Quanta, Physicists, and Probabilities ... ?

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Dedicated to Marie-Louise Nykamp

Abstract

There seems to be nothing short of a *double whammy* hitting the users of probability, and among them physicists, especially those involved in the foundations of quanta. First is the instant instinctual reaction that phenomena which interest one do sharply and clearly divide into the *dichotomy* of *two and only two* alternatives of being *either* "deterministic", *or* on the contrary, being "probabilistic". However, there is also a second, prior and yet deeper trouble, namely, the "probabilistic" case is strongly believed to be equally clear and well-founded as is the "deterministic" one. And the only difference seen between the two is that the latter can talk also about "individual" phenomena, while the former can only do so about large enough "ensembles" for which, however, it is believed to be equally clear, precise and rigorous with the "deterministic" approach. Or briefly, "probabilistic" is seen as nothing else but the "deterministic" on the level of "ensembles" ... The fact, however, is that there is a *deep gap* between the empirical world of "random" phenomena, and on the other hand, theories of "probability". Furthermore, any attempt to bridge that gap does in-

evitably involve *infinity*, thus aggravating the situation to the extent that even today, and even if not quite realized by many, theories of "probabilities" have a *shaky* foundation.
This paper tries to bring to the awareness of various users of "probabilities", and among them, to physicists involved in quanta, the fact that - seemingly unknown to them - they are self-inflicted victims of the mentioned double whammy.

> "Science is not done scientifically ..."
>
> "Physics is too important to be left to physicists ..."
>
> What is the ... probability ... that there is a valid concept such as "probability" ?

1. Introduction : the Gap between "Random" and "Probabilistic"

Formulated briefly, the main issue in this paper could be introduced as follows :

i) there is a *gap* between what go by the name of "random" empirical phenomena, and on the other hand, theories of "probabilities" which are supposed to be their mathematical models,

ii) that gap cannot be bridged without effective involvement of *infinity* at every "probabilistic" step,

iii) in view of i) and ii), none of the existing theories of "probabilities" is well-founded.

And now, some details.

Among physicists, as well as many others, there is a deeply entrenched instant reflex to classify manageable phenomena of interest according

to the sharp *dichotomy* of *two and only two* alternatives, namely, either "deterministic", or else, "probabilistic". Furthermore, and in fact, yet more questionably, this foundational error is seriously *aggravated* by considering the concept of "probabilistic" as being equally well founded with the "deterministic" one.
As for all other phenomena beyond the "deterministic" and "probabilistic" ones, they are seen to belong to that rather unfathomable category of "chaos", and thus preferably to be avoided as such, if possible ...

This paper starts by recalling main moments in what is seemingly hardly known still today to be in fact the yet *unsettled* issue of the foundation of "probabilities". And it does so beginning with the 1600s, when Blaise Pascal and Pierre de Fermat initiated in modern times the study of "probabilities". Then it mentions the pursuit of the subject in the 1700s, when the mathematician brothers Bernoulli started to deal with "probabilities" more deeply. Following that, the paper continues with important foundational moments nearer to our days.

Upon the hopefully resulting clarification of the fact of the long ongoing *lack* of foundation of the concept of "probability" several *negative* consequences in a better understanding of quanta are also presented.

However, it is important to note that the aim here is not the presentation of a comprehensive history of the foundations of "probabilities", or of the misunderstandings implied upon the understanding of quanta, tasks which quite likely would each have to run into a book of several volumes. Instead, the paper tries to focus only on sufficient instances of considerable foundational significance, sufficient - hopefully - to open up an awareness in those users of "probabilities" who happen to be more careful, than approaching "probabilities" in a mere routine manner, and doing so without any interest in a better understanding of what may in fact be involved.

And sorry to say, but with rare exceptions, physicists in foundations of quanta are among many others who make systematic use of "probabilities" regardless of the fact that their awareness of the still ongoing rather *shaky* foundations of "probabilities" is regrettably missing to a

large extent ...
Or even worse, they may be deeply convinced that, on the contrary, "probabilities" do by now have one or another as solid a foundation, as solid as "deterministic" theories may ever have.

This *double ignorance* does indeed act still today as a *double whammy*, and specifically as seen in the sequel, does so prominently regarding quanta ...

In this respect, and without intending any particular harm, one may point out as a typical such instance the recent 2003 book "Probability Theory, The Logic of Science", by the physicist E.T. Jaynes (1922-1998), a book which in many respects is indeed remarkable, and yet - as argued in the Appendix to this paper - presents but one of the various misguided and rather superficial claims to offer a foundation to "probabilities", even if only with the modest aim to be of use on empirical levels mainly, and mostly by physicists ...

All in all, since by far the most important and consequential present day users of "probabilities" seem to be theoretical physicists, especially those involved in quanta, this paper is dedicated to an attempt to try and save, so to say, their ... intellectual, and in particular, professional souls ...

Amusingly, a main culprit in perpetuating the mentioned *double* ignorance regarding the shaky foundations of "probabilities" has - ever since the mid 1920s - been the group of supporters of various versions of the "Copenhagen Interpretation" of quanta, where "probabilities" were for the first time ever in physics claimed to be *foundationally* important in what become the concept of the "instant collapse of the wave function", and the subsequent "measurement problem".
Indeed, it was precisely that "Copenhagen Interpretation", as fervently promoted by Niels Bohr, Werner Heisenberg, Max Born, and others, which with an obvious pride of setting up an unprecedented first in the whole history of science insistently kept claiming that the quanta were *ontologically* probabilistic, and not merely *epistemically*, like for instance, was the case with the role of "probabilities" in Statistical Mechanics ...

And such an ontological claim kept being made in spite of the ongoing objection of Albert Einstein, Erwin Schroedinger, and later David Bohm, among others ...

And so it comes to pass that noticed by preciously few in quanta, a still *deeper* and more *crucial* problem to be addressed even *before* that of "collapse" and "measurement", has been - and keeps being even today - the still ongoing *shaky* foundations of the very concept of "probabilities", a concept which gets essentially - and all too obviously - involved in both the mentioned so called "collapse" and "measurement".

Looking forward, therefore, to the better future times when the very *first* foundational issue in quanta may at long last become the issue of the foundations of "probabilities" themselves ...

Let us now try to get into some details ...

Ludwig Boltzmann (1844-1906) introduced Statistical Mechanics in the late 1800s and developed it to a considerable extent, including important applications in physics. Then, starting in the mid 1920s and for several decades after, there was the exclusive domination of the "Copenhagen Interpretation" in quanta. An effect of such a state of affairs seems to have been the instant and unquestioned reflex in the thinking not only of physicists, according to which the various phenomena of their respective interest do clearly and sharply divide into the *dichotomy* of *two and only two* classes, namely, "deterministic", or on the other hand, "probabilistic".
Regarding the "deterministic" phenomena, the foundational situation has always seemed to be quite clear and rigorous, except perhaps for what is called since the 1960s, and especially the late 1970s, by the amusing name of "deterministic chaos".
After all, the foundational clarity and precision involved are supposed to be some of the essential features of "deterministic" phenomena, and obviously, they are a sine-qua-non in "causality" which is seen to be but a paradigmatic feature of "determinism" and which, among others, means that one can obtain a precise mathematical modelling even of all individual entities involved, and not only of large enough

ensembles of such individual entities.

On the other hand, due to the long ongoing lack of familiarity with - let alone understanding of - the reality of the essentially *messy* foundational aspects of the concept of "probability", the rather unshakable conviction rules still today, and not only among physicists, that the mentioned dichotomy "deterministic versus probabilistic" has by now both sides of it equally clearly defined and well founded.

The aim of this paper is to point to some of the most fundamental moments when, ever since the early 1700s and the contributions of the Bernoulli brothers in mathematics, the highly questionable foundations of "probabilities" have in fact been highlighted and discussed, even if hardly with any effect upon most of the users of "probabilities", among them as rather significant ones nowadays, the physicists, and especially those involved in quanta.

