Poincaré and probability

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Abstract The paper is devoted to Poincaré's work in probability. The starting point for the discussion is Poincaré's intervention in the Dreyfus Affair. Although works on probability do not represent a large part of the mathematician's achievements, they provide 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. The general historical context of this evolution is presented.

Keywords Henri Poincaré · Probability · Mathematical physics · Statistical mechanics · History of mathematics

Mathematics Subject Classification 01A55 · 01A70 · 60-03 · 37A60

In 1906, Poincaré signed one of the most unusual texts of his scientific career [10], a report written for the *Cour de Cassation* in order to close the Dreyfus Affair. In 1904, 10 years after the condemnation and demotion of the unfortunate captain and all that had followed, the French government had decided to bring to an end this lamentable

story, which had torn French society for years, and obtained the rehabilitation of the young officer who had been so unjustly martyred. As is well known, the accusation of 1894 had been proclaimed despite a total absence of material proof, during a hasty and biased trial complacently orchestrated by the military hierarchy, whose aim was to provide a culprit as soon as possible. The only concrete document was the famous *bordereau* found in a wastepaper basket in the German Embassy in Paris and briefly scrutinized by several experts of greater or lesser competence. Among them, Alphonse Bertillon played a specially sinister role, and afterwards he became Dreyfus's most obstinate accuser. He built a bizarre edifice of self-fabrication (*autoforgerie*) of the *bordereau*, a theory having a pseudo-scientific presentation, in order to *prove* the guilt of the innocent captain. Bertillon became trapped by his own conviction, more out of self-confidence and stupidity than from a real partisan spirit. When the Affair broke out at the end of the 1890s, and the political plot became obvious and Dreyfus's innocence apparent, Bertillon unceasingly complicated his theory to prove Dreyfus's guilt. This frenzy resulted in an avalanche of troubles for him and came close to ending his career in the Paris Police department. However, for the trial in the *Cour de Cassation*, in order to silence any last dissenting voices which might be raised, it was decided not to ignore Bertillon's rantings, but to ask experts of unquestionable academic authority 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, everybody knew that only Poincaré had actually worked on the document, a tedious task he undertook with honesty but not without complaint. Besides, this was not Poincaré's first intervention in the

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Dreyfus case: in 1899, Painlevé, who came to Rennes to give evidence at the trial reviewing the 1894 judgement of the court-martial, had read from the witness box a letter of Poincaré in which he expressed a harsh opinion about the lack of scientific foundation for Bertillon's work.

This story is well known and has been narrated in detail many times [13, 14, 29] and I shall not dwell on it further. However, one may ask why Poincaré had been called for this task. A first answer immediately comes to mind: in 1906, Poincaré, then fifty-two, was without a doubt the most prominent of 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 won the important prize of the King Oscar II of Sweden in 1889; the publication of semi-popular books on the interpretation of science brought him real popularity, and he was also famous due to several papers which appeared in newspapers, for instance during the International Congress of Mathematicians in Paris in 1900, at which Poincaré had been the main authority. To call on such a personage in order to quash the insignificant but noisy Bertillon was therefore a logical calculation on the part the government. However, a second and more hidden reason probably played also a part. The report for the *Cour de Cassation* [10] opens with a chapter whose title was quite original in judicial literature: *Notions sur la probabilité des causes* (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 seriously answering him was only possible by confronting the so-called expert with his own weapons. It was therefore necessary not only to call a scientific star for the job, but also someone whose authority in these matters could not be challenged. In 1906, Poincaré was unquestionably regarded by everyone as the leading specialist in the mathematics of randomness in France. It is true that at the time he no longer held chair of the calculus of probability and mathematical physics at the Sorbonne, but he had held it for some 10 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 textbook on the calculus of probability which, in 1906, was still the primary textbook on the subject in French. Moreover, several publications had presented his ideas on the presence of randomness in modern physics to a large cultivated audience, in particular his *Science and Hypothesis* [23], which enjoyed a great success. It was therefore as a specialist in the calculus of probability that Poincaré was called by the judicial authorities, to help them to finally dispose of the Dreyfus case.

If we go back in time 15 years before this event, we cannot but be struck by a contrast. Since 1886, although Poincaré held the Sorbonne chair just mentioned, there is no doubt that he essentially focussed on the part of mathematical physics. For instance, he had signed several publications with the qualification *Henri Poincaré, professeur de Physique mathématique*. In 1892, he published an important textbook on thermodynamics [16] based on the lectures he had given at the Sorbonne several years before. Naturally, no publication by Poincaré ever went unnoticed, and one attentive reader had been the Scottish 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* [31]; the review was quite negative despite the obvious talent Tait recognized in his young French colleague. In Tait's criticisms of Poincaré's book, we can recognize a comment sometimes aimed by Anglo-Saxon scientists at the works of their French counterparts (who, whenever possible, never missed an opportunity to respond in kind): to put it briefly, the Anglo-Saxons often think that the French are too formalist and remote from experiment, or even despise it, while the French think that the Anglo-Saxons are too obsessed with a practical approach to problems, without reflecting enough on the underlying tectonic structure. Thus, according to Tait, Poincaré 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 English 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 ignorance 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 20 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; the

tones used are rather sharp, each sticking to his position. On 17 March, Poincaré wrote the following comment about the main 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.¹

