PLAUSIBILITIES IN ECONOMICS

Abstract. Contrary to the frequentist interpretation of probability, I propose to distinguish Popperian propensities ("objective") and plausibilities (information-dependent or "subjective") as two different meaningful applications of probability theory. Plausibilities give meaningful numerical values for single cases and epistemological questions, applicable for rational decision-making under conditions of insufficient information.

The frequentist rejection of numerical plausibilities distorts economic reasoning. This is shown by comparison of Knight's and the Austrian's position on risk vs. uncertainty. Knight's undistorted position appears much more reasonable in comparison with the later frequentist Austrian position.

I propose to correct also some of Knight's concepts. In particular, I identify uncertainty not with impossibility of numerical plausibilities, but as the non-existence of a straightforward algorithm to compute them.

1. Introduction

The conflict about the "interpretation of probability" – "frequentists" against "subjectivists" – causes a lot of confusion in all domains where uncertainty plays a role, and in particular in economics. Austrian economics is no exception.

Roughly speaking the Austrian mainstream (Mises, Rothbard, Hoppe) supports frequentism (see chap. 6 of [1], sec. 8.9 of [2], [3]). But there are also a few supporters of subjectivist probability (like Langlois [7], Crovelli [8]), and the issue is open to discussion and discussed ([9], [10], [11]).

Conceptually I prefer a "neutral" position: Probability theory is a mathematical formalism. There is no such animal as *the* interpretation of such a formalism. Mathematical formalisms often have very different applications, with quite different interpretations. This allows to consider two *different applications* of probability theory at the same time as meaningful in their different domains of applicability.

The two applications of probability theory I accept as reasonable have only a rough correspondence with the frequentist resp. subjectivist interpretations of probability. It is clear that the positivistic nonsense of the original frequentist interpretation (by Richard von Mises [12]) has to be corrected, which has been done by Popper in his "propensity interpretation". In comparison with purely subjectivist interpretation, I prefer the approach of Jaynes [13], who defines information-dependent but otherwise objective plausibilities. They follow an objective logic of plausible reasoning.

To emphasize these differences I name the two applications "propensity" and "plausibility".

Above camps share the error of unjustified complete rejection of the other one. But the rejection of propensities by subjectivists is comparatively harmless: Whenever we have a propensity predicted by a statistical theory, the same numerical probability can be interpreted as a meaningful plausibility. So nothing is lost in applications, and the error reduces to a philosophical one. Moreover, propensities do not play an important role in economics – their natural domain of applicability is fundamental physics.

Instead, plausibilities are extremely important in economics – they describe rational human decision-making under uncertainty and insufficient information. Their rejection by frequentism is fatal – the adequate mathematical apparatus for these applications is lost.

Austrian economic theory of uncertainty is a nice example to illustrate the harm caused by frequentism. The approach used by Ludwig von Mises [1] (1946) (developed by Hoppe [3] (2007)) has been heavily influenced by the positivistic frequentist interpretation as proposed by his brother Richard von Mises [12] (1939). This has caused important differences in comparison with the earlier, classical approach proposed by Knight [14] (1921). Wherever the two approaches differ, I argue that Knight's approach is more reasonable.

It is not that I agree completely with Knight. But his basic distinction between risk and uncertainty (and the corresponding economical conclusion risk = no profit, true uncertainty = profit) remains valid, even if only in a modified way: True uncertainty is not characterized by complete impossibility of numerical plausibilities, but, instead, by the absence of a straightforward algorithm to compute or measure them.

The situation is quite different for the ideas proposed by Hoppe [3]. He proposes a nice but unfortunately wrong series of identifications: Roughly speaking, observable frequencies exist = objective probabilities exist = numerical probabilities exists = natural sciences applicable = risk (insurable hazards), and, reversely, single cases and epistemology = no objective probability = no numerical probabilities = humanities = true uncertainty (uninsurable hazards).

I reject this scheme completely. In particular, I show that: 1.) numerical plausibilities are meaningful and reasonable for single cases as well as epistemological problems, 2.) for single cases there exist even a straightforward algorithm to compute them based on observable frequencies, 3.) given the frequentist definition of true randomness, the risks related with natural accidents do not fit, so, would be uninsurable, so that 4.) almost all what insurance companies insure are, if one follows Hoppe's argument, uninsurable risks.

