Cournot in English

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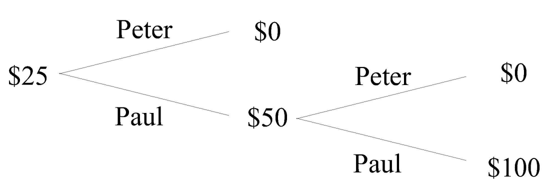

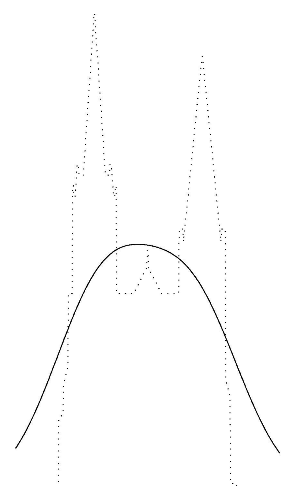

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Abstract

Trained as a mathematician, the French scholar Antoine Augustin Cournot (1801–1877) is remembered as a philosopher and economist. Unfortunately, his 1843 book on the philosophy of probability and statistics was never translated into English.

Here I provide English translations of a few passages from Cournot's books. To provide context, I also quote a few earlier and later scholars.

A complete translation of Cournot's 1843 book by Oscar Sheynin is available at `http://sheynin.de/download/cournot.pdf`.

1 Introduction

Cournot sought to understand the relation between pure and applied mathematics—how mathematics can describe the natural and social worlds. His work on economics was pioneering. His work on the philosophy of probability and statistics is relevant to contemporary debates in philosophy, statistics, and economics but remains under-appreciated, in part because it was never translated into English.

The analysis of duopoly in Cournot's first book, *Recherches sur les principes mathématiques de la théorie des richesses* ([4], 1838), is now seen as the first appearance of game theory in economics but was little noticed at the time. As he said in the preface of an 1861 book [7] in which explained the same ideas with less mathematics,

> When you try to go against the conventional wisdom, you either make a revolution (which fortunately happens very rarely), or else you attract hardly any notice, and this is what happened to me.[1]

The 1838 book did gain more notice after Cournot's death in 1877. An Italian translation appeared in 1878. An English translation appeared in 1897, with commentary by the American economist Irving Fisher.[2] A German translation appeared in 1924, a Japanese translation in 1982.

Cournot's book on the philosophy of probability and statistics, *Exposition de la théorie des chances et des probabilités* [5], appeared in 1843. No English translation has ever been published. A German translation appeared in 1849 and a Russian translation in 1970. The book discusses conditions under which mathematical probabilities (which are *prima facie* subjective, since the mathematical theory of probability is about betting) can gain an objective meaning. Cournot discouraged the use of mathematical probability in judicial and philosophical settings, where he thought they can have only a subjective meaning. In these realms, he argued, one must deal with non-numerical *philosophical probabilities*.

Stephen M. Stigler, mathematical statistician and historian of statistics, wrote in 1986 ([30], page 196) that Cournot's *Exposition*

> ... was immensely wise without being profoundly original. He was proudest of the contribution the book made to the philosophical understanding of probability, and indeed he did achieve a much higher level of clarity than did any of his predecessors in his discussions of distinctions between subjective and objective probability.

As this appraisal suggests, Cournot brought no new mathematical methods into statistical work. But his philosophy of probability had an originality that is still

[1] The French orginal: "Quand on veut aller contre les habitudes prises, ou l'on fait une révolution (ce qui heureusement est fort rare), ou l'on n'attire point l'attention, et c'est ce qui m'est arrivé."

[2] Only two of Cournot's many books have appeared in English. The other was his *Essai sur les fondements de nos connaissances et sur les caractères de la critique philosophique* ([6], 1851), which was translated as *An essay on the foundations of our knowledge* in 1956.

under-appreciated. His understanding of multiple testing and confidence limits was deeply original and not matched by later authors for nearly a century. His arguments against putting philosophical probabilities into Bayesian terms are relevant to contemporary Bayesian philosophy. His understanding of objective probability has a depth and nuance missing in most contemporary expositions of non-Bayesian (*Bernoullian* or *frequentist*) statistics.

Cournot further developed his understanding of probability and his broader philosophy of science in a series of later books, written during a career as a university professor and administrator. This work, like his work in economics, became better known after his death. It was influential during the early 20th century among French philosophers and among French and Russian mathematicians working on probability. His collected works, edited by a team of French philosophers and mathematicians, appeared in thirteen volumes from 1973 to 2010 [10]. Thierry Martin published an extensive bibliography of work by and about Cournot in 2005 [23]. There are no recent appraisals of his work in English, but relatively recent appraisals in French include those by Martin [21] and Bertrand Saint-Sernin [27]. See also [22, 31].

This note documents Cournot's originality by providing passages from his work in English translation, along with quotations from a few earlier and later scholars. Except when otherwise noted, translations are mine. In the case of translations from French and German, I reproduce the original immediately following each translation.

2 Before Cournot

Nearly all 18th century European scientists and philosophers, whatever their religious convictions, subscribed to the doctrine of necessity: every event has a cause that makes its happening necessary. Believers and deists differed from atheists only on whether the chain of causation begins with God – the supreme intelligence and primer mover. Here I repeat widely quoted passages from three scholars cited by Cournot: Jacob Bernoull, David Hume, and Laplace.

2.1 Jacob Bernoulli (1655–1705)

Bernoulli's celebrated book on probability, *Ars Conjectandi*, was published posthumously in 1713. The following passages, all from Part IV, are reproduced from Edith Sylla's translation.

From Chapter 1:

> The *certainty* of anything is considered either *objectively* and in itself or *subjectively* and in relation to us. Objectively, certainty means nothing else than the truth of the present or future existence of the thing. Subjectively, certainty is the measure of our knowledge concerning this truth.
>
> In themselves and objectively, all things under the sun, which are, were, or will be, always have the highest certainty. This is evident,

concerning past and present things, since, by the very fact that they are or were, these things cannot not exist or not have existed. Nor should there be any doubt about future things, which in like manner, even if not by the necessity of some inevitable fate, nevertheless by divine foreknowledge and predetermination, cannot not be in the future. Unless, indeed, whatever will be will occur with certainty, it is not apparent how the praise of the highest Creator's omniscience and omnipotence can prevail. Others may dispute how this certainty of future occurrences may coexist with the contingency and freedom of secondary causes; we do not wish to deal with matters extraneous to our goal.

...

Something is *morally certain* if its probability comes so close to complete certainty that the difference cannot be perceived. ...

Something is *necessary* if it cannot not exist, now, in the future, or in the past. ...

A thing that can now, in the future, or in the past *not* exist is *contingent* (either *free* depending on the will of a rational creature, or *fortuitous* and *haphazard* depending on accident or fortune). This should be understood with reference to a remote rather than proximate power; nor does contingency always exclude all necessity even with respect to secondary causes. Let me clarify this by examples. It is most certain, given the position, velocity and distance of a die from the gaming table at the moment when it leaves the hand of the thrower, that the die cannot fall other than the way it actually does fall. Likewise, given the present condition of the atmosphere, given the mass, position, motion, direction, and velocity of the winds, vapors, and clouds, and given the laws of the mechanism according to which all these things act on each other, tomorrow's weather cannot be other than what in fact it will be. Indeed, these effects follow from their own proximate causes no less necessarily than the phenomena of eclipses follow from the motion of heavenly bodies. Yet is customary to count only the eclipses as necessary and to count the fall of the die and future weather as contingent. The only reason for this is that those things which, to determine the subsequent effects, are supposed as given, and which indeed are given in nature, are not yet sufficiently known to us. ... [S]o contingency also mainly has reference to our knowledge, insofar as we see no contradiction in something not existing in the present or future, even if, here and now, by the force of a proximate cause unknown to us, it may necessarily exist or be produced.

