigator of mathematical statistics in France, and especially in realizing the fundamental role the methods of probability were destined to play in modern statistics<sup>77</sup>. In 1907 Borel had very interesting exchanges on this subject with Lucien March, director of the Statistique Générale de France, who became one of his most frequent contacts during the 1920s. Lucien March's reply on 16 July 1907 is the sole surviving letter of what was probably a more abundant series.

*"The application of the mathematical theory of probability to statistics raises issues to which you have most justly responded in the Revue du Mois. Observations of frequencies are made in statistics, and while there is no valid, logical reason to assimilate them as probabilities, under certain conditions, one can, in practice, guardedly treat them as probabilities. But, most of the time, it would be useful to assimilate them, as "variable probabilities" considered by Poisson. Bertrand declares their treatment too difficult for him, and, without doubt, deep down, really means to say that their research isn't rigorous enough. Nevertheless the study of variable probabilities leads to rules that are useful and that can be defended. Therefore, it would be desirable to have mathematicians picking up and continuing the work done by Poisson. Is not "being not rigorous enough" only another way of describing a research on a subject where some parts remain hidden? If we cannot shed light on all aspects of a subject, is this enough of a reason to leave the field unexplored? I hope that one of your students will be tempted by the subject."*<sup>78</sup>

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<sup>76</sup> "Tout n'est pas faux dans les calculs de Laplace. Seulement, il convient de les appliquer à des jugements simples et non à cette chose infiniment complexe qui est l'acquittement ou la condamnation d'un accusé. J'ai souvent réfléchi à ces questions, mais je ne voyais pas de moyen pratique de les aborder expérimentalement. Je me demande s'il ne serait pas possible d'utiliser dans ce but certains des nombreux faits que vos collaborateurs et vous avez observés.", Letter from Emile Borel to Alfred Binet, 24th June 1906, Académie des sciences, Archives Borel,

<sup>77</sup> On Borel's principal role in the resurgence of statistics in France, between the two wars see (Catellier and Mazliak, 2011)

<sup>78</sup> [L]es applications à la statistique de la théorie mathématique des probabilités soulèvent des objections auxquelles vous avez très justement répondu dans la Revue du Mois. En statistique on observe des fréquences

While Borel does not seem to have committed any student in the field of statistics at that time, he himself was interested enough to develop his thoughts on the matter. In this it is worth noting that Borel, in 1908, published one of the first note in the *Comptes Rendus de l'Académie des Sciences* listed as Statistical Mathematics<sup>79</sup> where he showed the use of analytical techniques of approximation of functions considerably improved the rigid classification of Pearson's biometric curves, curves that were probably brought to his attention by the *Prolusione*.

In 1908, Borel published in the *Revue du Mois* an article with the intriguing title *Probability calculations and the standpoint of the individual Le calcul des probabilités et la mentalité individualiste*, which, in our opinion, is the key to the Borelian approach to the science of quantified chance and its role in society. Volterra, in his *prolusione*, had insisted on the service probability calculations could have in science, in spite of the fact that its "principles are not maintained rigorously and are constantly open to criticism and discussion/argument." "principes ne sont pas posés rigoureusement et sont constamment ouverts à la critique et à la discussion". "However little credence we give to its basis, we must acknowledge that probability theory has and continues to give incalculable and uncontested service to all of the sciences"<sup>80</sup> wrote Volterra, going on to give some details on the theories developed by Quetelet and by Galton-Pearson, for whom the practice of probability is especially adapted to deal with "a very large number of small agents acting simultaneously and impossible to distinguish one from another."<sup>81</sup> Borel, as we have seen, emphasized the practical aspect of the theories, and, in his article, he tried to understand why this antipathy rest mostly on a misunderstanding. Borel wrote, "it would be desirable to clear up this misunderstanding, for the popularization of the results, if not the methods, of this branch of science is of great service to society"<sup>82</sup>". According to Borel, the problem with using statistics to deal with human issues is that it ignores individual qualities in order to consider the mass. However, "everyone of us appreciates especially those qualities we attribute to our individuality, and takes part in that spirit of individualism which is sometimes seen as the privilege of certain members of the elite."<sup>83</sup>" Mankind does not like "being counted solely as a member of a group without being identified as an individual. There, already, is a reason for the lack of popularity of statistics<sup>84</sup>. Moreover, the calculus

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et, s'il n'est pas légitime logiquement de les assimiler à des probabilités, pratiquement on peut sous certaines réserves traiter ces fréquences comme des probabilités. Mais, le plus souvent, il y aurait intérêt à orienter l'assimilation du côté des probabilités variables étudiées par Poisson. Bertrand déclare leur étude trop difficile pour lui, et au fond il veut dire sans doute que cette étude manque de rigueur. Cependant la considération des probabilités variables conduit à des règles utiles et défendables. Aussi serait-il désirable que des mathématiciens voulussent bien reprendre et continuer les travaux de Poisson, quand même les théories manqueraient quelque peu de rigueur. Manquer de rigueur, n'est-ce pas travailler un sujet dont certaines parties restent dans l'ombre ? Si nous ne pouvons porter la lumière en tous points est-ce une raison suffisante pour laisser le champ inexploré ? J'espère que quelqu'un de vos élèves sera tenté par le sujet."