But plausible reasoning, rejected as "utter nonsense" by frequentism, has also another aspect extremely important for libertarians: It is the logic we really need in our life, the logic necessary for decision-making in a situation of uncertainty, with insufficient information. The ideology behind frequentism – verificationism – supports only classical logic, which cares only about certainty. It tells us how to identify the loopholes in strong proofs, without telling us how to handle the resulting uncertainties. The fear of uncertainty leads, in a natural way, to dogmatic acceptance of some theories and rejection of others, independent of their scientific value. The preferred theories will be those which make promises of certainty, which is quite typical for theories supporting the state. So the message of verificationism is that our private decision-making is inherently faulty, that decision-making has to be left to Big Government Science.

Instead, from point of view of the logic of plausible reasoning everybody is, philosophically, on equal foot with Big Science: All have to follow the same logic, and even if Big Science has more information and, therefore, can obtain better expectations for plausibilities, this is only a difference in degree. There is nothing inherently faulty in our everyday common sense decisions based on our plausibility expectations, and Big Science is not doing anything conceptually better. Its basic message is that one does not have to be afraid of uncertainty – there are rational methods to handle it.

2. Definitions

The main error in the whole issue of "interpretation of probability" is, in my opinon, the very question of "interpretation", which implicitly suggests that there is such a thing as a single "interpretation of probability". Probability theory is a mathematical apparatus, and such a mathematical apparatus has often very different applications. In particular, the mathematical apparatus of a symmetric positive definite tensor field may be used to describe Euclidean distances on a curved surface by a metric $g_{ij}(x)$, but also the stress tensor $\sigma_{ij}(x)$ or the deformation tensor $\varepsilon_{ij}(x)$ in condensed matter theory. The very idea of a conflict about the "interpretation of a symmetric positive definite tensor field" would be ridiculous.

So I think the first thing to be done is to recognize that there are different applications of the mathematical formalism of probability theory, and to distinguish them by different names. One possibility would be to use the notions "objective probability" and "subjective probability". But there are two disadvantages: They are long, and there would be a natural tendency to omit the objective/subjective, and to fall back into the confusion of "interpretation of probability".

Moreover, "subjective probability" is misleading. It is, in fact, more accurately characterized as information-depending. Now, information is usually different for different people, so different people will access different plausibilities to the same objective question. But if different people, with different interests, but in the same circumstances, with the same available information, come to different conclusions about plausibility, the question who is right and who is wrong makes a lot of sense. Intersubjective agreement about this question is not only possible – we have even established notions like "wishful thinking" for typical subjective errors in plausible reasoning. So plausible reasoning is an objective endavour, even if it depends on information.

But let's give now some short definitions:

Definition 1 (plausibility)**.** *A* plausibility distribution *defines for each statement A, in dependence on a set of information I, a* plausibility $P(A|I)$ *– a real number which characterizes the degree of certainty of, or the degree of belief into, the statement A. Greater plausibility characterizes a greater belief, greater certainty. We have $0 \leq P(A|I) \leq 1$, where 0 stands for "absolutely impossible" and 1 for "absolutely certain".*

The plausibility distribution has to fulfill, as a consistency and rationality condition, the rules of probability theory.

Moreover, it has to fulfill additional restrictions of agreement with common sense. In particular, it has to fulfill the following symmetry principle: If the information I contains nothing which distinguishes a set of alternatives A_i, then they are all equally plausible, thus, all the $P(A_i|I)$ have to be equal.

For details, in particular for a more detailed specification of the necessary conditions of consistency and agreement with common sense, as well as the derivation of the rules of probability theory from these conditions, see Jaynes [13].

The definition is obviously *not* an operational one: Nor does it define a general algorithm how to compute plausibilities, nor a prescription how to measure them. Instead, what is defined is a set of conditions which has to be fulfilled by a reasonable plausibility assignment. This is similar to the situation in logic: We have no algorithm how to compute the truth value of a statement given some axioms. The rules of logic only define what is a proof, but do not define a way to find a proof of a given statement from the axioms. In fact, the rules of probability theory are simply the logic of plausible reasoning, and the classical logic appears as the subset of those rules which allow to identify (at least some) absolutely certain statements.