From Chapter II:

> *Because ... it is rarely possible to obtain certainty that is complete in every respect, necessity and use ordain that what is only morally certain be taken as absolutely certain.* It would be useful, accordingly,

> if definite limits for moral certainty were established by the authority of the magistracy. for instance, it might be determined whether 99/100 of certainty suffices or whether 999/1000 is required. . . .

2.2 David Hume (1711–1776)

The celebrated Scottish philosopher David Hume (1711–1776), thought to be irreligious, sought to reconcile necessity with free will. Here are two frequently quoted passages from his *Enquiry Concerning Human Understanding* [16], first published in 1748.

From 6.1:

> THOUGH there be no such thing as *Chance* in the world; our ignorance of the real cause of any event has the same influence on the understanding, and begets a like species of belief or opinion.

From 8.25:

> It is universally allowed, that nothing exists without a cause of its existence, and that chance, when strictly examined, is a mere negative word, and means not any real power, which has any where, a being in nature.

2.3 Pierre Simon Laplace (1749–1827)

This famous passage is drawn from the first pages of Laplace's *Essai philosophique sur les probabilités* [19], which first appeared in 1814.

> All events, even those that seem too trifling to depend on the grand laws of nature, follow from these laws as surely as the rotation of the sun. Being ignorant of their connections with the whole system of the universe, we have supposed that they depend either on final causes or on chance, depending on whether they happen and follow each other in a regular way or in no apparent order. But these imagined causes have been progressively pushed back as the boundaries of our knowledge have expanded, disappearing entirely in the face of sound philosophy, which sees in them only the expression of our ignorance of the true causes.
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> . . .
>
> So we should regard the present state of the universe as the effect of its previous state and the cause of its following state. An intelligence that could take in at any given instant all the forces that animate nature and the situation of each of the beings that compose it, were it also vast enough to analyze all these data, would put into a single formula the movements of the largest bodies of the universe and those of the smallest atom. For it, nothing would be uncertain; the future, like the past, would be present to its eyes. The

human mind, in the way it has perfected astronomy, offers a feeble resemblance to this intelligence. ...

. . . .

Tous les événement, ceux même qui par leur petitesse semblent ne pas tenir aux grandes lois de la nature, en sont une suite aussi néssaire que les révolutions du soleil. Dans l'ignorance des liens qui les unissent au système de l'univers, on les a fait dépendre des causes finales, ou du hasard, suivant qu'ils arrivaient et se succédaient avec reégularité, ou sans ordre apparent; mais ces causes imaginaires ont été successivement reculées avec les bornes de nos connaissances, et disparaissent entièrement devant la saine philosophie, qui ne voit en ellles que l'expression de l'ignorance où nous sommes des véritables causes.

...

Nous devons donc envisager l'état présent de l'univers comme l'effet de son état antérieur, et comme la cause de celui qui va suivre. Une intelligence qui, pour an instant donné, connaîtrait toutes les forces dont la nature est animée, et la situation respective des êtres qui la composent, si d'ailleurs elle était assez vaste pour soumettre ces données à anlyse, embrasserait dans la même formule les mouvements des plus grands corps de l'univers et ceux du plus léger atome: rien ne serait incertain pour elle, et l'avenir comme le passé serait présent à ses yeux. L'esprit humain offre dans la perfection qu'il a su donner à l'astronomie une faible esquisse de cette intelligence.

. . . .

We find similar passages in a memoir Laplace read to the French Academy of Sciences in 1773 ([18], page 113) and in his mentor Condorcet's *Essais d'analyse*, published in 1768 ([3], page 5). Earlier antecedents are discussed by Bernard Bru [1], Roger Hahn [15], and Marij van Strien [32]. See also [2].

3 Passages from Cournot

The passages translated here come from these books by Cournot:

- *Recherches sur les principes mathématiques de la théorie des richesses* (Research on mathematical principles of the theory of wealth), 1838 [4].
- *Exposition de la théorie des chances et des probabilités* (Exposition of the theory of chances and probabilities), 1843 [5].
- *Traité de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire* (Treatise on the progression of fundamental ideas in science and history), 1861 [7].
- *Considérations sur la marche des idées et des événements dans les temps modernes* (Considerations on the succession of ideas and events in modern times), 1872 [8].
- *Matéralisme, Vitalisme, Rationalisme: Études sur l'emploi des données de la science en philosophie* (Materialism, Vitalism, and Rationalism: Studies on the use of scientific discoveries in philosophy), 1875 [9].

3.1 Cournot's principle

According to Cournot's principle, probability acquires objective scientific content only by the predictions it makes, and it makes predictions by assigning some events small or zero probability. Cournot said this most succinctly in Section 43 of his 1843 book:

> *The physically impossible event is therefore the one that has infinitely small probability*, and only this remark gives substance— objective and phenomenal value—to the theory of mathematical probability.
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> ....
>
> *L'événement physiquement impossible est donc celui dont la probabilité mathématique est infiniment petite*; et cette seule remarque donne une consistance, une valeur objective et phénoménale à la théorie de la probabilité mathématique.
>
> ....

One might suppose that an infinitely small probability is exactly zero, but Cournot interpreted the idea more broadly. He explains this, for example, in 1875 ([9], Section IV.4), when he discusses the impossibility of standing a needle on its point:

> In practice, moreover, and in the world of realities, what geometers call an infinitely small probability is and can only be an exceedingly small probability. The tip of this very sharp needle is not a mathematical point like the apex of the cone in question. Viewed through a magnifying glass, it becomes a *blunt* tip. With whatever care we polish the plane of steel or agate on which we try to balance it, very delicate experiments will show roughness and streaks. It follows that the probability of success in putting the needle in equilibrium is no longer infinitely small, that it is only excessively small, as would be the probability of rolling an ace a hundred times with an unloaded die, which is enough for us to judge, with no fear of being refuted by experience, that the equilibrium is physically impossible.
>
> The same remarks apply to the market value of commercial chances. ...
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> ....
>
> En pratique d'ailleurs et dans le monde des réalités, ce que les géomètres appellent une probabilité infiniment petite, n'est et ne saurait être qu'une probabilité excessivement petite. La pointe de cette aiguille si effilée n'est pas un point mathématique comme le sommet du cône en question. Elle devient une pointe *mousse*, regardée à la loupe. Avec quelque soin qu'on ait poli le plan d'acier ou d'agathe sur lequel on essaie de la faire tenir en équilibre, des expériences très délicates y indiqueront des aspérités et des stries. Il en résulte que la probabilité de réussir à mettre l'aiguille en équilibre n'est plus à la rigueur infiniment petite, qu'elle n'est qu'excessivement petite, comme le serait la probabilité d'amener l'as cent fois de suite avec un dé non pipé: ce qui suffit pour que l'on juge, sans crainte d'être démenti par l'expérience, que l'équilibre est physiquement impossible.
>
> Pareille remarque s'applique aux valeurs vénales des chances mises dans le commerce. ...
>
> ....

On other occasions, Cournot repeatedly cited the event that one might be killed by a tile falling from a roof as an event of such small probability that it can be considered impossible and neglected.

The name *Cournot's principle* was introduced by Maurice Fréchet, and Cournot's originality in stating the principle was also acknowledged by other Continental probabilists in the first half of the twentieth century; see [29, 28].