As a *caveat* before getting into the main subject, let us mention no less than *two* divergent trends :

Since the late 1920s, Bruno de Finetti (1906-1985), being aware of the lack of a proper foundation of the concept of "probability", did develop an alternative approach which got named "subjective probability", [3], and which later became popular in certain circles which promoted a Bayesian a view of "probability". In fact, "subjective probability" goes so far as simply to *deny* the very existence of "probability", as mentioned in the sequel.

Then, since the 1960s, there is also an awareness in certain circles about the *third* alternative class of phenomena of interest - beyond the "deterministic" and "probabilistic" ones - namely, the so called "fuzzy" phenomena which were introduced by the Azerbaijan-American electrical engineer Lofti A. Zadeh (1921-).
Furthermore, as mentioned, since the 1960s and especially late 1970s, there is also an awareness of the so called "deterministic chaos".

Seemingly - and quite hard to say whether fortunately or not - the above two additional classes of phenomena, namely, "fuzzy" and "deterministic chaos", have not so far made it into the foundations of quanta ...

Last, and by no means least, there has also been the class of "plausible" events as promoted by the well known Hungarian-American mathematician G. Polya (1887-1985). In some useful and important sense, the concept of "plausibility", as meant by Polya, is supposed to be weaker, and thus more general, than that of "probability". A good fist description of that relationship can be found in the mentioned book of E.T. Jaynes, for instance. Further related comments will be presented in the sequel.

2. Why at all "Probabilities" ?

Pascal seems to be the first in modern times to have had an interest in trying to *bridge* the *gap* between a certain class of empirical phenomena, called for convenience "random", and on the other hand, some hoped for convenient rational, and in particular mathematical modelling of them by what came to be called "probabilities". His interest arose related to usual card games, or games in tossing a coin or a dice. Such were the class of "random" empirical phenomena considered. And it was precisely their a priori clearly stated rules which - at first sight - made them seem amenable to a rational, and even numerical computational approach.

On the other hand, to set up any clearly stated and well founded mathematical model, one had first to give a precise and usable *definition* of "probability", which Pascal did as follows :

In the case of tossing a *fair coin*, for instance, the "probability" of getting a "head", or alternatively, getting a "tail", was by definition equal to 1/2. And in more general situations with a *finite* number of "random" outcomes, Pascal's definition was similarly simple and practical as well. Namely, the number $0 \leq p \leq 1$, given by

(2.1) $\quad p = \frac{m}{n}$

is by definition the "probability", where $n \geq 1$ is the *finite* number of all possible "random" empirical events under consideration, while $0 \leq m \leq n$ is the implicitly *finite* number of all favourable such "random" empirical events.

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Let us, therefore, for convenience - and hopefully, also clarity - denote by $\mathcal{R}an$ the realm of empirical phenomena which are to be considered "random", and on the other hand, let us denote by $\mathcal{P}rob$ the realm of mathematical models described by theories of "probabilities" which are supposed to model the so called "random" empirical phenomena, that is, the realm $\mathcal{R}an$.

Consequently, and for instance, the successive tossing of a given *fair coin* in which each such toss is assumed to be independent of all the other ones, belongs to $\mathcal{R}an$, while the mathematical formula (2.1) does belong to $\mathcal{P}rob$.

As for the mentioned *gap* between $\mathcal{R}an$ and $\mathcal{P}rob$, it is of no less than of *foundational* importance to note - and then keep in mind in the sequel - that the above concept $\mathcal{P}rob$ is up to us humans to define. Indeed, any given mathematical theory, be it even merely in a stage of development, can - rightly or wrongly - be considered by us to belong, or for that matter not to belong, to $\mathcal{P}rob$.
On the other hand, regarding the realm of $\mathcal{R}an$, it is obvious that we simply *cannot* give a definition even to the concept of *fair coin*, unless in some way it is circular, that is, self-referential, and/or unnoticed by us, it makes some appeal to concepts in $\mathcal{P}rob$ ...
And on top of it, such a possible definition may - in a way or another - have serious problems in trying to avoid the concept of *infinity* ...
As for giving a proper and useful definition for the concept of "random", the situation is by no means less easy ...

Thus the above essential *difference* between $\mathcal{R}an$ and $\mathcal{P}rob$ is precisely the mentioned *gap* which still today causes the foundational *shakiness* in theories of "probability" ...
Regarding the mentioned essential difficulties in defining the concept

of "random", we cannot so easily avoid an implied *lack* of clarity at the very foundational levels of the whole theory of "probabilities" ...

Furthermore, the inevitable involvement of *infinity* in dealing with the *gap* between $\mathcal{R}an$ and $\mathcal{P}rob$, that already happens even in that most simple case of tossing a fair coin, as seen in (2.4), (2.5) in the sequel.

And precisely here - with the involvement of *infinity* - there is a *main* and yet *unsolved* issue regarding "probabilities" ...
And it is not only unsolved, but very few of those using "probabilities" are aware of it to any relevant extent, let alone are aware of its all important and unavoidable *foundational* role ...

To give here a rather blatant example in this regard, anticipating briefly related arguments in the sequel, let us note the following.

The "Copenhagen Interpetation" of quanta :

> a) claimed to abolish "determinism" in the realms of quanta,
>
> b) claimed to replace "determinism" with "probabilities",
>
> c) imposed the transition from a) to b), with the claim to be but a new universal paradigm not only related to quanta, but in the whole of "modern" - as opposed to "classical" - physics, thus expelling the relevance of any and all ontological concerns from "modern" physics, and allowing instead only and only epistemological concerns,
>
> d) utterly failed to realize that the transition from a) to b) did in *no way* eliminate "determinism" from physics, let alone, from its foundations, but instead, it merely *shifted* "determinism" from the level of individual empirical phenomena, to that of certain *alleged* to exist ensembles of "random" empirical phenomena,
>
> e) the transition from a) to b) was made at the cost of

involving *infinity* at each and every even minutest "probabilistic" step, as noted already in that most simple example of (2.1) above.

Indeed, the "Copenhagen Interpretation" of quanta, as well as the interpretations associated with it in various ways, did - and still do today - all the above a) - e), and seemingly keep doing so

f) in a proverbial ... blissful ignorance ... of the utter lack of any satisfactory foundations of "probabilities",

g) yet are clinging as much as possible to "determinism", albeit this time simply shifted away by *one single* level, as mentioned at d) ,

h) and then on top of all that, bring *infinity* in, at each and every even minutest "probabilistic" step.

Let us try to recapitulate :

Obviously, the motivating idea of the *definition* of "probability" chosen in (2.1) was simple, and at least at first sight, it was also intuitively to the point, namely :

(2.2) The empirical phenomenon of tossing a given number $n \geq 2$ of times a fair coin, with each such toss being independent of all the other ones, was considered to be "random", where the meaning of the concept of "random", and in particular, of *fair coin*, was left in fact *undefined* in any proper manner.

(2.3) Furthermore, it was supposed that an *infinite* succession of such so called, yet not defined precisely "random" tossing, that is, with n going through all the values $2, 3, 4, \ldots$, had the miraculous property that a *unique* number $0 \leq \rho \leq 1$ was - irrespective of all other considerations - associated with it once and for evermore. That is, no matter when and where, if an alleged fair coin was to be subjected to the above procedure, then ρ would for sure be there, and would by necessity have ever the very same value.

(2.4) Counting for each $n \geq 2$ the value m of "heads" in the respective n tosses of the alleged fair coin, one would *always* obtain

(2.5) $$\lim_{n\to\infty} \frac{m}{n} = \lim_{n\to\infty} \frac{n-m}{n} = \frac{1}{2}$$

and thus a really nice connection would result between a presupposed yet undefined "randomness" at the *empirical* end which is given by that mysterious number ρ, and on the other hand, at the *theoretical* end, where the number p is defined in (2.1). Indeed, in (2.5), either we like it or not, the alleged and yet undefined "randomness" appears due to the infinite sequence of numbers m which correspond to each $n = 2, 3, 4, \ldots$, and which numbers m can be obtained properly - that is, *effectively* - only and only by being noted as the *result* of n successive tossing under the conditions mentioned above, that is, for *all* values $n = 2, 3, 4, \ldots$, which of course is but *infinitely* many ...