Plausibilities do not have empirical character, so they *cannot* be observed or compared in some other sense with experiment. Instead, they have logical character: They describe what follows, according to the logic of plausible reasoning, from a given set of information.

That does not mean that observation is irrelevant: Observation gives new, additional information, and such new information can require large modifications of our plausibility assignments. But this does not mean that the old plausibilities have been wrong: It means that the old information was insufficient, and that the new information obtained by the observation was important and relevant.

Instead, propensities are part of empirical theories:

Definition 2 (propensity)**.** Propensities *are particular physical hypotheses, derived from physical theories (like, for example, quantum theory), which predict relative frequencies of the outcome of repeatable experiments.*

Observation, in particular the observation of frequencies in repetitions of the experiment in question, may be used to test these hypotheses.

So, different from plausibilities, there is a general and straightforward algorithm how to test propensities: To repeat the related experiments often enough, to observe the frequencies, and to compare them with the propensities: By definition, in the limit of infinitely many experiments, the frequency should agree with the propensity.

2.1. **The differences between propensity and plausibility.** I think these definitions already show that propensities and probabilities are very different things, even with a completely different logical status: Plausibilities appear information-dependent but have certain, logical character, while propensities are information-independent, but have empirical and therefore also hypothetical character.

To illustrate this difference, consider the case of a loaded die. The physical theory that the die is fair is, in this case, simply false, and the observation of frequencies allows to refute this theory.[1]

The situation is different for plausibility: If I have no information which makes a difference between the six possible outcomes, it *logically follows* from the symmetry principle that I have to assign the plausibility $\frac{1}{6}$ to each side. After throwing the dice a few times, I have different information, and the situation changes. In which way, depends again on the information I have, in particular on the plausibility of various theories about loaded dices. But even if the result will be that the die gives always only a 6, it does not mean that the *original* plausibility $\frac{1}{6}$ was wrong in any

[1]Of course, there is much more than the physical properties of the die alone involved in the frequency predictions, namely the method of throwing (see [13] chap.10) and the properties of the environment (think of a magnetic die and a variable magnetic field of the table). Nonetheless, all this has to do with physics and is predictable in principle by the physical theory.

way – it was all what could be extracted from the available information, and *this* result is completely certain, is a mathematical theorem.

So we have, in fact, the paradoxical situation that the numerical plausibilities, rejected by the frequencies as "utter nonsense", may be (of course only in some situations) derived as strong mathematical theorems, while propensities always remain hypothethical.

On the other hand, let's note that this mathematical, logical character of plausibility makes the problem of establishing the plausibility of a statement arbitrary complex: If there exists a mathematical proof $A \Rightarrow B$, then $P(B|A) = 1$. But what if I have no idea that such a proof exists? If my knowledge of mathematics is so rudimentary that the possibility of existence of a proof $A \Rightarrow B$ seems as plausible to me as that of a proof $A \Rightarrow \neg B$ or the non-existence of any such connection? This does not matter – it is my personal problem. Given the information A, I can derive that B is certain, even if only in principle. The information which distinguishes B from its negation $\neg B$ is available to me, even if I do not recognize this. And there is no algorithm which allows me to recognize this, because there is no algorithm which allows me to decide if a mathematical statement can be proven from a given set of axioms.

So conceptually, philosophically propensity and plausibility don't have much in common. What above notions share are only the properties they share with the mathematical apparatus of probability, and the nice but irrelevant point that all three notions share the scheme "p . . . ity", with the consequence that the mathematical denotion $P(.|.)$ looks natural for all of them.

3. A priori probabilities

The notion "propensity" is a reference to Popper, who has introduced it in his propensity interpretation of probability [16]. It is a variant of the frequency interpretation proposed by Richard von Mises [12], but differs from the original, positivistic frequency interpretation in the same way as Popper's fallibilism differs from the positivistic concept of derivation of scientific theories from observation.