3.2 Multiple testing

Cournot's principle gives us a way of testing probabilistic theories. We test such a theory by selecting an event to which it assigns small probability; the theory is discredited if the event happens. But it is not legitimate to test repeatedly with the same data. Cournot dealt with this issue in 1843 ([5], Section 111) as follows:

> ... Clearly nothing limits the number of the aspects under which we can consider the natural and social facts to which statistical research is applied nor, consequently, the number of variables according to which we can distribute them into different groups or distinct categories. Suppose, for example, that we want to determine, on the basis of a large number of observations collected in a country like France, the chance of a masculine birth. We know that in general it exceeds 1/2. We can first distinguish between legitimate births and those outside marriage, and as we will find, with large numbers of observations, a very appreciable difference between the values of the ratio of masculine births to total births, depending on whether the births are legitimate or illegitimate, we will conclude with very high probability that the chance of a masculine birth in the category of legitimate births is appreciably higher than the chance of the event in the category of births outside marriage. We can further distinguish between births in the countryside and births in the city, and we will arrive at a similar conclusion. These two classifications come to mind so naturally that they have been an object for examination for all statisticians.
>
> Now it is clear that we could also classify births according to their order in the family, according to the age, profession, wealth, and religion of the parents; that we could distinguish first marriages from second marriages, births in one season of the year from those in another; in a word, that we could draw from a host of circumstances incidental to the fact of the birth, of which there are indefinitely many, producing just as many groupings into categories. It is likewise obvious that as the number of groupings thus grows without limit, it is more and more likely *a priori* that merely as a result of chance at least one of the groupings will produce, for the ratio of the number of masculine births to the total number of births, values appreciably different in the two distinct categories. Consequently, as

we have already explained, for a statistician who undertakes a thorough investigation, the probability of a deviation of given size not being attributable to chance will have very different values depending on whether he has tried more or fewer groupings before coming upon the observed deviation. As we are always assuming that he is using a large number of observations, this probability will nevertheless have an objective value in each system of groupings tried, inasmuch as it will be proportional to the number of bets that the experimenter would surely win if he repeated the same bet many times, always after trying just as many perfectly similar groupings, providing also that we had an infallible *criterium* for distinguishing the cases where he is wrong from those where he is right.

But usually the groupings that the experimenter went through leave no trace; the public only sees the result that seemed to merit being brought to its attention. Consequently, an individual unacquainted with the system of groupings that preceded the result will have absolutely no fixed rule for betting on whether the result can be attributed to chance. There is no way to give an approximate value to the ratio of erroneous to total judgments a rule would produce, even supposing that a very large number of similar judgments were made in identical circumstances. In a word, for an individual unacquainted with the groupings tried before the deviation δ was obtained, the probability corresponding to that deviation, which we have called Π, loses all objective substance and will necessarily carry varying significance for a given magnitude of the deviation, depending on what notion the individual has about the *intrinsic importance* of the variable that served as the basis for the corresponding grouping into categories.

. . . .

. . . Il est clair que rien ne limite le nombre des faces sous lesquelles on peut considérer les événements naturels ou les faits sociaux auxquels s'appliquent les recherches de statistique, ni, par suite, le nombre des caractères d'après lesquels on peut les distribuer en plusieurs groupes ou catégories distinctes. Supposons, pour prendre un exemple, qu'il s'agisse de déterminer, d'après un grand nombre d'observations recueillies dans un pays tel que la France, la chance d'une naissance masculine qui, en général, comme on le sait, surpass 1/2: on pourra distinguer d'abord les naissances légitimes des naissances hors mariage; et comme on trouvera, en opérant sur de grands nombres, une différence très-sensible entre les valeurs du rapport du nombre des naissance masculines au nombre total des naissances, selon qu'il agit d'enfants légitimes ou naturels, on en conclura avec une probabilité très-grande que la chance d'une naissance masculine, dans la catégoire des naissances légitime, surpasse sensiblement la chance du même événement, dans la catégoire des naissances hors mariage. On pourra distinguer encore les naissances dans les campagnes des naissances dans les villes, et l'on arrivera à une conclusion analogue. Ces deux classifications s'offrent si naturellement à l'esprit, qu'elles ont été un objet d'épreuve pour tous les statisticiens.

Maintenant il est clair qu'on pourrait aussi classer les naissances d'après l'ordre de primogéniture, d'après l'âge, la profession, la fortune, la religion des parents; qu'on pourrait distinguer les premières noces des secondes, les naissances survenues dans telle saison de l'année, des naissances survenues dans une autre saison; en un mot, qu'on pourrait tirer d'une foule de circonstances accessoires au fait même de la naissance, des caractères, en nombre indéfini, qui serviraient de base à autant de systèmes de distribution catégorique. Il est pareillement évident que, tandis que le nombre des coupes augmente ainsi sans limite, il est *à priori* de plus en plus probable que, par le seul effet du hasard, l'une des coupes au moins offrira, pour le rapport du nombre des naissances masculines au nombre total des naissancees, dans les deux catégories opposées, des valueurs sensiblement différentes. En conséquence, ainsi que nous l'avons déjà expliqué, pour le statisticien qui se livre à un travail de dépouillement et de comparaison, la probabilité qu'un écart de grandeur donnée n'est pas imputable aux anomalies du hasard, prendra des valeurs très-différentes, selon qu'il aura essayé un plus ou moins grand nombre de coupes avant de tomber sur l'écart observé. Comme on suppose toujours qu'il a opéré sur de grands nombres, cette probabilité ... n'en aura pas moins, dans chaque système d'essais, une valeur objective, en ce sens qu'elle sera proportionnelle au nombre de paris que l'expérimentateur gagnerait effectivement, s'il répétait un grand nombre de fois le même pari, toujours à la suite d'autant d'essais parfaitement semblables, et si l'on possédait d'ailleurs un *criterium* certain pour distinguer les cas où il se trompe des cas où il rencontre juste.

Mais ordinairement ces essais par lesquels l'expérimentateur a passé ne laissent pas de traces; le public ne connaît que le résultat qui a paru mériter de lui être signalé; et en conséquence, une personne étrangère au travail d'essais qui a mis ce résultat en évidence, manquera absolument de règle fixe pour parier que le résultat est ou non imputable aux anomalies du hasard. On ne saurait assigner approximativement la valeur du rapport du nombre des jugements erronés qu'elle portera, au nombre des jugements portés, même en supposant très-grand le nombre des jugements semblables, portés dans des circonstances identiques. En un mot, la probabilité que nous avons appelée Π, et qui correspond à l'écart δ, perdra, pour la personne étrangère aux essais qui ont manifesté cet écart, toute consistance objective; et, selon l'idée que cette personne se fera de la *valueur intrinsèque* du caractère qui a servi de base à la division catégorique correspondante, elle devra porter des jugements différents, la grandeur de l'écart signalé restant la même.

. . . .

Cournot discussed this issue further in Sections 102 and 112–114, concluding that judgement about the meaningfulness of such observed differences is ultimately a matter of philosophical (non-numerical) probability. His views on the topic were mentioned by Edgeworth in 1887 [11] and by John Venn 1888 (in the third edition of his *The Logic of Chance* ([33], pages 338–339).

3.3 Cournot's superior intelligence

Whereas Laplace's superior intelligence can predict every detail of the future with precision, Cournot imagined a superior intelligence that can sometimes give only probabilities. He explained this clearly in 1843 ([5], Section 45):

> Thus often repeated is Hume's thought, "that properly speaking there is no such thing as chance, but there is its equivalent: our ignorance of the real causes of events."[3] Laplace himself posited at the beginning of his book the principle "that probability is relative in part to our knowledge and in part to our ignorance", from which it

[3]See Section 2.2 for Hume's original words in English.

follows that for a superior intelligence that can sort out all the causes and follow all the effects, the science of probability would disappear for lack of an object. But all these thoughts miss the mark. Surely the word *chance* designates not a substantial cause, but an idea: the idea of the combination of many systems of causes or facts that develop, each in its own series, each independently of the others. An intelligence superior to man would differ from man only in erring less often or not at all in the use of this idea. It would not be liable to consider series independent when they actually influence each other in the causal order; inversely, it would not imagine a dependence between causes that are actually independent. It would distinguish with greater reliability, or even with rigorous exactness, the part due to chance in the evolution of successive phenomena. ...In a word, it would push farther and apply better the science of those mathematical relations, all tied to the idea of chance, that become the laws of nature in the order of phenomena.

It is right to say ... that chance governs the world, or rather that it has a part, and a notable part, in the government of the world. This does not conflict at all with the understanding we should have of a providential prime mover. Either the prime mover is concerned to determine only average and general results, assured by these same laws of chance, or the prime mover uses details and particular facts to coordinate these results following purposes that go beyond our sciences and our theories.

....