(2.6) Therefore, the miraculous connection in (2.5) between the empirical ρ and the theoretical p, that is, between the realms of $\mathcal{R}an$ and $\mathcal{P}rob$, is only obtained at *infinity* ...
And so terribly regrettably, *nobody* would ever be able effectively to perform the *infinite* operations in (2.5), so as to verify whether indeed the claimed result in (2.5) may hold, or on the contrary ...
And as seen later, this is indeed a major *spoiler* of the whole idyllically assumed achievement in (2.5) ...

(2.7) Also, before one may accept (2.5) regarding "probabilities", let us recall the following. The seemingly neat and obvious nature of the alleged link in (2.5) between the realms of $\mathcal{R}an$ and $\mathcal{P}rob$ can quite easily be challenged upon certain second thoughts that may arise from what is called the Law of Truly Large Numbers, of briefly (LTLN), mentioned in Example 4.1 in the sequel.

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Now of course, there are any number of far more complicated so called "random" empirical phenomena of interest than those in (2.5), and which are waiting to benefit from a mathematical modelling ...
To mention but one of them which popped up early in the study of

"probabilities", we can recall the Law of Large Numbers, or briefly (LLN).

Indeed, one of the first major *positive* hints regarding the existence of some sort of theory of "probabilities" was considered to be given by the mathematical theorem of the (LLN), which was seemingly first stated, but without a proof, by Gerolamo Cardano (1501-1576).
As it happened, Jacob Bernoulli managed to come up with a rigorous enough proof on (LLN), at least according to the standards of the time, and published it in the early 1700s. And in our times, it is known as the Weak Law of Large Numbers, that is in fact, as (WLLN). Hence (WLLN) = (LLN).

Before we would, however, get carried away too much with the enthusiasm produced by the (LLN) regarding any possible theory of "probabilities", let us mention here briefly the other side of the proverbial coin, namely :

> The result in the (LLN) seen below in (2.12), is that
>
> $$\lim_{n \to \infty} P(|Y_n - \mu| \geq \epsilon) = 0$$
>
> which means that, as $n \to \infty$, the "probability" of no matter how small a deviation of the *sample mean* Y_n from the *mean* μ of the *samples* X_n, a deviation above any given a priori fixed $\epsilon > 0$, must tend to zero.

As mentioned, this result has been seen as being of a *major* positive nature about the chances and meaning of any suitable theory of "probabilities" ...

However, what passes hardly at all noticed is the following :

> By the very same kind of argument which leads to the above (LLN), we have also to accept that *together* with such a positive result we may - with equally significant "probabilities" - have to encounter, along $n \to \infty$, no matter how *long* sequences of completely aberrant values of the

samples $X_1, X_2, X_3, ..., \ldots, X_n$, as argued in the sequel in Example 4.1.

In other words, we have for too long by now been focusing *exclusively* on the "good news" of the (LLN), and totally *disregarded* the *equally* valid "bad news" of the Law of Truly Large Numbers, or briefly (LTLN) ...

Let us then first have a closer look at what (LLN) did actually accomplish. In a brief presentation, the situation is as follows.

Example 2.1

Let $X_1, X_2, X_3, \ldots$ be an infinite sequence of independent and identically distributed "random" variables, each having the same mean $< X_n > = \mu$, and the same standard deviation $var(X_n) = \sigma$. Define then an infinite sequence of associated "random" variables, which constitute what is called the *sample means* of the "random" variables $X_1, X_2, X_3, \ldots$, namely

$$(2.8) \qquad Y_n = \frac{X_1 + ... + X_n}{n}, \quad n \geq 1$$

Then we have

$$(2.9) \qquad < Y_n > = \frac{< X_1 + ... + X_n >}{n} = \frac{< X_1 > + ... + < X_n >}{n} = \mu, \quad n \geq 1$$

Further we obtain

$$(2.10) \qquad \begin{aligned} var(Y_n) &= var(\tfrac{X_1 + ... + X_n}{n}) = var(\tfrac{X_1}{n}) + ... + var(\tfrac{X_n}{n}) = \\ &= \tfrac{\sigma^2}{n^2} + ... + \tfrac{\sigma^2}{n^2} = \tfrac{\sigma^2}{n}, \quad n \geq 1 \end{aligned}$$

Therefore, by the Chebyshev inequality, for all $\epsilon > 0$, one has

$$(2.11) \qquad P(\,|Y_n - \mu| \geq \epsilon\,) \leq \frac{var(Y_n)}{\epsilon^2} = \frac{\sigma^2}{n \times \epsilon^2}, \quad n \geq 1$$

It then follows that, for all $\epsilon > 0$, we have

(2.12) $\qquad \lim_{n \to \infty} P(|Y_n - \mu| \geq \epsilon) = 0$

that being the expression of the (LLN).

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What one can easily note in the above relations (2.8) - (2.12) is the following.

The entities $X_1, X_2, X_3, \ldots$ are merely the mathematical *models* - belonging to $\mathcal{P}rob$ - of entities in the realm of $\mathcal{R}an$. And of course, so are the derived entities $Y_1, Y_2, Y_3, \ldots$.
As for the entities $< X_n > = \mu$ and $var(X_n) = \sigma$, their left hand terms are in general mathematical integrals, which in particular cases may be countable or finite sums of numbers, thus they as well belong to the realm of $\mathcal{P}rob$.
In rest, (2.9) - (2.12) are purely mathematical relations, thus again, they belong to the realm of $\mathcal{P}rob$.

And then, arises the following QUESTION :

> How does - if at all - the (LLN) help bridge the gap between $\mathcal{R}an$ and $\mathcal{P}rob$?

Well, one thing is clear : all the entities $X_1, X_2, X_3, \ldots$, $Y_1, Y_2, Y_3, \ldots$, $< X_n > = \mu$ and $var(X_n) = \sigma$, as well as the relations (2.8) - (2.12) belong to $\mathcal{P}rob$.
So that, the *only* possible connection to $\mathcal{R}an$ of the above formulation of the (LLN), as well as of its proof, may possibly come from some *abuse* by which we may for the moment assume that the entities $X_1, X_2, X_3, \ldots$, $Y_1, Y_2, Y_3, \ldots$ do actually belong to $\mathcal{R}an$ as well ...

However, even in the case of such an abuse, the limit in (2.12) is essentially *different* from the limits in (2.5). Indeed, in the latter, the values of m are supposed to be extracted from the effective observation of a specific "random" empirical process, thus clearly, all those values m do belong rather to $\mathcal{R}an$, than to $\mathcal{P}rob$.
On the other hand, *none* of the values in (2.12) belongs to $\mathcal{R}an$!

Consequently, the (LLN) has a highly *questionable* relation to the realm of $\mathcal{R}an$, being instead rather a *purely mathematical* theorem within the realm of $\mathcal{P}rob$ ...

By the way, the so called Strong Law of Large Numbers, or (SLLN), does not change at all the negative facts mentioned above. In particular, it does not give any positive answer to the corresponding QUESTION :

> How does - if at all - the (SLLN) help bridge the gap between $\mathcal{R}an$ and $\mathcal{P}rob$?

Indeed, the (SLLN) states that

(2.13) $\quad P(\lim_{n \to \infty} Y_n = \mu) = 1$

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As it happened however - and quite unfortunately - the highly *questionable* interpretation which the (LLN) got from the very beginning was that of representing a most relevant and promising *inroad* into the realm of "random" empirical processes, of representing a most relevant and promising *link* between $\mathcal{R}an$ and $\mathcal{P}rob$ ...
And either we like it or not, that early interpretation has still remained quite the same today ...
Except that there is *no longer* hardly any awareness of the deeply problematic nature of any such possible claim, as seen above in some of the more blatant details, details known in fact by now for about three centuries ...

Instead of such an awareness, and also in the case of no matter how general and involved "random" empirical processes, we witness a full and never questioned confidence in no less than *two* assumptions, namely :

(2.14) There exists a well defined numerical value ρ associated with each and every of the specific "random" empirical processes modelled.

(2.15) The larger the numbers of empirical "random" instances involved, the more the mathematically computed "probability" p is supposed to approach the corresponding and assumed to exist unique numerical value ρ.