The first important difference is the priority of theory: Following Popper, theories are free inventions of the human mind. Even if their creators may be influenced by results of observations or by inductive reasoning, the ideas used to develop the theory have no importance for the evaluation of the theory. Observation is used to evaluate the theories after they have been proposed: First, the theory has to be proposed by somebody. Then, predictions about the outcome of experiments have to be derived from the theory. And only after this, these predictions can be compared with the observed outcome of the experiments. This is a logical sequence: In real time, the experiment may have been done before the presentation of the theory and may have motivated its creation. But this is logically irrelevant – we cannot tell if the theory is supported by the experiment or not before the theory has been presented, and before the prediction for the outcome of the experiment has been derived from the theory. In this sense, theories are in Popper's methodology in general a priori. Propensities are part of the theory, and are, therefore, a priori too.

The second important difference is the hypothetical character of the theory, and, as a consequence, of the propensities as part of the theory too.

In physics (and I think in other natural sciences too) the positivistic idea of derivation of a theory from observation is as dead as possible for a philosophical theory, and the propensity interpretation as presented here is in fact what physicists have in mind if they describe themself as supporters of the frequency interpretation.

But it seems that this part of the positivistic doctrine, long dead in physics, has survived in Austrian economics. At least Hoppe classifies the position of Mises in this way:

> With this definition of class probability, Ludwig von Mises shows himself in complete agreement with his brother. For him, too, there is no such thing as a priori probability ([3] p. 9),

without distancing himself. Moreover, discussing an argument proposed by Knight in favour of the reasonableness of a-priori probabilities, namely that

> [i]f the die is really perfect and known to be so, it would be merely ridiculous to undertake to throw it a few hundred thousand times to ascertain the probability of its resting on one face or another ([14] p. 215),

Hoppe supports the positivistic rejection of a priori probabilities by inferring a counterargument

> Richard von Misess reply to this definition can be inferred . . . : Precisely. But this definition only shows that there is no such thing as a priori probability. Because in order to classify a die as perfect, one must first show this to be true and that cannot be done other than by means of long-run observations ([3] p. 8).

Then he characterizes Knight's position in a not really supporting way:

> his deviation turns out little more than a minor if unfortunate slip ([3] p. 7).

But maybe there really is a point against a priori probabilities? No. If Richard von Mises argues

> How is it possible to be sure, that each of the six sides of a die is equally likely to appear. . . . Our answer is of course that we do not actually know this unless the dice . . . have been the subject of sufficiently long series of experiments to demonstrate this fact ([12] p. 71, as quoted by [3] p. 6),

the straightforward Popperian reply is that, first, we do not even claim to be sure – propensities are always hypothetical – and that, second, a series of experiments can never give certainty.

In fact, if the die is fair, then the sequence $\{6, 6, 6, 6, 6, 6\}$ is as probable as any other particular sequence, say, $\{1, 5, 3, 6, 2, 2\}$, namely $(\frac{1}{6})^6$. What makes the two sequences qualitatively different is that the first one makes a particular alternative theory – that the die is unfair and gives a 6 with much larger probability – much more plausible, while there is no such alternative theory getting advantages in the second case. But anyway this gives only plausibility, not certainty.

So I see no need for further argumentation. But one should not forget below that there is disagreement even about this.

3.1. **About true randomness.** An important part of the original, positivistic frequency interpretation is the "Principle of Randomness or the Principle of the Impossibility of a Gambling System", which is also supported by Hoppe:

> The second condition to be fulfilled is that of "randomness." In Richard von Misess words, "only such sequences of events or observations, which satisfy the requirements of complete lawlessness or randomness [are true] collectives." In order to employ the probability calculus, it must be impossible to devise "a method of selecting the elements so as to produce a fundamental change in the relative frequencies" (R. Mises 1957, p. 24). "The limiting values of the relative frequencies in a collective must be independent of all possible place selections" (pp. 24 – 25; ...). Or as Ludwig von Mises expressed the same requirement: for every element of a class it must hold that nothing is known about its attributes under consideration but that it is an element of this class (and that everything is known about the relative frequency of specified attributes for the class as a whole). ([3] p. 12f)

An example provided by Richard von Mises illustrates this condition:

> Imagine, for instance, a road along which milestones are placed, large ones for whole miles and smaller ones for tenths of a mile. If we walk long enough along this road, calculating the relative frequencies of large stones, the value found in this way will lie around 1/10. ... The deviations from the value 0.1 will become smaller and smaller as the number of stones passed increases; in other words, the relative frequency tends towards the limiting value 0.1. (R. Mises 1957, p. 23)
>
> [t]he sequence of observations of large or small stones differs essentially from the sequence of observations, for instance, of the results of a game of chance, in that the first sequence obeys an easily recognizable law. Exactly every tenth observation leads to the attribute "large," all others to the attribute "small." (p. 23)
>
> The essential difference between the sequence of the results obtained by casting dice and the regular sequence of large and small milestones consists in the possibility of devising a method of selecting the elements so as to produce a fundamental change in the relative frequencies. We begin, for instance, with a large stone, and register only every second stone passed. The relation of the relative frequencies of small and large stones will now converge toward 1/5 instead of 1/10. ... The impossibility of affecting the chances of a game by a system of selection, this uselessness of all systems of gambling, is the characteristic and decisive property common to all sequences of observations or mass phenomena which form the proper subject of probability calculus. ... The limiting values of the relative frequencies in a collective must be independent of all possible place selections. (pp. 24 – 25) ([12] as cited by [3] p. 4)

What is the place of this principle in the propensity interpretation? The notions of "place selection" and "gambling strategy" are a little bit unfortunate, because experiments which may be used to test statistical theories do not have an order, except in very special but accidental circumstances. In fact, in statistical theories like quantum theory an experiment is defined by a procedure of state preparation. This procedure is necessarily incomplete – at least the moment of time cannot be fixed completely, because this would prevent any repetition and, as a consequence, any observation of frequencies. But there are, of course, much more parameters than time which have to be left unspecified. So, a "gambling strategy" is simply a more complete specification of the experiment, which fixes some of the remaining infinite number of unspecified conditions. Now, according to the theory in question, this more completely specified experiment is a valid experiment, thus, the theory predicts the same frequency. So, if the more complete specification gives another frequency, the theory in question is false. So there is no need for an additional "principle of randomness" for propensities – it is automatically part of the statistical theory in question.

In this sense, it is even part of approximate theories. Now, for approximate theories we usually know that the principle of randomness does not hold – that the more fundamental theory allows to make more specific descriptions so that the resulting frequencies differ from those predicted by the approximation. But what would be the point of this? We know, last but not least, anyway that the approximate theory is in a strong sense false, else we would not name it an approximation.

Nonetheless, for discussing the consequences of the "principle of randomness", it seems useful to distinguish the case of approximation, where it is not even claimed that the principle holds – we will name this "approximate randomness" – from a fundamental theory, like, in particular, quantum theory, where the question if the principle of randomness is fulfilled is a serious, viable hypothesis. The last case deserves to be named "true randomness".

In itself, true randomness is not observable. In a world with strong encryption, there the NSA would be extremely interested to learn a method to distinguish with more or less certainty, by observation, a file containing random numbers from a truecrypt container (different from observing the filetype being ".tc"), the idea of establishing true randomness by observation of a random sequence is quite naïve. The best one can hope for is to distinguish truely random sequences from those created by special encryption algorithms.

3.2. **What is measured by relative frequencies?** But what has happened with the quite plausible basic idea of frequentism that probabilities are what is measured by relative frequencies of repeatable experiments?

The point is that it is not clear, without any theoretical background, what is a frequency, and what a repeatable experiment. What is such a repeatable experiment is, quite obviously, a theory-dependent notion. Indeed, to define the experiment, one has to specify everything which influences the outcome completely. If we forget to fix some relevant parameter in the general specification, different experimenters will consider this parameter as irrelevant, and in their experiments this parameter will have different values. The results will be different, the parameter which caused the difference is not considered, not known, and no reasonable prediction is possible.

But what are the parameters which have to be fixed to obtain a unique result, or at least a unique frequency, is clearly theory-dependent. There is no abstract, theory-independent principle which allows to distinguish relevant from irrelevant parameters. And in different experiments at least some parameters have to be different – at least position in space and time.

We see, yet again, the complete meaninglessness of the empiricistic idea to derive theories from observations. It is not even clear what is an observation without the specification of a theory.

Now, consider the specific case of a theory which postulates some probabilities as fundamental, and another, better theory, which recognizes that there is, in fact, a gambling strategy. Then, the meaning of a complete description of an experiment is different in above theories. What is a complete description from point of view of the first theory does not specify the gambling strategy – there is no such gambling strategy according to this theory, or, in other words, any proposals for gambling strategies are irrelevant parameters which do not change the observable frequencies.