<sup>79</sup> (Borel, 1908)

<sup>80</sup> "Quelque créance que nous accordions à ses bases, il faut avouer que la théorie des probabilités a rendu et rend à toutes les sciences des services incalculables et incontestables"

<sup>81</sup> un très grand nombre de petites causes agissant simultanément et qu'il est impossible de distinguer les unes des autres

<sup>82</sup> . "Il serait désirable que ce malentendu soit dissipé, car la vulgarisation des conclusions, sinon des méthodes, de cette branche de la science est d'une grande utilité sociale

<sup>83</sup> "chacun de nous tient particulièrement à tout ce qui constitue son individualité et participe ainsi plus ou moins à cette sensibilité individualiste que l'on a parfois signalée comme l'apanage de quelques esprits d'élite".

<sup>84</sup> . "être compté seulement comme une unité dans un groupe sans être individuellement désigné. C'est déjà là une raison pour que la statistique ne soit pas populaire".

of probability, which “*is not the same as statistics*”, claims its scientific credentials from making predictions which are offensive to primal psychological sentiments of human liberty. But while calculations cannot arm us against risk and misfortune and while “arguments to console those who suffer from social inequality<sup>85</sup>” cannot be found in statistics or calculations, their worth is inestimable for interpreting and, in so-doing, advising action. Borel wrote, “We should not look here for moral arguments or urgent reasons for action : but only, as with the physical sciences, a means of getting to know the past and predicting, with some approximation, the future.<sup>86</sup>” (we underline). The comparison with physics seems particularly significant. Borel was truly trying to have probabilistic methodology accepted by the social sciences in the same fashion as when he had been trying to have the major propositions concerning the kinetic theory of gases in his 1906 article. And he did not hesitate to comment ironically on those who refused the offer of his services.

*“Perhaps ignorance is convenient for those ostrich-types ;it is never desired by those who prefer to see things clearly and do not let themselves be influenced by the more accurate knowledge they may gain about the nature of a possible danger, while its probability is remarkably less than the dangers the most timid of men unwittingly expose themselves to, every day.We have nothing to fear from calculations as long as we decide not to be governed by their warnings without firstly weighing up their proper value :it is an extraordinary illusion to believe that the independence of the individual is accrued by ignorance.”<sup>87</sup>*

Thus, concludes Borel, probability calculations and its application to *social mathematics* go against the most antisocial aspects of a badly thought-out individualism, which is none other than an “unintelligent egoism.” They achieve this by constantly bringing to the foreground the status of Man . belonging to a society and acting within it. It therefore follows that the study and practice of these different techniques, over and beyond their scientific goals of analysis and prediction, have the virtue of limiting “the excesses of the individualistic mentality”. In its stead, it promotes the values of social solidarity, so transparently linked to the radical politics that had got into power in France at the turn of the 20<sup>th</sup> century.

## Conclusion

The *Prolusione* delivered by Volterra in 1901 came in the form of an exposé, essentially descriptive, concerning the involvement of mathematical practices in new scientific fields like economics and biology. This talk, presented at the solemn inauguration of an academic year, necessarily maintained a certain formality and was aimed at university academics of the most diverse disciplines. It is curious that the applications mentioned by Volterra had little to do with the work he was involved in at the time.: economics always

<sup>85</sup> “arguments pour consoler ceux qui souffrent des inégalités sociales”

<sup>86</sup> “*On ne doit y chercher ni arguments moraux, ni raisons immédiates d’agir: mais seulement, comme dans les sciences physiques, un moyen de bien connaître les événements passés et de prévoir avec une certaine approximation les événements futurs*”

<sup>87</sup> *L’ignorance peut être commode pour ceux qui pratiquent cette politique d’autruche; elle n’est jamais désirable pour ceux qui préfèrent voir clair et ne se laissent pas influencer par la connaissance plus exacte qu’ils acquièrent d’un danger possible, lorsque sa probabilité est notamment inférieure à celle des dangers inconnus auxquels les hommes les plus timorés s’exposent tous les jours. On n’a rien à redouter du calcul lorsqu’on est décidé à ne pas régler sa conduite sur ses indications sans les avoir au préalable pesées à leur juste valeur: c’est une illusion singulière de penser que l’indépendance individuelle est accrue par l’ignorance.*

remained at the edge of his interest, and he was involved in research linked to biology only much later. The decision to talk about “mathematics in the social sciences and biology” was based primarily on a desire to show off little known recent work carried out in Italy; next, display their exemplary quality in order to demonstrate the degree of mathematisation of a subject as the criteria to gauge its scientific validity.

As for Borel, doubtless he found in Volterra’s text ample material to back up his own program to reveal the sciences, and especially mathematics, to a wide public. This program of education through the sciences married well with his political inclinations, close to radicalism, and the privileged forms often associated with Borelian mathematics heavily centred on techniques to increase accuracy. The science of chance, discovered in various forms, during 1905, became for Borel, from then on, the instrument of choice at the service of the citizen to wrestle with the question of risk in society.

What followed in Borel’s career and the consequences of the First World War had a major impact on the subsequent instigation of the program. It was during the conflict that Borel became most keenly aware of the importance of statistics and how they are employed in the art of government. He, himself, moreover, became more closely involved with the political players. This lead him all the way up to being nominated *secretary* in the Paul Painlevé government. At the end of the war, Borel used the powers of his political and scientific offices to create new structures of teaching and research in France, where, in the foreground, was the science of randomness. The Institut de Statistiques de l’Université de Paris (ISUP) was created in 1922 followed in 1928 by the Institut Henri Poincaré (IHP)<sup>88</sup>.

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<sup>88</sup> On that subject, one may consult (Catellier and Mazliak, 2011)

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