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In other words, the justification of the definition of "probability" in (2.1) rests upon no less than *two* limit-like assumptions :

(2.16) Each and every "random" empirical phenomenon has from the start, and independent of all else, associated with it for evermore a unique numerical value $0 \leq \rho \leq 1$.

(2.17) For each and every "random" empirical phenomenon, the mathematical number p defined according to (2.1) does *approach* arbitrarily close the value ρ when, for instance, the limit procedure in the (LLN), or other appropriate and far more general limit procedures are applied.

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However, ever since the emergence of such results as the (LLN), those involved in "probabilities" did - in their hope-charged enthusiasm - not much care about highly questionable and/or conflicting facts which, as mentioned, started to accumulate right at the beginning.

First of all in this regard, is the massive *asymmetry* between the existential status of ρ in (2.14), and on the other hand, of p in (2.15).
Indeed, the existence of every such ρ is merely an *assumption* which hardly ever - if at all - has any kind of more serious empirical and/or theoretical support.
On the other hand, the existence of p is as simple an issue as its direct and effective computation in the fraction in (2.1), in which the only difficulty may arise with the combinatoric complexities possibly involved in the computation of m as a function of n, with n finite and arbitrary.

This most relevant asymmetry, however, has up until today all too frequently been completely missed from consideration ...

Another truly dramatic trouble was produced by the Bernoulli brothers themselves as early as 1713, and it became known under the name of the "St. Petersburg Paradox".

Amusingly, this paradox, mentioned briefly below as cited from Wikipedia, shows the *untenable* nature of having to introduce *infinity*, in order to hope to bridge the gap between (2.14) and (2.15), or put more simply, between the number p which mathematics is supposed to be able to deliver, and on the other hand, the number ρ whose very existence, let alone uniqueness, is - and can only be - a sort of not easy to accept ontological assumption.

And as a side remark here, it is to be noted that E. T. Jaynes himself in his mentioned book [4] - as seen in the Appendix to this paper, as well as related to his interpretation of the Bertrand Paradoxes in Probability, [4, pp. 386] - says rather explicitly that ρ is supposed to *depend* on one or another of the *finite* processes involved which give the terms of the limit-like processes that are assumed to define the "probabilities" of interest. Thus it is hard to see how such numbers ρ may in general end up being unique, depending, as they are supposed to do, on one or another of the mentioned finite processes ...

Later we shall see that *infinity* does massively trouble the whole foundations of "probability". And it does so no less than in *two* ways, namely, not only by its sine-qua-non direct effect by the arbitrarily *large* positive numbers needed to be able to apply (LLN) meaningfully, but also by its effect through arbitrarily *small* strictly positive numbers as well, as seen in the so far never properly dealt with issue of the "Cournot Principle" which was formulated back in the early 1800s.

Briefly now, to the St. Petersburg Paradox, see Wikipedia.

A casino offers a game of chance for a single player in which a fair coin is tossed at each stage. The initial stake starts at 2 dollars and is doubled every time "head" appears. The first time "tail" appears, the game ends and the player wins whatever is in the pot. Thus the player wins 2 dollars if "tail" appears on the first toss, 4 dollars if "head" appears on the first toss and "tail" on the second, 8 dollars if "head" appears on the first two tosses and "tail" on the third, and so on. Mathematically, the player wins 2^k dollars, where "k" equals number of tosses.

QUESTION : What would be a fair price to pay the casino for entering the game?

To answer this, one needs to consider what would be the average payout ?
The answer is obviously as follows : with "probability" $\frac{1}{2}$, the player wins 2 dollars; with "probability" $\frac{1}{4}$, the player wins 4 dollars; with "probability" $\frac{1}{8}$, the player wins 8 dollars, and so on. The expected value is thus

$$E = (\tfrac{1}{2} \times 2) + (\tfrac{1}{4} \times 4) + (\tfrac{1}{8} \times 8) + \ldots = 1 + 1 + 1 + \ldots = \infty$$

Assuming the game can continue as long as the coin toss results in "head", and in particular that the casino has unlimited resources, this sum grows without bound and so the expected win for repeated play is an *infinite* amount of money.
Considering nothing but the expected value of the net change in one's monetary wealth, one should therefore play the game at any price if offered the opportunity.

And yet, NO ONE is willing to pay any larger amount of money in order to play that game, in spite of the fact that on average, and according to the definition in (2.1), the expectation of the game is an *infinite* amount of money ...

This is, therefore, the paradox ...

Of course, there have been any number of comments, interpretations, alleged solutions, and so on ...
And yet, none of them found anything wrong with the definition (2.1), and even less suggested a replacement for it which would eliminate that paradox ...

And needless to say, a redefinition of p in (2.1) and/or a clarification of the ontological status of ρ in (2.14) would be the natural direct way to deal with the "St. Petersburg Paradox" ...
But then, who knows ?
Far more radical approaches to "probability" may as well be needed ...

And in order to indicate possible hints, let us recall in passing two views which have more than on occasion fascinated larger numbers of people interested in "probabilities".

Richard von Mises (1883-1953) - not to be confused with Ludwig von Mises (1881-1973), his yet more famous brother - was a mathematician and engineer interested among others in "probabilities". Being an adept of Positivism in philosophy, he strongly advocated the view that "probability" should be defined - in the above terms - rather by "ρ", than by "p". That is, he tried hard to base the whole issue of "probability" upon the realm of $\mathcal{R}an$, rather than $\mathcal{P}rob$. In his respective venture, he inevitably conflicted sharply with Kolmogorov's view of "probabilities", [5], and in the longer run, rightly or wrongly, he lost that competition ...
As understood by many at the time, von Mises was stating the so called "frequentist" view of "probabilities", which formulated roughly would mean that - in terms of (2.1), (2.5) - he claimed as *definition* of the concept of "probability" the relation

$$p = \lim_{n \to \infty} \frac{m}{n}$$

which, of course, cannot be effectively implemented in the realm of $\mathcal{R}an$, as it would involve no less than *infinitely* many operations.

A second, far less ... unrealistic ... definition of "probability" was that one given by Bruno de Finetti (1906-1985), and according to which, [3] :

> "Probability does NOT exist."

or more precisely, "probabilities" do not and cannot have any objective existence or reality, being instead mere expressions of uncertainties of individuals ...

Therefore the name of "subjective probability" for the theory developed by de Finetti. And to add to the drama, let us mention related to the above sentence of de Finetti, the following citation from the very beginning of the paper [7], namely :

> "It is strange that the summary of a lifetime of work on the theory of "X'' should begin by declaring that "X'' does not exist, but so begins de Finetti's Theory of Probability (1970/1974) :
>
> > 'My thesis, paradoxically, and a little provocatively, but nonetheless genuinely, is simply this :
> >
> > PROBABILITY DOES NOT EXIST
> >
> > The abandonment of superstitious beliefs about the existence of the Phlogiston, the Cosmic Ether, Absolute Space and Time, ... or Fairies and Witches was an essential step along the road to scientific thinking. Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading misconception, an illusory attempt to exteriorize or materialize our true probabilistic beliefs, [3, p. x].'
>
> Of course, what de Finetti meant by this was that probability does not exist objectively, independently of the human mind. Rather :
>
> > '[I]n the conception we follow and sustain here, only subjective probabilities exist i.e., the degree of belief in the occurrence of an event attributed by a given person at a given instant and with a given set of information, [3, pp. 34].'

And yet, amusingly, there has recently been a certain interest in using "subjective probabilities" related to quanta, as for instance in [2] ...

So much for having any ... clarity ... on the level of foundations of "probabilities" ...

3. The Cournot Principle

A. A. Cournot (1801-1877) was a French philosopher and mathematician involved among others in theoretical economics. He formulated the following principle which was supposed to govern the *practical* applications of "probabilities", see [11-14] :

> A physically impossible event is one whose probability is infinitely small. This remark alone gives substance - an objective and phenomenological value - to the mathematical theory of probability.

Maurice Frèchet (1878-1973), one of the great French mathematicians who started the two modern branches of mathematics called Topology, respectively, Functional Analysis, did propose the name "Cournot's Principle" for the above statement.