Ainsi l'on a souvent répété cette pensée de Hume, "qu'il n'y a point de hasard à proprement parler, mais qu'il y a son équivalent : l'ignorance où nous sommes des vrais causes des événements"; et Laplace lui-même pose en principe au commencement de son livre, "que la probabilité est relative, en partie à nos connaissances, en partie à notre ignorance"; d'où il suit que, pour une intelligence supérieure qui saurait démêler toutes les causes et en suivre tous les effets, la science des probabilités s'évanouirait faute d'objet. Mais toutes ces pensées manquent de justesse. Sans doute le mot de *hasard* n'indique pas une cause substantielle, mai une idée : cette idée est celle de la combinaison entre plusieurs systèmes de causes ou de faits que se développent, chacun dans leur série propre, indépendamment les uns des autres. Une intelligence supérieure à l'homme ne différerait de l'homme qu'en ce qu'elle se tromperait moins souvent que lui, ou même ne se tromperait jamais dans l'application de cette donnée de la raison. Elle ne serait pas exposée à regarder comme indépendantes, des séries qui s'influencent réellement, dans l'ordre de la causalité, ou inversement, à se figurer une dépendance entre des causes réellement indépendantes. Elle ferait avec une plus grande sûreté, ou même une exactitude rigoureuse, la part qui revient au hasard dans le développement des phénomènes successifs. ...En un mot elle pousserait plus loin que nous et appliquerait mieux la science de ces rapports mathématiques, tous liés à la notion du hasard, et qui deviennent des lois de la nature; dans l'ordre des phénomènes.

Il est juste de dire ... que le hasard gouverne le monde, ou plutôt qu'il a une part, et une part notable, dans le gouvernement du monde; ce qui ne répugne en aucune façon à l'idée qu'on doit se faire d'une direction suprême et providentielle: soit que la direction providentielle ne porte que sur les résultats moyens et généraux, assurés par les lois mêmes du hasard; soit que la cause suprême dispose des détails et des faits particuliers pour les coordonner à des vues qui surpassent nos sciences et nos théories.

. . . .

3.4 Independent causal chains

In Cournot's picture, chance and objective probability arise from the intersection of independent causal chains ([5], Section 40):

> Events brought about by the combination or confluence of phenomena that belong to series that are independent in the causal order are those we call *random* or the result of *chance*.
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> . . . .
>
> Les événements amenés par la combinaison ou la rencontre de phénomènes qui appartiennent à des séries indépendantes, dans l'ordre de la causalité, sont ce qu'on nomme des événements *fortuits* ou des résultats du *hasard*.
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> . . . .

Here is one of the examples he gave ([5], Section 41):

> A man who does not know how to read draws type-setting letters from an unordered pile. In the order that he draws them, they spell *Alexander*. This is a random event or a result of chance, for there is no connection between the causes that directed the man's hand and those that gave the name *Alexander* to a famous conqueror . . .
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> . . . .
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> Un homme qui ne sait pas lire extrait un à un des caractères d'imprimerie entassés sans ordre: ces caractères, dans l'ordre où il les amène, donnent le mot *Alexandre*. C'est une rencontre fortuite ou un résultat du hasard; car il n'y a nulle liaison entre les causes qui ont dirigé la main de cet homme, et celles qui ont imposé le nom d'*Alexandre* à un conquérant fameux . . .
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> . . . .

In 1861 ([7] 1861, Section 59), he gave an example in which he weighs a rock using a balance and standard weights:

> Suppose I want to weigh a rock that has come into my hands, and at first my only weights are kilograms. All I can do with my balance is to observe that the weight is between three and four kilograms. I will write the number 3 somewhere to remind myself that the rock's weight is 3 kilograms plus part of a kilogram.
>
> Now I acquire some hectogram weights, putting myself in a position to determine in the same way the number of hectograms in the partial kilogram. I again obtain the number 3, which I write to the right of the previous 3. But without needing to perform this second weighing, I was quite sure in advance that there is no necessary connection between the number already obtained and the one I was going to obtain. What connection could there be between the causes that gave this piece of rock the weight it has at the moment I weigh it and the reasons that gave the French legislator the idea

of using the kilogram as the unit of weight and subdividing it by decimals? So for the number of hectograms I did not expect to find any particular digit of our decimal system any more than any other. If I obtain the number 3 for a second time, which seems to make the combination more notable, it is no less random. It will happen, as we say, *by chance*. It follows that the idea of chance is the idea of the intersection of facts rationally independent of each other, an intersection that is nothing but pure fact, to which we can assign neither law nor reason.

. . . .

Supposons que je veuille déterminer le poids d'une pierre qui me tombe sous la main, et que je n'aie d'abord à ma disoposition que des kilogrammes. Tout ce que je pourrai faire, ce sera de constater avec la balance que le poids de la pierre est compris entre trois kologrammes et quatre kilogrammes; j'écrirai quelque part le ciffre 3 pour me rappeler qu'il entre dans le poids de la pierre 3 kilogrammes, plus une portion d'une kilogramme.

Je me procure des hectogrammes, et je suis en état de déterminer de même combien il entre d'hectogrammes dans la portion de kilogramme dont il s'agit: j'obtiendrai ainsi un chiffre 3, que j'écrirai à la droite du chiffre 3 trouvé d'abord. Mais, sans avoir besoin de faire cette nouvelle pesée, je suis bien certain à l'avance qu'il n'existe aucune liaison nécessaire entre le chiffre déjà trouvé et celui que je vais obtenir, car, quelle liaison peut-il y avoir entre les causes qui ont donné à ce fragment de roche le poids qu'il a au moment où je le pèse, et les raisons qui ont suggéré au législateur français l'idée de prendre le kilogramme pour unité de poids, et de le subdiviser suivant la progression décimale? Je ne m'attendrai donc pas à trouver pour le chiffre des hectogrammes tel des dix chiffres de notre numération décimale plutôt que tel autre. Si j'obtiens une second fois le chiffre 3, ce qui semblera donner à la combinaison plus de singularité, la recontre n'en sera pas moins accidentelle et fortuite; elle arrivera, comme on dit, *par hasard*, d'où cette conséquence que l'idée de hasard est l'idée d'une reconcontre entre des faits rationellement indépendantes les uns des autres, recontre qui n'est elle-même qu'un pur fait, auquel on ne peut assigner de loi ni de raison.

. . . .

3.5 On Bernoulli and Bayes

Exposition [5] 1843, Section 86:

In the case of random events whose conditions have not been set up by man, the nature and operation of the causes that determine the chances for different outcomes or the probability law for the various values of a random quantity are almost always unknown, or so complicated that we cannot analyze them rigorously or calculate their effects. . . .

It is therefore necessary, for the application of the theory of chances, to be able to determine through experience, or *a posteriori*, those chances whose direct determination from the data of the question now surpasses and apparently will always surpass the reach of calculation. It follows from what we have said so far that Jacob Bernoulli's principle leads to this experimental determination. For if, writing x for the unknown chance an event will happen and n for the number of times it happens in m trials, we can always obtain a probability P that the random difference $x - \frac{n}{m}$ will fall within the limits $\pm l$ (the number l and the difference $1 - \mathrm{P}$ being less than any given magnitude if the numbers m and n are sufficiently large), the probability x can clearly be determined with indefinite precision provided nothing limits the number of trials. We can end up being sure, for example, that there is not a hundred-thousandth difference between the ratio $\frac{n}{m}$ given by experience and the unknown number x. At any rate, the existence of larger difference, although technically possible, would be an event of the type that we consider with good reason to be physically impossible and so may leave aside in explaining phenomena.

At this point, relying on the theorems of Jacob Bernoulli, who had already understood perfectly their meaning and significance, we could move on immediately to the applications made of those theorems in the science of data and observations. But a rule that was first stated by the Englishman Bayes and upon which Condorcet, Laplace, and their successors wanted to construct the doctrine of *a posteriori* probability has become a source of numerous ambiguities that we must first resolve, serious errors that we must correct. They correct themselves as soon as we have present in our minds the fundamental distinction between probabilities that have an objective existence, giving the measure of the the possibility of things, and subjective probabilities, depending in part on our knowledge and in part on our ignorance, varying from one intelligence to another, according to their capacities and the data they are given.