We can thus see that Poincaré, in 1892, had a very negative vision of statistical mechanics, the field where the principal emergence of probabilities in the description of matter was located at the end of nineteenth century. 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 teach the kinetic theory of gases to his students. 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 published his first paper on the kinetic theory of gases [20], 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. This was the subsequent basis of his book of 1896, the same year when he exchanged his chair for the chair of celestial mechanics. Moreover, in the years that followed reflections on the mathematics of randomness appeared more frequently in his writings, up to the publication of his more philosophical works, which acknowledged the integration of the theory of probability into Poincaré’s kit of mathematical tools. By 1906, as already noted, the transition had been completed, especially as new elements, such as Einstein’s newly published theory of Brownian motion, made the increasing presence of probability in scientific theories even more necessary.

Here I will discuss the probabilistic aspects of Poincaré’s enormous production, aspects which comprise a rather limited part of his enormous output. One significant

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 onto this rather marshy terrain. 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 (see [11]).

Despite this limited contribution, Poincaré succeeded in leaving a significant legacy 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 its rather miserable position in the French academic world for more than half a century.

We shall take a closer look at the evolution of Poincaré’s thinking in the 15 years that have just been mentioned, in the attempt to understand how was this gradual taming of mathematical randomness came about. Poincaré’s life and writings made (and for sure will make again) a lot of ink flow. It will therefore not surprise anyone that many of the topics discussed in the present paper have already been discussed several times before. In particular, I shall refer several times to Sheynin’s text [30], to von Plato’s publications [32, 33], to the book [8], as well as to several papers by Bru, some published and some not [5, 6].

In the beginning was the Chair. As we will see, it was not only by through fortuitous scientific interest that Poincaré first came to be involved in probability theory, but also as a result of 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 Appel (his nephew), Emile Picard (his son-in-law) and of course Henri Poincaré, who Hermite considered the most brilliant of the three, 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: in the space of 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. By the end of the whirlwind, the average age of the professors of mathematics and physics at the Sorbonne had been reduced by 18 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 [1] and here I will touch on only the most important aspects insofar as they relate to our story.

¹ ‘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’.

The chair of calculus of probability and mathematical physics, occupied until 1882 by Briot, had been created some 30 years before, after numerous unsuccessful attempts by Poisson at creating it, but in a form different than the one he had hoped for. 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, due in large measure to some of the works of Laplace and, above all, to Poisson himself dealing with the application of probability in the judicial domain. The book by Poisson [28] ignited a dispute in the Academy in 1836, when Dupin and Poincaré harshly contested Poisson's conclusions, and the philosophers led by Victor Cousin made a scene *au nom des droits sacrés de la liberté contre les prétentions avancées par les mathématiciens pour calculer comment survient un événement dans le domaine moral* (in the name of the sacred rights of liberty against the claims advanced by the mathematicians in order to explain how events occur in the moral domain). John Stuart Mill summed it up by referring to 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.

Though Lippmann was nominated for the chair in 1885, Jamin's unexpected demise liberated the chair of research in physics, which Lippmann took over immediately, leaving a position free for Poincaré. One might be initially surprised by this choice, first of all because Poincaré was preferred to other candidates, less gifted of course, but whose area of research was closer to that of the position. One would rather have expected, for instance, to see Poincaré in one of the chairs of analysis. In 1882, when Briot died and the great game began, Poincaré had no published work in physics (let alone the calculus of probability), while among the candidates who were eliminated, there was for instance Boussinesq, who had significant contributions in that domain to his credit, 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 would be later Poincaré's successor in this same chair when Poincaré moved 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 [1], a number of indications demonstrate that Poincaré had indeed desired this particular position. An examination of his lectures in 1887–1888 shows that he already possessed 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 to support 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 expectations.

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 [16], 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 [12]. 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 [20], published in 1894, though he again expressed some skepticism, Poincaré, whose conventionalism was formed during these years, already seemed 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.²

In this article Poincaré proposed a new formulation of the ergodic principle, introducing the restriction of exceptional initial states. The most interesting point is that Poincaré seemed to come up with this idea in his previous work on the three body problem, which had earned him the prize of the King of Sweden. In the memoir presented for the prize,

² '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' [18, p. 513].

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 [15, pp. 71–72]; this passage was included some years later in his treatise of new methods for celestial mechanics [17–19] 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, published in 1896 and transcribed by Albert Quinet, a former student of the Ecole 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 [21]. 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). The book, in its edition of 1896, is a natural successor to Bertrand’s textbook [3], which had been the usual textbook of reference up to then (see [7]). Compared it to Bertrand’s book, Poincaré’s book consolidates the material in several interesting ways, an aspect that is even more apparent in the second edition [26], completed by Poincaré some months before his death in 1912.