The essence - so easily missed by so many even today - of that principle is that :

(3.1) It asks the practically all important question whether there is a *small* enough, yet nonzero probability $p > 0$, such that all events with that, or with a still smaller probability in some given class of "random" events, can be considered as being impossible from practical point of view, thus could simply be disregarded as events ?

(3.2) It answers that question with a firm and clear "YES".

(3.3) It is, however, rather ambiguous regarding the respective values of $p > 0$. Indeed, it merely qualifies such small values of $p > 0$ as being "infinitesimal", which is of course rather vague, especially when considered within usual mathematics, that is, outside of Nonstandard Analysis ...

***

Indeed, at the time when Cournot formulated that principle, there was not yet any rigorous mathematical theory of "infinitesimals", except for the rather informal ideas of G. W. Leibniz (1646-1716). In this regard, the first rigorous such mathematical theory, Nonstandard Analysis, came later, in the 1960s, as developed by Abraham Robinson.
And as follows from the context of his writings, Cournot did most likely not refer to the "infinitesimals" of Leibniz, when stating his mentioned principle.

Amusingly, when in the 1930s, A. N. Kolmogorov (1903-1987) introduced his presently used theory of probability, [5], he took in fact a decision - not quite mentioned explicitly, and even less argued for - to *reject* (3.2) above in the Cournot Principle. Indeed, according to Kolmogorov, every probability P is a mathematical entity given by a non-negative *sigma-additive* measure, thus it is characterized by the relation

$$(3.4) \qquad P(\bigcup_{1 \leq n < \infty} A_n) = \sum_{1 \leq n < \infty} P(A_n)$$

whenever the "random" events $A_1, A_2, A_3, \ldots \subseteq E$ are pair-wise disjoint, plus of course the relation $P(E) = 1$, where E is the total space of "random" events.

In this regard we can note that Kolmogorov makes from the start a fundamental difference between what he calls "finite fields of probabilities", and on the other hand, "infinite fields of probabilities", [5, p. 15]. The former have to satisfy his Axioms I - V, while the latter must also satisfy Axiom VI, [5, p. 14, (1) - (3)]. In modern terms, this Axiom VI goes as follows :

Given an infinite sequence of probabilistic events $A_1 \supseteq A_2 \supseteq A_3 \supseteq \ldots$, such that $\bigcap_{1 \leq n < \infty} A_n = \phi$, then

$$(3.4^*) \qquad \lim_{n \to \infty} P(A_n) = 0$$

which, as is known, is equivalent with (3.4).

Thus it is a kind of *continuity* axiom regarding the behaviour of smaller and smaller probabilistic events $A_1 \supseteq A_2 \supseteq A_3 \supseteq \ldots$, more precisely, regarding the behaviour of their probabilities $P(A_1), P(A_2), P(A_3), \ldots$. Which means that Axiom VI may be seen as belonging to some extent to the *same* kind of concerns with the Cournot Principle, although quite clearly, it only and only refers to the realm of $\mathcal{P}rob$, and does *not* refer directly in any way to any empirical realm whatsoever.

Implicitly however, (3.4) obviously implies that no matter how *small* the nonzero probability $P(A_n) > 0$ of any given event A_n may be, that event simply *cannot* ever be disregarded in the right hand term, since its disregard would invalidate the equality in (3.4), thus would make P no longer be sigma-additive.
And yet, the series in the right hand term of (3.4) does *converge* to a number which is between 0 and 1, since the left hand term of the equality in (3.4) is a "probability", thus it is a finite number, more precisely, at most 1. Therefore we must always have

(3.5) $\qquad \lim_{n \to \infty} P(A_n) = 0$

Amusingly, the comment Kolmogorov makes related to his above Axiom VI, that is, (3.4*), and thus (3.4), is rather cursory and minimal, and it goes as follows, [5, p. 15] :

> "... Since the new axiom (that is, Axiom VI) is essential for infinite fields of probability only, it is almost impossible to elucidate its empirical meaning, as has been done, for example, in the case of Axioms I - V in §2 of the first chapter. For, in describing any observable random process we can obtain only finite fields of probability. Infinite fields of probability occur only as idealized models of real random processes. We limit ourselves, arbitrarily, to only those models which satisfy Axiom VI. This limitation has been found expedient in researches of the most diverse sort ...

In other words, Kolmogorov chooses to limit himself to the vague justification that his Axiom VI "has been found expedient in researches of the most diverse sort" ...

On the other hand, however, it follows that, whenever the left hand term in (3.4) is not zero, and the sequence $P(A_1), P(A_2), P(A_3), \ldots$ contains infinitely many nonzero terms, then it must contain terms $P(A_n) > 0$ which are arbitrarily small.

Furthermore, and clearly, in the usual *finite* case of the additivity property (3.4), namely

$$(3.6) \qquad P(A_1 \bigcup \ldots \bigcup A_n) = P(A_1) + \ldots + P(A_n)$$

with pair-wise disjoint "random" events $A_1, \ldots, A_n$, the Cournot Principle simply need *not* always apply, since *none* of the *finitely* many strictly positive quantities $P(A_i)$ in the right hand term can be arbitrarily small, although at least one of them may nevertheless happen to be "infinitesimal" in the less than clear enough sense of Cournot.

So that, it is precisely here that we see the fundamental difference, and still disregarded *problem* which stares us right into the face the moment *infinity* is introduced into "probabilities" ...

However, other questionable things happen as well with the concept of Kolmogorov probability, and no one seems to care much in the least ...

Indeed, if we take as a Kolmogorov probability space the usual unit interval $[0, 1] \subsetneq \mathbb{R}$ with the usual Lebesgue measure, then every single point $x \in [0, 1]$ defines a one point event $A = \{x\}$ which has probability $P(A) = 0$, and thus it is simply *redundant* probabilistically. Yet, if one now eliminates from $[0, 1]$ all such redundant points $x \in [0, 1]$, then clearly, one remains with the *void* set.
In other words, in the Kolmogorov probability space $[0, 1]$ one *cannot* eliminate but only a rather *small* subset of all the redundant points.
This is clearly in contradiction with what happens in a probability space given by a finite or countable set E, where all points with zero probability can be eliminated, and one remains with a space that is isomorphic probabilistically.
The above strange behaviour even of such a simple Kolmogorov prob-

ability space like $[0, 1]$ is the reason while a whole lot of technical complications arise in the study of time-continuous stochastic processes. And unfortunately, even Nonstandard Analysis, and the respective Loeb Integration, cannot get rid of all such technical difficulties.

4. Axioms Involving INFINITY

The axiomatic method in mathematics was, as is well know, originated by Euclid in his geometry book "Elements" more than two millennia ago. Instead of "axioms", Euclid used the term "postulates". However, in view of the modern mathematical terminology, we shall prefer the term "axioms".
Amusingly, the whole book "Elements" rests only on *five* postulates, or rather axioms, which are the following, see Wolfram MathWorld :

I) A straight line segment can be drawn joining any two points.

II) Any straight line segment can be extended indefinitely in a straight line.

III) Given any straight line segment, a circle can be drawn having the segment as radius and one endpoint as center.

IV) All right angles are congruent.

V) If two lines are drawn which intersect a third in such a way that the sum of the inner angles on one side is less than two right angles, then the two lines inevitably must intersect each other on that side if extended far enough.

This fifth postulate is equivalent to what is known as the "Parallel Postulate".

Euclid's fifth postulate cannot be proven as a theorem based alone on the previous four postulates, although this was attempted by many people, among them a number of outstanding mathematicians. Euclid himself used only the first four postulates - the so called "Absolute Geometry" - for the first 28 propositions of the Elements, but was forced to invoke the parallel postulate for proving the 29th proposition in his book. In 1823, Janos Bolyai and Nicolai Lobachevsky independently realized that entirely self-consistent "non-Euclidean geometries" could be created in which the parallel postulate did not hold. Seemingly, Gauss had also discovered, but suppressed the existence of non-Euclidean geometries, being possibly concerned about the likely negative effects upon his personal reputation, had he supported the existence of such geometries in public.