....

Pour les événements fortuits dont l'homme n'a pas déterminé les conditions, les causes qui donnent telles chances à tel événement, ou qui déterminent la loi de probabilité des diverse valeurs d'une grandeur variable, sont presque toujours inconnues dans leur nature et dans leur mode d'action, ou tellement compliquées, que nous ne pouvons en faire rigoureusement l'analyse, ni en soumettre les effets au calcul. ...

Il est donc bien nécessaire, pour les applications de la théorie des chances, que l'on puisse déterminer par l'experience, ou *a posteriori*, ces chances dont la mesure directe, d'après les données de la question, surpasse actuellement et vraisemblablement surpassera toujours les forces du calcul. Il ressort de ce que nous avons dit jusqu'ici, que le principe de Jacques Bernoulli conduit à cette détermination expérimental: car si, en désignant par x la chance inconnue de la production d'un événement, par n le nombre de fois que cet événement est arrivé en m épreuves, on peut toujours obtenir une probabilité P que l'écart fortuit $x - \frac{n}{m}$ tombe entre les limites $\pm l$ (le nombre l et la différence $1 - \mathrm{P}$ tombant au-dessous de toute grandeur assignable, pourvu que les nombres m, n soient suffisament grands), il est clair que, si rien ne limite le nombre des épreuves, la probabilité x peut être déterminé avec une précision indéfinie; qu'on peut arriver, par exemple, à être sûr qu'il n'y a pas, entre le rapport $\frac{n}{m}$ donné par l'expérience et le nombre inconnu x, une différence d'un cent-millième. Du moins l'existence d'une différence plus grande, quoique rigoureusement possible, serait un événement du genre de ceux que l'on répute avec raison physiquement impossibles; de sorte que nous sommes autorisés à les laisser à l'écart dans l'explication des phénomènes.

Nous pourrions, dès lors, en nous appuyant sur les théorèmes de Jacques Bernoulli, dont l'inventeur avait déjà parfaitemente saisi le sens et la portée, passer immédiatement aux applications que ces théorèmes reçoivent dans les sciences de faits et d'observations; mais une règle dont le premier énoncé appartient à l'Anglais Bayes, et sur laquelle Condorcet, Laplace et leurs successeurs ont voulu édifier la doctrine des probabilités *a posteriori*, est devenue la source de nombreuses équivoques qu'il faut d'abord éclaircir, d'erreurs graves qu'il faut rectifier, et qui se rectifient dès qu'on a présente à l'esprit la distinction fondamentale entre les probabilités qui ont une existence objective, qui donnent la mesure de la possibilité des choses, et les probabilités subjectives, relatives en partie à nos connaissances, en partie à notre ignorance, variables d'une intelligence à une autre, selon leurs capacités et les données qui leur sont fournies.

. . . .

After these general remarks, Cournot plunges into examples involving balls drawn from urns, showing that the results given by Bayes's rule are correct if we know the process by which an urn is chosen and a ball is drawn, not so meaningful if we do not know this process, and simply wrong if we are misinformed about it. He concludes, at the end of Section 89, with these words:

> So Bayes's rule, thus applied to determine subjective probabilities, has no utility aside from leading to the fixing of bets under a certain hypothesis about what the arbiter knows and does not know. It leads to a unfair fixing if the arbiter knows more than we suppose about the real conditions of the random trial.

. . . .

. . . La règle de Bayes, ainsi appliquée à la détermination de probabilités subjectives, n'a donc d'autre utilité que celle de conduire à une fixation de paris, dans une certaine hypothèse sur les choses que connaît et sur celles qu'ignore l'arbitre. Elle conduirait à une fixation inique si l'arbitre en savait plus qu'on ne le suppose, sur les conditions réelles de l'èpreuve aléatoire.

. . . .

3.6 Large-sample confidence limits

Exposition [5] 1843, Section 107:

> Usually the immediate purpose of statistical records and tables is either to find the chance of an event that may or may not happen as a result of random coincidences under given circumstances, or to determine the mean value of a quantity that can vary randomly between certain limits, or, finally, to give the probability law for the infinitely many values that a variable quantity can take under the influence of random causes. It is a natural to treat first the problem of determining from statistics the chance of an event, or to measure its possibility.
>
> We already saw in the preceding chapter that if the event A, which has the unknown probability that we will designate by p, happens n times in n observations or trials gathered statistically,

there is a probability P that the error committed in estimating p by $\frac{n}{m}$ will fall between the limits $\pm l$, the number l being tied to the auxiliary quantity t and therefore to the probability P[4] by the formula

$$t = lm\sqrt{\frac{m}{2n(m-n)}}.$$

As we have explained, the probability P has an objective value. It measures in effect the probability of error that we incur when we declare that the difference $\left|p - \frac{n}{m}\right|$ falls between the limits $\pm l$. Even if, for unknown reasons, certain values of p are able to appear more often than others in the ill-defined multitude of phenomena to which statistical observations can be applied, the number of true judgements that we will produce by declaring with probability P that the difference $\left|p - \frac{n}{m}\right|$ falls between the limits $\pm l$ will be to the number of mistaken judgements approximately in the ratio of P to $1 - \mathrm{P}$, provided that we make a large enough number of judgements that chance anomalies more or less cancel each other out.

. . . .

L'objet immédiat des relevés et des tableaux statistiques est ordinairement, soit de faire connaître la chance de l'arrivée d'un événement qui peut se produire ou ne pas se produire, dans des circonstances données, selon des combinaisons fortuites; soit de déterminer la valeur moyenne d'une quantité variable, susceptible d'osciller fortuitement entre certaines limites; soit enfin d'assigner la loi de probabilité des valeurs, en nombre infini, qu'une quantité variable est susceptible de prendre, sous l'influence de causes fortuites. Il est naturel de traiter d'abord du problème qui consiste à déterminer par la statistique la chance d'un événement, ou à donner la mesure de sa possibilité.

Déjà l'on a vu dans le chapitre précédent [96] que si l'événement A, dont nous désignerons par p la probabilité inconnue, s'est produit n fois dans un nombre m d'observations ou d'épreuves recueillies par la statistique, il y a une probabilité P que l'erreur que l'on commet en prenant pour p le rapport $\frac{n}{m}$, tombe entre les limites $\pm l$, le nombre l étant lié à l'auxiliaire t, et par suite à la probabilité P, au moyen de la formule

$$t = lm\sqrt{\frac{m}{2n(m-n)}}.$$

La probabilité P a, comme nous l'avons expliqué, une valueur objective; elle mesure effectivemnent la probabilité de l'erreur du jugement que nous portons, en prononçant que la différence $\left|p - \frac{n}{m}\right|$ tombe entre les limites $\pm l$. Lors même que, dans la multitude indéfinie de faits auxquels peuvent s'appliquer les observations statistiques, des raisons inconnues rendraient certains valeurs de p habiles à se produire plus fréquemment que d'autres, le nombre des jugements vrais que nous émettrions, en prononçant, d'après la probabilité P, que la différence $\left|p - \frac{n}{m}\right|$ tombe entre les limites $\pm l$, serait au nombre des jugements erronés sensiblement dan le rapport de P à $1 - \mathrm{P}$, si d'ailleurs on embrassait une série de jugements assez nombreux pour que les anomalies fortuites aient dû se compenser sensiblement.

. . . .