From point of view of the second theory, the description of the experiment is simply incomplete. One has to specify which of the different gambling strategies is used. Without this specification, the resulting frequencies are not completely specified.

So what is measured if one simply measures frequencies, without any theory which prescribes what is the complete preparation procedure of the experiment, is therefore easy to guess – utter nonsense.

This argument should not be taken too seriously – the assumption "without any theory" is quite strong, and, in fact, whenever one observes some frequencies, there exists an easy to formulate and simple theory: That the conditions used to distinguish an experiment from everything else which happens in the world specify the outcome as much as possible, so that the remaining uncertainty fulfills the principle of true randomness. This "theory" is in most cases nothing one has to take seriously, in particular it is usually quite clear that it can be only a case of approximate randomness. But, because of such simple "theories", the assumption "without any theory" is, in fact, never fulfilled in reality. Everybody has some "theories".

4. Is there anything wrong with approximate propensities?

The distinction between true and approximate randomness is important because the frequentist case against numerical plausibilities is based on the assumption of true randomness. Or, more accurate, on the fact that statistical theories applicable to human behaviour make sense only as approximations, with approximate randomness. Indeed, here is the argument as presented by Hoppe:

> The randomness (or homogeneity) assumption can be made vis-a-vis events of the accident variety. For instance, we know nothing about the attribute of any particular bottle (will it break or not?) except the bottles membership in a class of bottles (of which we know the probability of bottles breaking or not); and we know nothing about the attribute of any particular throw of a die (will it be a 6 or not?) except the throws membership in a class of dice throws (of which we know the probability of throwing sixes). In the case of human actions this assumption is incorrect, however. In the case of human actions, "we know," writes Ludwig von Mises, "with regard to a particular event, *some* of the factors determin[ing] its outcome" (L. Mises 1966, p. 110 emphasis added). Hence, insofar as we know more about a single event than merely its membership in a given class of events of which we know the frequency of certain

> attributes, we are, with regard to human actions, in a better position to make predictions than we are in the case of "accidents," where nothing about particular events – one bottles vs. anothers breaking – is known. ...
>
> Based on this general knowledge concerning the nature of human actions as opposed to accidents, then, we are in possession of a method which, according to Richard von Misess frequency theory, we are most definitely not allowed to possess if the probability calculus is to be applicable: namely a method of "place selection." We know of no rule how to distinguish one bottle from another as far as breakage is concerned (otherwise they would not be "classed" together). However, for any presumed collective of action-events (such as "men watch basketball on TV tonight" or "I watch basketball on TV nightly") we do know of such a rule. ([3] p. 13-14)

A quite ridiculous argument, if one thinks about it: If we know less (no method of "place selection"), it follows that we know more (numerical probabilities which are otherwise nonsense).

4.1. **Approximations remain meaningful and useful.** But let's nonetheless consider it in more detail. I do not doubt that we are, in the case of human actions, not in a situation of true, fundamental randomness. It is a situation of approximate randomness – better theories are, in principle, available, so that we know that the propensities predicted by our approximate theories do not always predict the frequencies accurately.

The natural question of a natural scientist is simple: So what? Okay, there is no true randomness, only approximate, but that's nice: There is a method to improve our predictions. Additional possibilities are always fine. But there is no obligation to apply them. One can use them or leave them. The original, approximate method does not become worse if we find a way to predict with more accuracy. It remains as good, as accurate, as before. There is nothing which could make it meaningless or utter nonsense to use the approximation.

In fact, the mere theoretical possibility of future improvement of scientific theories is sufficient to show the absurdness of the rejection of approximations. It may be that even our most fundamental theories will be replaced, in some future, by a more fundamental, better one. But in this case, the general argument against the use of approximations applies to our current application of the actually best scientific theories too – utter nonsense remains utter nonsense. Instead of computing some utter nonsense using the best available scientific theory, we would better rely on intuition or whatever else – those who argue against numerical probabilities do not specify what would be a better replacement for the utter nonsense.

4.2. **The intuition behind the argument.** Whenever one rejects an argument, one would better care about the question what supports the argument. Is there some modification of the argument which appears defensible?