Amusingly, Euclid, and following him everybody for more than two millennia, that is, until the early 1800s, firmly believed that a postulate, that is, an axiom *must* be obviously and beyond any doubt true. This is why the fifth postulate was not accepted easily, since unlike the first four postulates, it could obviously *not* be verified - and thus confirmed or denied - without involving *infinity*.

Well, the next time the same kind of obstacle appeared with the inevitable involvement of *infinity* in an axiom happened with the essential gap between "probability" p defined in (2.1), and and on the other hand, an assumed to exist unique numerical value ρ for every so called "random" empirical phenomenon which the "probability" p was supposed to approximate arbitrarily well, under conditions such as, for instance, those in the (LLN).
Or put simply, the inevitable involvement of *infinity* happened with the gap between (2.14) and (2.15).

Thus it turns out that *two* rather crucial moments in mathematics when *infinity* appears in axioms, and does so in unavoidable manner are :

First, more than two millennia ago

(4.1) with the fifth Euclidean postulate of parallels,

and then, about two millennia later,

(4.2) with the gap between "probability" and the mentioned assumed to exist unique numerical value of so called "random" empirical phenomena, that is, with the gap between (2.14) and (2.15).

Now, a relevant *difference* between these two cases of axioms, both essentially involving *infinity*, is as follows :

In the case of Euclid's fifth postulate, the two parallel lines involved are *not* supposed to come arbitrarily near to one another. In addition, both those parallel lines are perfectly equally supposed to exist as purely mathematical entities, that is, the existence of none of them is less certain than that of the other one.

On the other hand, in the case of "probability", only the "probability" "p" is supposed to exist, and as such, exist as a mathematical entity, namely as given by the definition (2.1), and further specified in (2.15).

Indeed, the assumed to exist unique numerical value "ρ" associated to every so called "random" empirical phenomenon which the "probability" "p" is supposed to approximate arbitrarily well, under conditions such as, for instance, those in the (LLN), is only an *assumed* existence, without any satisfactory supporting argument ever being given in this regard.
In addition, it is also supposed that the "probability" "p" given by (2.1) must tend to that merely assumed to exist unique numerical value "ρ" of the respective so called "random" empirical phenomenon.

Further, it is worth noting that the difficulties involved with the inevitable presence of infinity in the fifth postulate have been surprisingly and spectacularly solved in the early 1800s, leading to most protean generalizations like Reimannian and yet more general geometries, some of which turned out to be sine-qua-non for General Relativity, for instance.
And amusingly, the situation as a whole proved to be in principle trivially easy : all three logical possibilities can indeed happen, namely, given a line L and a point P outside of it, then every line through P must meet L, or alternatively, there is only one line through P which never meets L, as well as the third possibility, namely that, there may be many lines through P which never meet L.

On the other hand, the difficulties involved with the similarly inevitable presence of infinity related to the gap between the "probability" p, and on the other hand, the assumed to exist unique numerical value ρ associated with each so called "random" empirical phenomenon, value which the "probability" p is supposed to approximate arbitrarily well, has not only been *not* solved at all, but it has rather passed unnoticed. Or like in the case of Kolmogorov approach to "probability", it has been replaced by *enforcing* an ad-hock decision which was not explicitly enough expressed, and even less supported by any satisfactory argument.
And needless to say, therefore : much unlike with the fifth postulate of Euclid, here there is only one single expected outcome. Namely, the "probability" p must always converge to the assumed to exist unique numerical value ρ of the so called "random" empirical phenomenon under consideration.

One important fact should nevertheless be mentioned. Namely, as mentioned in section 3, Kolmogorov does make an *essential* differentiation between what he calls the finite fields of probability, and on the other hand, the infinite fields of probability, without however, touching upon the crux of that difference, namely, the way one deals with the fundamentally important Cournot Principle.

The rather obvious conclusion from the above is that foundationally - and yet hardly known by its various users - the very concept of "probability" is still today on *shaky* grounds.

And as if the disregard of the Cournot Principle would not be enough, recently, the long ongoing hope-charged enthusiasm propagated ever since by results like the (LLN) has rather seriously been punctured by what is called "The Law of Truly Large Numbers", or in short (LTLN), Wikipedia.

Indeed, a simple version of that result goes at follows.

Example 4.1

Assume that a given event happens with a probability of 0.1% in one trial. Then the probability that this rather unlikely event does not happen in a single trial is $99.9\% = 0.999$.

In a sample of 1000 independent trials, the probability that the event does not happen in any of them is 0.999^{1000}, or 36.8%. The probability that the event happens at least once in 1000 trials is then $1 - 0.368 = 0.632$ or 63.2%.

Now, the probability that it happens at least once in $10,000$ trials is $1 - 0.999^{10000} = 0.99995$, that is 99.995%, which of course is *surprisingly* near to certainty, for comfort.

Consequently, this "unlikely event" has a probability of 63.2% of happening if 1000 independent trials are conducted, or over 99.9% for $10,000$ trials. In other words, a highly unlikely event, given enough trials with some fixed number of draws per trial, is even more likely to occur.

Thus one can conclude that with a sample size large enough, any outrageous thing is likely to happen.

Let us now look at the above in a more general setup.

We start with a probability $0 < a < 1$, instead of 0.1% as above.
It follows, as above, that the probability of that less likely event X not happening is $b = 1 - a$, which in the above specific numerical case was larger than $99.9\% = 0.999$.

Let us now take a large integer number, say, $N > 1000$.
Then, the probability in N independent trials that the event X never happens is b^N, while the probability that the event X will happen at least once is $c = 1 - b^N = 1 - (1 - a)^N$.

And it is easy to show that

$$\sup_{0 < a < 1,\, N > 1} (1 - (1 - a)^N) = 1$$

since for every given $0 < \epsilon < 1$, we have

$$0 < (1-a)^N < \epsilon \quad \Longleftrightarrow \quad 1 - \epsilon^{(1/N)} < a$$

and for any given $0 < a < 1$, one can obviously choose $N > 1$ for which the relation $1 - \epsilon^{(1/N)} < a$ holds.

Thus the probability c for the less likely event X to happen in N independent trials can become arbitrarily near to 1, no matter how small the probability a of the event X in one single trial is, provided that N is ... truly ... large ...

This Law of Truly Large Numbers, or (LTLN), is attributed to Persi Diaconis and Frederick Mosteller.

Remark 4.1

It follows that the Law of Large Numbers is but only one side of the proverbial coin.
And the other side is that the convergence in (2.12) guaranteed by (LLN) does *not* in any way mean any sort of steady, orderly, disciplined, let alone, monotonic convergence, since with high probability, it can any time be interrupted by arbitrarily long behaviour contrary to any usual expectation regarding convergence.

Briefly, the *quality* in managing a process merely by "probabilities" is in fact surprisingly far *lower* than what happens to be in the common perception or intuition ...
Consequently, any alleged "foundation" of any more important class of phenomena on "probabilistic" grounds is by it very nature a rather poor performance ...

So much, therefore, for the view that - regarding quanta, for instance - the "probabilistic" concept provides a neat foundation, even if shifted from ontology to epistemology ...

***

The obvious practical effect is that, while the (LLN) tells us the "good news", namely, how much the "probability" may approach the assumed to exist unique numerical value of the so called "random" empirical phenomenon studied, on the other hand, the (LTLN) tells us how *incredibly* "bad news" can with high probability happen nevertheless, and that we do not - and simply cannot - have any guarantee against them ...

So much for the ... enthusiasm ... implied in "probabilities" by purely mathematical results such as the (LLN) ...

5. Quantum Implications

Starting with the "Copenhagen Interpretation" in the mid 1920s, as well as with all other later interpretations of quanta in which the concept of "probability" plays some more or less foundational role, there is in fact inevitably a yet *deeper, prior* and *all overriding* concern, one more important than any possible other ones which have been raised related to the involvement of "probabilities" in such interpretations. Namely, it is the above mentioned concern about the very foundations of "probabilities" as a valid concept ...
And that concern may as well regard the concept of "subjective probability", even if it does so in a way different from the concern about the usual and shaky concept of "probability" ...