[4] Earlier, in Section 33, Cournot explained that

$$\mathrm{P} = \frac{2}{\sqrt{\pi}} \int_0^t e^{-t^2} dt.$$

3.7 Philosophical probability

Traité [7] 1861, Section 64:

> ... The probability that we have called *philosophical*, and which takes the names *analogy* and *induction* in certain particular applications, is related both to the notion of chance and to our sense of order and simplicity in the laws that express them. But on the other hand, philosophical probability differs in an essential way from mathematical probability, in that it cannot be reduced to numbers, not because of shortcomings in our present knowledge of the science of numbers, but in itself and by its own nature. There is no way to count the possible laws or to rank them like quantities with respect to the formal property that constitutes their degree of simplicity and gives unity, symmetry, elegance, and beauty to our theoretical conceptions. You might as well ask how many simple forms a body can take and whether the form of a cylinder is more or less simple than that of a cube or a sphere.

....

> ... Cette probabilité que nous qualifions de *philosophique*, et qui, dans certaines applications particulières, prend les noms d'*analogie* ete d'*induction*, tient à la fois à la notion du hasard et au sentiment de l'ordre et de la simplicité des lois qui l'expriment. Mais, d'autre part, la probabilité philosophique diffère essentiellment de la probabilité mathématique, en ce qu'elle n'est pas réductible en nombres, non point à cause de l'imperfection actuelle de nos connaissances dans la science des nombres, mais en soi et par sa nature propre. Il n'y a lieu, ni de nombrer les lois possibles, ni de les échelonner comme des grandeurs par rapport à cette propriété de forme qui constitue leur degré de simplicité, et qui donne, dans des degrés divers, à nos conceptions théoriques l'unité, la symétrie, l'élégance et la beauté. Autant vaudrait demander combien il y a des formes simples que les corps puissent revêtir, et si la forme d'un cylindre est plus simple ou moins simple que celle d'un cube ou d'une sphère.

....

3.8 Summarizing in 1843

The concluding section of Cournot's *Exposition de la théorie des chances et des probabilités* ([5] 1843, Section 240) summarizes the book as follows:

> Let us summarize in a few words the main points that we have undertaken to establish in this essay.
>
> 1. The idea of chance is the idea of the concurrence of independent causes to produce a given event. The combinations of different independent causes that all give rise to the same event is what should be meant by the chances of that event.
>
> 2. When only one out of an infinity of chances can produce the event, that event is called *physically impossible*. The notion of physical impossibility is neither a mental fiction nor an idea that has value only relative to the imperfect state of our knowledge. Elle

must figure as an essential element in the explanation of natural phenomena, whose laws do not depend on the knowledge that people might have about them.

3. When we consider a large number of trials of the same event, the ratio of the number of cases where the event happens to the total number trials becomes practically equal to the ratio of the number of chances favorable to the event to the total number of chances, or to what we call the *mathematical probability* of the event. If we could repeat the trial an infinite number of times, it would be physically impossible that the two ratios would differ by a finite amount. In this sense, the mathematical probability can be considered a measure of the *possibility* of the event, or of the facility with which it happens. By the same token, the mathematical probability expresses a ratio that stands outside the mind that conceives of it, a law to which phenomena are subject, whose existence does not depend on the expansion or narrowing of the our knowledge about their happening.

4. If, with our imperfect knowledge, we have no reason to suppose that one combination happens more often than another, even though in reality these combinations are events that can have unequal mathematical probabilities or possibilities, and if we understand the *probability* of an event to be the ratio of the number of combinations favorable to the event to the total number of combinations that we put in the same group, this probability can still serve, when there is nothing better, to fix the terms of a bet or any other risky exchange, but it will no longer express a real and objective relation between things. It will take on a purely subjective character and will be liable to vary from one individual to another depending on their knowledge. Nothing is more important than to carefully distinguish between these two meanings of the term *probability*, one an objective meaning, the other a subjective meaning, if we want to avoid confusion and error, whether in the exposition of the theory or in the applications we make of it.

5. In general, for natural events, whether physical or social, objective mathematical probability, conceived of as measuring the possibility of events arising from the concurrence of independent causes, can only be determined by experience. If the number of trials of the same chance increases to infinity, the probability will be determined exactly, with a certainty comparable to that for an event whose contrary is physically impossible. When the number of trials is merely very large, the probability is given only approximately, but we are still entitled to consider it very unlikely that the real value differs notably from the value derived from observations. In other words, we will very rarely err significantly in taking the observed value to be the real value.

6. When the number of trials is not very great, the usual formulas for evaluating probabilities *a posteriori* become illusory. They no

longer give us anything but subjective probabilities, appropriate for determining the terms of a bet but without use with respect to the determination of natural phenomena.

7. Nevertheless, we should not conclude from the preceding remark that the number of trials should always be very large in order to give the real values of the probability of an event with sufficient precision and sufficient confidence. We should conclude merely that the confidence will not be equivalent a probability in the objective sense. We cannot evaluate the chance we have of erring when we say that the real value falls between certain limits. In other words, we cannot determine the ratio of the number of mistaken judgements to the total number of judgements made in similar circumstances.

8. Independently of mathematical probability, in the two senses considered above, there are probabilities that are not reducible to the enumeration of chances but motivate a host of our judgements, and even the most important ones. These probabilities pertain mainly to our idea of the simplicity of nature's laws, of the order and rational sequence of phenomena, and for this reason we can call them *philosophical* probabilities. All reasonable people have a confused sense of these probabilities. When they are distinct or concern delicate subjects, they belong only to cultivated intelligences and or can even constitute a mark of genius. They form the basis of a system of critical philosophy, glimpsed in the most ancient schools, that represses or conciliates dogmatism, but which we must not, for fear of strange aberrations, bring into the domain of mathematical probability.

....

Résumons en quelques mots les principaux points de doctrine que nous avons pris à tâche d'établir dans cet essai.

1. L'idée de *hasard* est celle du concours de causes indépendantes, pour la production d'un événement déterminé. Les combinaisons de diverses causes indépendantes, qui donnent également lieu à la production d'un même événement, sont ce qu'on doit entendre par les chances de cet événement.

2. Quand, sur une infinité de chances, il n'y en a qu'une qui puisse amener l'événement, cet événement est dit *physiquement impossible.* La notion de l'impossibilité physique n'est point une fiction de l'esprit, ni une idée qui n'aurait de valeur que relativement à l'état d'imperfection de nos connaissances: elle doit figurer comme élément essentiel dans l'explication des phénomènes naturels, dont les lois ne dépendent pas de la connaissance que l'homme peut en avoir.

3. Lorsque l'on considère un grand nombre d'épreuves du même hasard, le rapport entre le nombre des cas où le même événement s'est produit, et le nombre total des épreuves, devient sensiblement égal au rapport entre le nombre des chances favorables à l'événement et le nombre total des chances, ou à ce qu'on nomme la *probababilité mathématique* de l'événement. Si l'on pouvait répéter l'épreuve une infinité de fois, il serait physiquement impossible que les deux rapports différassent d'une quantité finie. En ce sens, la probabilité mathématique peut être considérée comme mesurant la *possibilité* de l'événement, ou la facilité avec laquelle il se produit. En ce sens pareillement, la probabilité mathématique exprime un rapport subsistant hors de l'esprit qui le conçoit, une loi à laquelle les phénomènes sont assujettis, et dont l'existence ne dépend pas de l'extension

ou de la restriction de nos connaissances sur les circonstances de leur production.

4. Si, dans l'état d'imperfection de nos connaissances, nous n'avons aucune raison de supposer qu'une combinaison arrive plutôt qu'une autre, quoiqu'en réalité ces combinaisons soient autant d'événements qui peuveut avoir des probabilités mathématiques ou des possibilités inégales, et si nous entendons par *probabilité* d'un événement le rapport entre le nombre des combinaisons qui lui sont favorables, et le nombre total des combinaisons mises par nous sur la même ligne, cette probabilité pourra encore servir, faute de mieux, à fixer les conditions d'un pari, d'un marché aléatoire quelconque; mais elle cessera d'exprimer un rapport subsistant réellement et objectivement entre les choses; elle prendra un caractère purement subjectif, et sera susceptible de varier d'un individu à un autre, selon la mesure de ses connaissances. Rien n'est plus important que de distinguer soigneusement la double acception du terme de *probabilité*, pris tantôt dans un sens objectif, et tantôt dans un sens subjectif, si l'on veut éviter la confusion et l'erreur, aussi bien dans l'exposition de la théorie que dans les applications qu'on en fait.