In our case, there is such a justified part. Consider the case where the better theory wins on the market. Our point was that this does not diminish the accuracy of the inferiour theory. So, once it was reasonable to use the approximation as long as it was the best available theory, it does not become utter nonsense after this.

But even if this is correct, the interesting, important question on the market is a different one: If you don't follow the progress, if you continue to use old methods of production, you do not have higher costs – the costs remain the same. But you nonetheless loose in the competition on the market: Other competitors can provide the services in a cheaper way. And this leads to a quite clear intuition: Once a better method is avaiable, it has to be used if possible. To continue to use the old method becomes, in this sense, nonsensical. To survive on the market, one has to use the new, better one.

And this intuition is correct in all the cases where the costs of collecting the necessary additional information are not prohibitively high.

But recognizing this, we see that the conclusion is a completely different one: Instead of using the old, inaccurate approximation (giving numerical probabilities), we should use the new, more accurate theory, which also gives numerical probabilities, only more accurate ones. So, fine, the numbers given by the old approximation are nonsense, but not because they are nonsensical by themself, but only because better, more accurate numbers are available. The intuition that the old numbers are nonsense is, therefore, not at all an argument against numerical probabilities in case of approximate randomness.

To throw the old numbers away without using the new, better ones would be even more stupid than to use the old ones.

4.3. **The costs of higher accuracy.** Moreover, there may be even good reasons to prefer the approximation. First of all, reasons which are especially relevant in economics – the additional costs of application of the better theory.

In the typical case of natural sciences, the equations of a more fundamental theory are more complicate, and whatever the available methods to solve them, they may simply appear unsolvable with these methods. Even if they are solvable, one needs much more resources to solve them. The laptop may be no longer sufficient, one needs a supercomputer. Or much more time.

The equations are not the only problem. There are also human resources. The more fundamental theory requires more sophisticated scientists or engineers to manage them. They have received a more expensive education, want higher wages.

But the most important and interesting problem is that of access to additional information. For a more accurate prediction one needs more accurate data. The typical physical theory needs sufficiently accurate initial data. Without them, an accurate evolution equation is of not much help. And the more fundamental the theory, the more data are necessary. For theories which are economically relevant – theories about natural accidents as well as about human behaviour – the problem of access to the necessary data is equally relevant. Of course, with possible rare exceptions, more accurate theories need more and more accurate data.

And access to additional data is almost always connected with costs. The situation may be even worse – that the additional data are simply not accessible because even the most accurate measurement devices are not sufficient. Or, in human action, because it is not in the interest of the other participants to give you accurate data. In fact, the more interesting the data would be for you, the higher the probability that the other actors are not interested to give you access to them. What would be the highest price you would pay for some object? You may have no problem to tell this almost everybody, except the one most interested in this information – the seller.

So the very existence of a method to improve the accuracy of the predictions is not only irrelevant for the question of accuracy and meaningfulness of the original, approximative method. It may be economically unreasonable or even practically impossible to apply the better method.

4.4. **The case of moral costs.** Are there other reasons, except for the costs of using the better approximations?

Let's see: What would be the conditions which distinguish these exceptional cases? Given the considerations above about the costs, one condition would be that the costs for obtaining the additional information are irrelevant. The typical situation would be that the additional information is known anyway in any particular case.

The second condition is that the outcome depends on the additional information in a sufficiently strong way.

The reaction of other people on you going nude is different on a nudist beach and in a church. Not a really good example, because nobody defends here the "approximation" that the difference between nudist beach and church doesn't matter.

But there is another example where the information is easily available: The various cases of racist, nationalist, religious or sexist "prejudices". Here, the situation is different: We have an ideology – the ideology of equality of all human beings – which forbids to use statistical differences between races, nations, religions, gender and sexual orientation, to discriminate between people.

Discussing this ideology in detail is beyond the scope of this paper. It seems quite plausible that a state which does not discriminate between people, whatever their gender, race, religion, nationality, or sexual orientation, is less evil than a state who does. But I would not wonder if, similar to Hoppe's comparison of democracy with monarchy, somebody finds good arguments against this thesis. At least one negative side effect is the increasing invasion of the state into the the private freedom of contract, which includes the freedom to discriminate, to refuse to make contracts with people one does not like, for whatever reasons.