Amusingly, this obvious lack of concern regarding the *very first priority* in the "Copenhagen Interpretation" - and of all other interpretations in which "probability" in any form plays some foundational role - seems to have been just about *completely* missed so far by the proud proponents of such "probability" based interpretations of quanta ...

This omission is of course natural, and in fact unavoidable, as long as the awareness about the *shaky* foundations of the presently used concept of "probability" is lacking in general, and thus it is not considered, let alone eliminated in a proper manner.
As for the proposition of using "subjective probability" in the foundations of quanta, the comment in the sequel may apply.

The effect is that the “Copenhagen Interpretation” - together with all other interpretations in which “probability” is foundational - simply becomes *ridiculous*, and does so in more than one way :

- The pride of the unprecedented demotion in physics of “determinism” from its traditional ontological position - which includes as well it ontological position in Special and General Relativity - to a mere epistemic one, followed by its replacement with “probabilities” which are claimed to occupy in quanta the position of the “new foundational ontological” concept, turns out to install in fact but only a *shaky, ill-founded* concept ...

- Even without the above error, the demotion of “determinism” is merely of *one single* step. Namely, “determinism” still remains perfectly valid, this time on the level of quantum *ensembles*, and it is rejected only on the level of *individual* quantum phenomena.

In view of the above, the objection of Einstein, Schroedinger and a few other remarkable physicists, against the demotion by the “Copenhagen Interpretation” of “determinism” from its traditional ontological role still stands, even if seemingly such objection was not based on the awareness that the concept of “probability” is poorly founded ...

As for the possible foundational role of “subjective probability” in quanta, the question obviously is :

> To what extent a foundation upon any sort of “subjectivity” may nevertheless lead to a theory which is not merely “subjective altogether” ?

After all, and for example, both Special and General Relativity, by setting aside the absolute nature of “space” and “time”, do essentially base themselves on the “subjectivity” of “reference frames” and the respective specific “observations”.
And yet, and most obviously, neither Special, nor General Relativity is merely a theory which is “subjective altogether” ...

Could any theory of quanta, based on “subjective probabilities”, replicate such a performance ?

6. And How About Plausibility ?

As also presented in some detail - and in fact as its very first chapter - by E.T. Jaynes in his mentioned book "Probability Theory, The Logic of Science", [4], it is important to note that the rules which associate numerical values to different degrees of "plausibility" do *not* contain any formula with sigma-additivity, like for instance, in (3.4), or in general, formulas referring to an infinite number of events.
Consequently, the concept of "plausibility" is not inevitably liable to the Cournot Principle.

Also, if we denote by $\mathcal{P}lau$ the realm of so called "plausible" empirical phenomena, then obviously

$$(6.1) \qquad \mathcal{R}an \subsetneqq \mathcal{P}lau$$

since as mentioned, the latter are required to satisfy fewer conditions.

On the other hand, just like in the case of "probabilities", the numerical values attributed to various degrees of "plausibility" are again to be in some conveniently approximating relationship with certain assumed to exist and unique values attributable to "plausible" phenomena.

What seems quite remarkable in this regard is that, as seen for instance in [8,9], a good deal of the basics of quantum theory can be reconstituted in the mentioned more general terms of "plausibility", instead of the more restrictive terms of "probability" ...

Appendix : Questionable Issues Regarding the E. T. Jaynes Approach to "Probabilities"

Once again, let us emphasize that the book "Probability Theory, The Logic of Science", [4], by E.T. Jaynes is most definitely worth reading, as it offers a remarkable trove of detailed knowledge regarding "probabilities", or more precisely, regarding attempts to relate the realm of $\mathcal{R}an$ to the realm of $\mathcal{P}rob$, and in that process, to accomplish a considerable development of the latter ...

Here however, we shall mention several questionable issues in that book, and the fact remains that they do not represent anywhere near an exhaustive such presentation of all such issues regarding the views of Jaynes on "probabilities".

First perhaps, we have already such a questionable issue at the very beginning of that book. Namely, there, on page xxi of the Preface, we find the following :

> "From many years of experience with its applications in hundreds of real problems, our views on the foundations of probability theory have evolved into something quite complex, which cannot be described in any such simplistic terms as 'pro-this' or 'anti-that'."

On the other hand, on the very next page xxii of the same Preface, one can read :

> "In our view, an infinite set cannot be said to possess any 'existence' and mathematical properties at all - at least in probability theory - until we have specified the limiting process that is to generate it from a finite set. In other words, we sail under the banner of Gauss, Kronecker and Poincaré rather than Cantor, Hilbert, and Bourbaki."

Certainly, E. T. Jaynes can perfectly be understood regarding his respective feelings - but not necessarily properly qualified views as well - regarding mathematics, given the fact that he was, and considered himself to be, a physicist. And as such, he was not supposed to be much familiar, let alone particularly fond of, modern, let alone the state-of-the-art mathematics of the second half of the 20th century, although much of his life happened to be spent during that period ... However, to the extent that one tries to deal with difficult issues in present day physics, such as among others, foundational ones in quanta, one *must* use mathematics ...

And then, and why not, preferably make use of mathematics at its best, a fact that is well known ever since Galileo Galilei (1564-1642) who is credited with a statement saying in essence that

> "... the book of Nature is written in the language of mathemtics ..."

a comment which was renewed by Eugene Wigner in 1960, in his famous paper "The Unreasonable Effectiveness of Mathematics in the Natural Sciences".

And then, would it indeed be appropriate for a physicists to ... pick and choose ... among mathematicians ?
And moreover, to do so by choosing exclusively those of two or more centuries old generations who had no - and simply could not have - any ideas about the extraordinary achievements of modern mathematics, while at the same time rejecting precisely those top modern mathematicians who contributed most to the best of present day mathematics ?

The amusing aspects involved in the above ... picking and choosing ... of mathematicians by Jaynes, however, do not seem to end so easily, once one embarks upon such a strange approach in science ...
Indeed, why - for instance - could not one choose Ptolemy's cosmological views, instead of those of Copernicus ?
After all, the breathtaking sophistication of those ... circles upon circles upon circles ... of the former would naturally seem not to few to be far far more suited ... there up in the Heavens ..., than the utter poverty in simplification introduced instead of them by the latter ...
Or how about the manifestly erroneous understanding on the part of Jaynes of the Bourbaki group of French mathematicians as being far too modern for comfort, a view which clearly is another source of amusement ... ?
Indeed, the fact is that the Bourbaki group was not exactly ... too modern ...
After all, they did refuse to accept in mathematics the so called Category Theory, introduced in the early 1940s by the American mathematicians Samuel Eilenberg and Saunders Mac Lane. And the reason for that refusal was the view of the Bourbaki group that the mentioned

theory was far too abstract, that is, so abstract as not to be able to have much meaningful input into mathematics ...
Therefore, at least on that count, the members of the Bourbaki group were not at all the most ... modern ... mathematicians at the time ...
On the other hand, what happened was that an exceptional mathematician at the time, Alexander Grothendieck (1928-2014), found it necessary to break with the Bourbaki group due, among others, to their mentioned rejection of Category Theory ...
Furthermore, by the time of the 1990s, while Jaynes was still with us, Category Theory proved to be important not only in mathematics, but also in physics, for instance, in the study of quanta ...
And to add to the amusement, it was the idea of certain mathematicians involved in quantum physics to introduced and develop a considerable generalization of Category Theory itself, namely, the so called n-Categories ...

Regardless of the above questionable picking and choosing, however, the very method suggested by Jaynes to deal with infinity, namely to "have specified the limiting process that is to generate it from a finite set", even when it is considered all on its own, can lead one to ask the following obvious QUESTION :

> When already in ancient Greece they discovered that $\sqrt{2}$ is *not* a rational number, and when later they tried to approximate it better and better, did they do so based on the mentioned "principle" of Jaynes, namely, to have the resulting limiting processes expected to deliver $\sqrt{2}$ in *different* ways depending on the various particular finite features of the infinite sequences involved in its assumed approximation ?