5. La probabilité mathématique, prise objectivement, ou conçue comme mesurant la possibilité des événements amenés par le concours de causes indépendantes, ne peut en général, et lorsqu'il s'agit d'événements naturels, physiques ou moraux, être déterminée que par l'expérience. Si le nombre des épreuves d'un même hasard croissait à l'infini, elle serait, déterminée exactement, avec une certitude comparable à celle de l'événement dont le contraire est physiquement impossible. Quand le nombre des épreuves est seulement très grand, la probabilité n'est donnée qu'approximativement; mais on est encore autorisé à regarder comme extrêmement peu probable que la valeur réelle diffère notablement de la valeur conclue des observations. En d'autres termes, il arrivera très-rarement que l'on commette une erreur notable en prenant pour la valeur réelle la valeur observée.

6. Lorsque le nombre des épreuves est peu considérable, les formules données communément pour l'évaluation des probabilités *à posteriori* deviennent illusoires : elles n'indiquent plus que des probabilités subjectives, propres à régler les conditions d'un pari, mais sans application dans l'ordre de production des phénomènes naturels.

7. Il ne faut pourtant pas conclure de la remarque précédente, que le nombre des épreuves doive toujours être très-grand, pour donner avec une exactitude suffisante et avec un degré suffisant de vraisemblance, les valeurs réelles de la probabilité d'un événement; seulement cette vraisemblance n'équivaudra pas à une probabilité prise dans le sens objectif. On ne pourra pas assigner la chance que l'on a de se tromper, en prononçant que la valeur réelle tombe entre des limites déterminées: en d'autres termes, on ne pourra pas assigner le rapport du nombre des jugements erronés au nombre total des jugements portés dans des circonstances semblables.

8. Indépendamment de la probabilité mathématique, prise dans les deux sens admis plus haut, il y a des probabilités non réductibles à une énumération de chances, qui motivent pour nous une foule de jugements, et même les jugements les plus importants; qui tiennent principalement à l'idée que nous avons de la simplicité des lois de la nature, de l'ordre et de l'enchaînement rationnel des phénomènes, et qu'on pourrait à ce titre qualifier de probabilités *philosophiques*. Le sentiment confus de ces probabilités existe chez tous les hommes raisonnables; lorsqu'il devient distinct, ou qu'il s'applique à des sujets délicats, il n'appartient qu'aux intelligences cultivées, ou même il peut constituer un attribut du génie. Il fournit les bases d'un système de critique philosophique entrevu dans les plus anciennes écoles, qui réprime ou concilie le scepticisme et le dogmatisme, mais qu'il ne faut pas, sous peine d'aberrations étranges, faire rentrer dans le domaine des applications de la probabilité màthématique.

. . . .

3.9 Determinism

Cournot always distinguished between the notion that God had predetermined everything and the notion that a human-like superior intelligence could predict the future in the smallest detail. In his 1872 book, for example, we find these passages ([8], pages 2 and 3):

> . . . From the fact that Nature constantly and everywhere rolls the dice, and from the fact that chains of conditions and secondary causes, independent of each other, are constantly crossing, perpetually giving rise to what we call chances and random combinations, we cannot conclude that God does not hold them all in his hand, or that he could not have made them all happen by a single initial decree.
>
> . . . the distinction between the essential and the random does not necessarily depend on repetition. It applies just as much to a single trial as to a large number of trials of the same event, even though we no longer have the experimental criterion of statistics to sort the one from the other.

. . . .

> . . . De ce que la Nature agite sans cesse et partout le cornet du hasard, de ce que le croisement continuel des chaînes de conditions et de causes secondes, indépendantes les unes des autres, donne perpétuellement lieu à ce que nous nommons des chances ou des combinaisons fortuites, il ne s'ensuit pas que Dieu ne tienne point dans sa main les unes et les autres, et qu'il n'ait pu les faire sortir toutes d'un même décret initial.
>
> . . . la distinction de l'essential et de l'accidental ne tient pas foncièrement à la répétition des épreuves; elle subsiste aussi bien pour une épreuve unique que pour un grand nombre dépreuves du même hasard, quoique nous n'ayons plus le critère expérimental de la statistique pour faire la part de l'un et l'autre.

. . . .

In his fourth and final book on the philosophy of science, published in 1875 [9], Cournot tried to present his ideas in the simplest and most general way, while relating them to the science and philosophy of his time. In particular, he explained his understanding of determinism. [5] In his view, determinism should be understood in different ways in different sciences. In physics and chemistry, he accepted Laplace's picture, according to which a superior but human-like intelligence could use scientific laws to calculate and predict a system's future state from its present state. For a simple mechanical system, this intelligence could also calculate past states from the present state, but it would not be able to do so in more complex physical systems, such as the system of waves created by a pebble dropped in water or the diffusion of heat, because there the effect of initial conditions would eventually become infinitesimal (Section I.6).

[5] As he noted, the word *déterminisme* had recently been popularized in France by the biologist Claude Bernard (1813–1878). See Claire Salomon-Bayet's 1987 commentary in the 1987 edition of the book ([10], Volume V, page 235–236, 241) and Ian Hacking's 1983 article [14].

In the case of life and human history, however, he did not believe that knowledge of the present is sufficient to predict the future. He explained this as follows (Section III.7):

> So complete knowledge of the present state and the laws currently in force would not suffice, as they do in physical cosmology, to predict future states. One would have to add knowledge of the entire past and certainty that one is dealing with immanent laws, independent of time and circumstance, that do not vary in the course of the ages and according to the goal to be attained. In the end, this comes down to saying that only God has the secret of his creation.
>
> ....
>
> ... Ainsi la connaissancre complète de l'état actuel et des lois actuellement constatées ne suffirait pas, comme en cosmologie physique, pour la prévision des états futurs: il faudrait y joindre la connaissance de tout le passé, et la certitude qu'on a affaire à des lois immanentes, indépendantes du temps et des circonstances, qui ne varient pas dans le cours des âges et selon la nature du but à atteindre. Au fond cela revient à dire que Dieu seul a le secret de son œuvre.
>
> ....

It was also part of Cournot's philosophy that laws that permit prediction in human affairs change over time. We see this for example, in his attitude towards his own economic theory. This theory assumed a highly evolved economy, and even France was not there yet ([4], Section 21; see the discussion by Philippe Le Gall in [20]). Statisticians might never be able to find the law of demand empirically

> ... because of the difficulty of obtaining sufficiently numerous and exact observations, and also because of the successive variation that the law of demand must undergo in a country that has hardly yet settled into a stationary state ...
>
> ....
>
> ... à cause de la difficulté de se procurer des observations assez nombreuses et assez exactes, et aussi à cause des variations progressives que doit éprouver la loi de la demand, dans un pays qui n'est point encore arriveé à un état stationnaire ...
>
> ....

4 After Cournot

Cournot's book on probability was read by most Continental mathematicians who worked on probability in the second half of the 19th century, and his themes were echoed in various ways. Here I quote just a few of those echoes.

4.1 James Clerk Maxwell (1831–1879)

Maxwell, whose scientific work departed from the Newtonian picture of necessity, was also one of the first to question necessity on the basis of the instabilities that can arise even in mechanics. His expressed his thoughts about this in an essay dated 11th February 1873 and published posthumously in 1882. [24]. He concluded it with these words:

> If, therefore, those cultivators of physical science from whom the intelligent public deduce their conception of the physicist, and whose style is recognised as marking with a scientific stamp the doctrines they promulgate, are led in pursuit of the arcana of science to the study of the singularities and instabilities, rather than the continuities and stabilities of things, the promotion of natural knowledge may tend to remove that prejudice in favour of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of that of the past.