Poincaré now seemed 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 devoted considerable effort towards this 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 and specialized language as possible) and innovation. Two of them are of particular importance: that of 1899 [22]—reprinted as a chapter of [23]—and that of 1907 [24]—reprinted first as [25] and then as a Preface for the second edition of his textbook [26]. These two texts signal Poincaré’s desire to show off his new probabilistic credo. But one has to realize that Poincaré wanted above all to convince himself, leading to the question Jean-Paul Pier used ironically as the title for his paper [27]: did Poincaré believe in the calculus of probability? Without pretending to give a definitive answer, one may observe that Poincaré had very honestly sought for a demarkation of the zone where it seemed to him that the use of 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: Where is it legitimate to let randomness intervene? What definition can be given of probability? What mathematical techniques can be developed as useful tools for physics, in particular for the kinetic theory of gases? Borel would later write about Poincaré’s position on this in [4].

In his 1907 text [24], Poincaré accurately defined the way in which he considered it legitimate to resort to the notion of randomness. He saw essentially three origins of 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; Poincaré, born in 1854, was the child of a century in which Laplace had been a tutelary figure. However, Poincaré knew well the accusations accumulated against Laplace’s theory and proposed several ways to adjust it: randomness, even if 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 ease. 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 all right; we must only apply the rules of arithmetic and algebra and we go until the end of the computation and our result does not leave any place to doubt. But, if we want to use it for any application, we need to prove that our convention was legitimate, and we shall face the difficulty we thought to have avoided.³

In a remarkably creative achievement, Poincaré forged a method allowing the objectification of some probabilities. Using it, if one considers for instance a roulette wheel with alternating black and red sectors, even without having the slightest idea of how it is put into motion, one may show it reasonable to suppose that after a large number of turns, the probability that the ball stops in a red zone (or a black one) is equal to 1/2. There are thus

³ ‘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 avons cru éluder’ [20, p. 262].

situations where one can go beyond the hazy Laplacian principle of (in)sufficient reason as a necessary convention for determining 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 place 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.

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 standing as the pre-eminent French authority in the theory of probability, he was, as far as his scientific thought was concerned, somewhat on the fence between a completely deterministic description of the world and our modern conceptions where randomness enters as a fundamental ingredient. Poincaré maintained this uncomfortable position until the end of his life. It is also worth noting that at the precise moment when Borel—so strikingly!—was, so to speak, taking over, Poincaré seems not to have been particularly interested in the enterprises of his young follower. Nor was he had been interested by the fortunate experiments of the unfortunate Bachelier: he had written, it is true, a benevolent report on his thesis [2] and had occasionally helped him to obtain grants, but the two men had no scientific contact afterwards [9]. Even more surprising, Poincaré seems to have been thoroughly unfamiliar with the works of the Russian School (Chebyshev, Markov, Lyapounov, etc.); this explains why he became never aware of some of their connections with his own works. Poincaré’s probabilistic studies therefore leave us with an impression 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 the upheavals which occurred after his departure.

Appendix: The Dreyfus Affair

Still suffering from the after-effects of the Franco-Prussian war (1870–1871), the French Third Republic, divided between pro-republicans and pro-monarchists, found itself facing the infamous Dreyfus Affair. The case exploded in 1894 following the presumed treason by Alfred Dreyfus, a captain in the French artillery, of Alsatian Jewish descent. Dreyfus was accused of acting as a spy for the German Empire, and after a trial that took place behind closed doors, he was court-martialled and condemned for life to hard labour. He was exiled to the penal colony of Devil’s Island, in French Guiana.

The case was reopened in 1896 by Colonel Georges Picquart, the new chief of the Army’s Office of Information. Picquart presented a report to his superiors in which Captain Dreyfus’s innocence was proved. Colonel Picquart was removed from his office and sent to Tunisia. This gave rise to a vehement outcry in the press in favour of Dreyfus. Many radical intellectuals of the day supported the accused captain, including the writer Émile Zola, who published a famous open letter in 1898 entitled ‘J’accuse!’ (I accuse!) addressed to Félix Faure, president of the French Republic, in the literary journal *L’Aurore*. The French army responded by arresting Picquart and trying and convicting Zola for defamation of the armed forces. This led to a violent anti-Semitic campaign in the French press, with riots breaking out against Jews, democrats and liberals.

Dreyfus was tried by another military court in Rennes in 1899, one in which a letter by Henri Poincaré played an important role. The atmosphere surrounding the trial was heavy with pressure and threats to the judges and attorneys. Dreyfus was condemned to 10 years, an extravagant conviction for ‘treason with extenuating circumstances’. In reality, the baseless nature of the accusations had been amply demonstrated during the course of the trial, but the military court was under pressure from the French Army (whose credibility had been seriously damaged by the affair) to confirm the original conviction. The court’s decision was not unanimous, but had a majority of five to two.

To resolve the obvious injustice of the trial, Dreyfus was offered the possibility to appeal for mercy, even though this implied an admission of guilt. To avoid further political and social tension, Dreyfus and his attorneys accepted the proposal, and in 1898 Dreyfus was pardoned by the President of the Republic, Émile Loubet.

Complete exoneration of all charges finally arrived in 1906, following a final trial in which Poincaré’s intervention was again significant. Dreyfus went on to fight for France in World War I, and was made an officer in the Légion d’honneur in 1919. He died in 1935.

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