Well, most certainly, no one ever expected to obtain *different* results for $\sqrt{2}$, depending on the specific infinite limiting process that was to generate $\sqrt{2}$ from one or another sequence of finite sets of numbers involved.
Instead, and due to various reasons - still very much valid today - everybody involved in approximating $\sqrt{2}$ could *only and only* think about *one and only one* resulting value for $\sqrt{2}$ ...

And such a basic and elementary *mistake* in understanding infinity, can nevertheless be so loudly and up front be expressed, although there simply cannot be any doubt that Jaynes knew and understood far more mathematics, than to expect different values for $\sqrt{2}$, depending on the specific finite aspects of the infinite limiting processes involved in its approximation, had he been asked that question directly and all on its own, and not in what appeared to be for him an emotionally charged - thus easily biased - context, such as when involved in writing his mentioned book ...

Now the amusing fact is that in modern mathematics, *both* methods may be used, namely, one in which the result of the infinite limiting process does in *no way* whatsoever depend on any specific features of the finite sets involved, or alternatively, in the way suggested by Jaynes.
And all of such choices depend on what is called the different *topological* or other yet more general structures on the sets whose elements are involved in the respective limiting type processes.

A typical and possibly somewhat wider known such example is in Nonstandard Analysis, where for instance the famous novelty of *infinitesimals* is obtained precisely with a method recalling that of Jaynes.

However, still today, $\sqrt{2}$ - as a usual real number - is supposed to have *one and only one* value. And then, that value *cannot* and does *not* in any way depend on particular finite features of the infinite limiting processes supposed to be giving $\sqrt{2}$.

Coming back, however, to the foundational desiderata regarding "probabilities" mentioned in (2.1), (2.14) and (2.15), one is supposed to have some real number $p \in [0, 1]$ which is expected to be equal with, or at least approximate better and better, some other real number $\rho \in [0, 1]$ ...
And then, one may say that we are in the same kind of situation as mentioned above concerning $\sqrt{2}$, a situation when we are looking for a result which should *not* depend on one or another particular algorithm used in its computation ...

Here perhaps, it would be appropriate to recall the way Jaynes deals with the celebrated Bertrand Paradoxes in Probability, [4, pp. 386]. Together with quite a few others, he also suggest a resolution of these paradoxes. However, none of those listed who suggested such solutions, including Jaynes himself, seem to be aware that one important possibility which all of them overlook may be precisely the fact that the mentioned paradoxes are related to the *shaky* foundations of usual theories of "probabilities" ...

However, in order to close on a positive note which, no doubt, the many various outstanding features of the mentioned book of Jaynes fully deserve, let us recall as well the following passage from it, in section 10.7, entitled "But what about quantum theory ?", on pages 327-329 :

> "... Those who cling to a belief in the existence of 'physical probabilities' may react to the above arguments by pointing to quantum theory, in which physical probabilities appear to express the most fundamental laws of physics. Therefore let us explain why this is another case of circular reasoning. We need to understand that present quantum theory uses entirely different standards of logic than does the rest of science. In biology or medicine, if we note that an effect E (for example, muscle contraction, phototropism, digestion of protein) does not occur unless a condition C (nerve impulse, light, pepsin) is present, it seems natural to infer that C is a necessary causative agent for E. Most of what is known in all fields of science has resulted from following up this kind of reasoning. But suppose that condition C does not always lead to effect E; what further inferences should a scientist draw? At this point, the reasoning formats of biology and quantum theory diverge sharply. In the biological sciences, one takes it for granted that in addition to C there must be some other causative factor F, not yet identified. One searches for it, tracking down the assumed cause by a process of elimination of possibilities that is sometimes extremely tedious. But persistence pays off; over and over again, medically important and intellectually impressive success has been

achieved, the conjectured unknown causative factor being finally identified as a definite chemical compound. Most enzymes, vitamins, viruses, and other biologically active substances owe their discovery to this reasoning process. In quantum theory, one does not reason in this way. Consider, for example, the photoelectric effect (we shine light on a metal surface and find that electrons are ejected from it). The experimental fact is that the electrons do not appear unless light is present. So light must be a causative factor. But light does not always produce ejected electrons; even though the light from a unimode laser is present with absolutely steady amplitude, the electrons appear only at particular times that are not determined by any known parameters of the light. Why then do we not draw the obvious inference, that in addition to the light there must be a second causative factor, still unidentified, and the physicist's job is to search for it?

What is done in quantum theory today is just the opposite; when no cause is apparent one simply postulates that no cause exists ergo, the laws of physics are indeterministic and can be expressed only in probability form. The central dogma is that the light determines not whether a photoelectron will appear, but only the probability that it will appear. The mathematical formalism of present quantum theory incomplete in the same way that our present knowledge is incomplete - does not even provide the vocabulary in which one could ask a question about the real cause of an event. Biologists have a mechanistic picture of the world because, being trained to believe in causes, they continue to use the full power of their brains to search for them - and so they find them. Quantum physicists have only probability laws because for two generations we have been indoctrinated not to believe in causes - and so we have stopped looking for them. Indeed, any attempt to search for the causes of microphenomena is met with scorn and a charge of professional incompetence and 'obsolete mechanistic materialism'. Therefore, to explain the indeterminacy in current quantum theory we need not suppose there is any indeterminacy in Nature; the mental attitude of quantum physicists is already sufficient to guarantee it. This point also needs to be stressed, because most people who have not studied quantum theory on the full technical level are incredulous when told that it does not concern itself with causes; and, indeed, it does not even recognize the notion of physical reality. The currently taught interpretation of the mathematics is due to Niels Bohr, who directed the Institute for Theoretical Physics in Copenhagen; therefore it has come to be called 'The Copenhagen interpretation'. As Bohr stressed repeatedly in his writings and lectures, present quantum theory can answer only questions of the form: 'If this experiment is performed, what are the possible results and their probabilities?' It cannot, as a matter of principle, answer any question of the form: 'What is really happening when ...?' Again, the mathematical formalism of present quantum theory, like Orwellian newspeak, does not even provide the vocabulary in which one could ask such a question. These points have been explained in some detail by Jaynes (1986d, 1989, 1990a, 1992a). We suggest, then, that those who try to justify the concept of 'physical probability' by pointing to quantum theory are entrapped in circular reasoning, not basically different from that noted above with coins and bridge hands. Probabilities in present quantum theory express the incompleteness of human knowledge just as truly as did those in classical statistical mechanics; only its origin is different.

Here, there is a striking similarity to the position of the parapsychologists Soal and Bateman (1954), discussed in Chapter 5.

They suggest that to seek a physical explanation of parapsychological phenomena is a regression to the quaint and reprehensible materialism of Thomas Huxley. Our impression is that by 1954 the views of Huxley in biology were in a position of complete triumph over vitalism, supernaturalism, or any other anti-materialistic teachings; for example, the long mysterious immune mechanism was at last understood, and the mechanism of DNA replication had just been discovered. In both cases the phenomena could be described in 'mechanistic' terms so simple and straightforward - templates, geometrical fit, etc. - that they would be understood immediately in a machine shop. In classical statistical mechanics, probability distributions represented our ignorance of the true microscopic coordinates - ignorance that was avoidable in principle but unavoidable in practice, but which did not prevent us from predicting reproducible phenomena, just because those phenomena are independent of the microscopic details. In current quantum theory, probabilities express our own ignorance due to our failure to search for the real causes of physical phenomena; and, worse, our failure even to think seriously about the problem. This ignorance may be unavoidable in practice, but in our present state of knowledge we do not know whether it is unavoidable in principle; the central dogma simply asserts this, and draws the conclusion that belief in causes, and searching for them, is philosophically naive. If everybody accepted this and abided by it, no further advances in understanding of physical law would ever be made; indeed, no such advance has been made since the 1927 Solvay Congress in which this mentality became solidified into physics. But it seems to us that this attitude places a premium on stupidity; to lack the ingenuity to think of a rational physical explanation is to support the supernatural view. To many people, these ideas are almost impossible to comprehend because they are so radically different from what we have all been taught from childhood.Therefore, let us show how just the same situation could have happened in coin tossing, had classical physicists used the same standards of logic that are now used in quantum theory ..."

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