4.2 Charles Sanders Peirce (1839–1914)

Peirce rejected *necessitarianism*, as he called it, in "The Doctrine of Necessity Examined", published in *The Monist* in 1892 [26]:

> ...The proposition in question is that the state of things existing at any time, together with certain immutable laws, completely determine the state of things at every other time (for a limitation to *future* time is indefensible). Thus, given the state of the universe in the original nebula, and given the laws of mechanics, a sufficiently powerful mind could deduce from these data the precise form of every curlicue of every letter I am now writing.
>
> Very well, my obliging opponent, we have now reached an issue. You think all the arbitrary specifications of the universe were introduced in one dose, in the beginning, if there was a beginning, and that the variety and complication of nature has always been just as much as it is now. but I, for my part, think that the diversification, the specification, has been continually taking place.

We know, from Peirce's letter to Simon Newcomb dated December 17, 1871 ([12], page 414), that Peirce was familiar with Cournot's work on economics. The letter briefly explains the mathematics of supply and demand and ends with the postscript "This is all in Cournot". It seems reasonable to conjecture that he was also aware of Cournot's book on probability.

4.3 Andrei Kolmogorov (1903–1987)

Cournot's principle was emphasized by many of the Russian and French mathematicians from whom Kolmogorov learned about probability theory, including Chuprov, Slutsky, Borel, Lévy, and Fréchet [29]. These mathematicians, like Cournot, saw clearly that while the law of large numbers often authorizes us to identify a probability with a frequency, it can do so only when we agree that an event with very small probability will not occur. In the celebrated monograph, published in 1933 in German [17], in which he stated the axioms of mathematical probability, Kolmogorov mentions both frequency and Cournot's principle in his brief explanation of how the theory can be used:

> Under certain conditions, that we will not go into further here, we may assume that an event A that does or does not occur under conditions $\mathfrak{S}$ is assigned a real number $\mathsf{P}(A)$ with the following properties:
>
> A. One can be practically certain that if the system of conditions $\mathfrak{S}$ is repeated a large number of times, n, and the event A occurs m times, then the ratio m/n will differ only slightly from $\mathsf{P}(A)$.
>
> B. If $\mathsf{P}(A)$ is very small, then one can be practically certain that the event A will not occur on a single realization of the conditions $\mathfrak{S}$.

....

> Unter gewissen Bedingungen, auf die wir hier nicht näher eingehen wollen, kann man voraussetzen, daß einem Ereignis A, welches infolge der Bedingungen $\mathfrak{S}$ auftritt oder nicht, eine gewisse reelle Zahl $\mathsf{P}(A)$ zugeordnet ist, welche folgende Eigenschaften besitzt:
>
> A. Man kann praktisch sicher sein, daß, wenn man den Komplex der Bedingungen $\mathfrak{S}$ eine große Anzahl von n Malen wiederholt und dabei durch m die Anzahl der Fälle bezeichnet, bei denen das Ereignis A stattgefunden hat, das Verhältnis m/n sich von $\mathsf{P}(A)$ nur wenig unterscheidet.
>
> B. Ist $\mathsf{P}(A)$ sehr klein, so kann man praktisch sicher sein, daß bei einer einmaligen Realisation der Bedingungen $\mathfrak{S}$ das Ereignis A nicht stattfindet.

....

As Cournot emphasized, many events do not have objective mathematical probabilities; we can give them only philosophical probabilities. Perhaps not even a superior intelligence could give them objective mathematical probabilities. Today many people think differently; many think, or assume without thinking, that if an event is not determined, then it has an objective mathematical probability. So it is worth noting that Kolmogorov, like many mathematicians of his time who worked with the concept of objective probability, thought that only some events have objective probabilities. He put the matter this way in 1951 in in the *Great Soviet Encyclopedia*:

> Certainly not every event whose occurrence is not uniquely determined under given conditions has a definite probability under these conditions. The assumption that a definite probability (i.e. a completely defined fraction of the number of occurrences of an event if the conditions are repeated a large number of times) in fact exists for a given event under given conditions is a hypothesis which must be verified or justified in each individual case.

4.4 Abraham Wald (1902–1950)

Wald became a mathematician working with Karl Menger in Vienna and participating in his seminar. Both Menger and Wald fled to the United States as Hitler seized Austria. Menger became a professor at Notre Dame in Indiana; Wald became a professor at Columbia in New York. In February 1941, Wald gave a series of lectures at Notre Dame entitled, "On the principles of statistical inference". He began with this introduction ([34], pages 1–2, references omitted):

> The purpose of statistics, like that of geometry or physics, is to describe certain real phenomena. The objects of the real world can never be described in such a complete and exact way that they could form the basis of an exact theory. We have to replace them by some idealized objects, defined explicitly or implicitly by a system of axioms. For instance, in geometry we define the basic notions "point," "straight line," and "plane" implicitly by a system of axioms. They take the place of empirical points, straight lines, and planes which are not capable of definition. In order to apply the theory to real phenomena, we need some rules for establishing the correspondence between the idealized objects of the theory and those of the real world. These rules will always be somewhat vague and can never form part of the theory itself.
>
> The purpose of statistics is to describe certain aspects of mass phenomena and repetitive events. The fundamental notion used is that of "probability." In the theory it is defined either explicitly or implicitly by a system of axioms. For instance, Mises defines the probability of of an event as the limit of the relative frequency of this event in an infinite sequence of trials satisfying certain conditions. This is an explicit definition of probability. Kolmogoroff defines probability as a set function which satisfies a certain system of axioms. These idealized mathematical definitions are related to the applications of the theory by translating the statement "the event E has the probability p" into the statement "the relative frequency of the event E in a long sequence of trials is approximately equal to p." This translation of a theoretical statement into an empirical statement is necessarily somewhat vague, for we have said nothing about the meanings of the words "long" or "approximately." But such vagueness is always associated with the application of theory to real phenomena.
>
> It should be remarked that instead of the above translation of the word "probability" it is satisfactory to use the following somewhat simpler one: "The event E has a probability near to one" is translated into "it is practically certain that the event E will occur in a single trial." In fact, if an event E has the probability p then, according to a theorem of Bernoulli, the probability that the relative frequency of E in a sequence of trials will be in a small neighborhood

> of p is arbitrarily near to 1 for a sufficiently long sequence of trials. If we translate the expression "probability near 1" into "practical certainty," we obtain the statement "it is practically certain that the relative frequency of E in a long sequence of trials will be in a small neighborhood of p."

4.5 Trygve Haavelmo (1911–1999)

Haavelmo's article, "The probability approach to econometrics" [13], is often seen as the founding charter of modern econometrics [25]. The article's most fundamental point was Cournot's principle.

As Haavelmo explained, econometricians had been reluctant to adopt probability as a foundation for their work because they incorrectly assumed that probability is applicable only in situations like those studied by the British school of statistics, where a large sample is drawn from a stable population. He made the point as follows (pages 477–478):

> The reluctance among economists to accept probability models as a basis for economic research has, it seems, been founded upon a very narrow concept of probability and random variables. Probability schemes, it is held, apply only to such phenomena as lottery drawings, or, at best, to those series of observations where each observation may be considered as an independent drawing from one and the same 'population'. From this point of view it has been argued, e.g., that most economic time series do not conform well to any probability model, 'because the successive observations are not independent'. But it is *not* necessary that the observations should be independent and that they should all follow the same one-dimensional probability law. It is sufficient to assume that the *whole set* of, say n, observations may be considered as *one* observation of n variables (or a 'sample point') following an n-dimensional *joint* probability law, the 'existence' of which may be purely hypothetical. Then, one can test hypotheses regarding this joint probability law, and draw inferences as to its possible form, by means of *one* sample point (in n dimensions). Modern statistical theory has made progress in solving such problems of statistical inference.

Haavelmo went on to explain that a probability law can be tested based on one observation because it makes predictions with very high probability about that one observation, and such predictions are the only kind of prediction science can ever make:

> The class of scientific statements that can be expressed in probability terms is enormous. In fact, this class contains all the 'laws' that have, so far, been formulated. For such 'laws' say no more and no less than this: The probability is almost 1 that a certain event will occur.

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