Whatever, this is not the point I want to consider here. The point is that a lot of people have a lot of private theories about statistical differences between races, nations, religions, man and woman, and sexual orientations. And they use these private theories for their private decision-making. And, different from the nudist beach vs. church example, we have here a moral argumentation that this is wrong, that these theories should be rejected, that to apply them is morally wrong.

There are other examples of ideologies which try to force us to ignore various statistically important differences. Animal rights argue that the differences between humans and other animals should not matter. Sexual abuse activists argue that it should not matter if a child willingly participates or not. And those who disagree are morally blamed – as racists, nationalists, sexists, religious fanatics, child abuse advocates, mass murderers of animals and so on.

It is not our point to argue if one or another of these ideologies is morally justified or not. The point we want to make here is a simple one: That the only interesting, relevant cases where it is irrational to use the "approximation", but where it is nonetheless argued that one should use the "approximation", are the cases where it is argued that it is morally comprehensible to use the better theory even if it would be reasonable.

But, justified or not, ethical rules which forbid to use the easily available information can be considered as another type of costs, moral costs. For firms, such moral costs can lead to real costs related with a loss of reputation, and, as a consequence, of customers. As far as these moral costs are also shared by all market participants, they also do not lead to profits.

5. The natural accident – human action distinction is irrelevant

Let's note also another, even if only minor, point. While there may be a relevant quantitative difference in the predictability of human behaviour, conceptually the situation is not different at all from the case of natural accidents. The claim of true randomness can be made only for the most fundamental theory, which is, in our current situation, quantum theory. Here, the final judgement is yet open. All other theories are approximate theories, thus, the propensities they propose are only approximate too.

The situations where the claim of true randomness may be justified correspond, in fact, nicely to the cases where Knight has considered a-priori probabilities as justified – the cases where the probabilities can be derived from fundamental theory. We disagree with Knight about the domain of applicability of a-priori probabilities – the approximate probabilities of approximate theories are also a priori – but Knight is clearly excused. The point that all physical theories, even approximative ones, are a priori, instead of being derived from observations, has been made only later by Popper [16]. If we ignore this point, Knight's distinction between a priori and statistical probabilities is almost exactly the one between true randomness and approximate randomness we have in mind:

> As an illustration of the first type of probability we may take throwing a perfect die. ...
>
> On the other hand, consider the case already mentioned, the chance that a building will burn. It would be as ridiculous to suggest calculating from a priori principles the proportion of buildings to be accidentally destroyed by fire in a given region and time as it would to take statistics of the throws of dice. ([14] p. 215)

And we also agree with Knight in the observation that the cases where one can apply the first type (Knight's "a priori probability" or my "true randomness") in economics are almost irrelevant, rare exceptions. This holds for natural accidents as well as for human actions.

So it follows that, if the condition of true randomness is what distinguishes the domain of applicability of numerical probabilities, then numerical probabilities are inapplicable and meaningless even in the case of almost all the results of natural sciences relevant to economics.

Reformulated in another way: Whatever the statistics about economically relevant natural accidents, like fire, hurricanes, earthquakes and so on, we can be sure that the condition of true randomness is never fulfilled. And that means that, if this condition is relevant, all the use of statistics in economically relevant questions is utter nonsense. Or, if one uses true randomness as the criterion which distinguishs insurable from uninsurable risks, then almost all the risks insured by insurance companies are uninsurable. This seems to me a sufficiently strong argument – if a theoretician argues that what insurance companies actually do is utter nonsense, that the risks they insure are uninsurable, I would think the survival of the insurance companies on the market is sufficient evidence that the theoretician errs.

One could think about saving the distinction by the argument that it is not the existence, in principle, of a violation of true randomness, but that we know a general method – the method of understanding. But in the case of approximate physical theories we also know a general method – to consider the more fundamental theory, the theory which predicts at least some differences between the predictions of the approximation and reality. This is also a general method – the only place where it is not applicable are the most fundamental theories. These are the theories where we have, if they predict randomness at all, automatically the prediction of true randomness. Else, the theory would not be fundamental.

In view of the much stronger arguments below, the point that natural accident – human action distinction is irrelevant seems to be only a minor point. Moreover, the argument about the market success of insurance companies can be made even if the distinction would be relevant. In fact, lot of insurance companies insure risks related with human action and use numerical frequencies to estimate their risks.

But in fact it is more important: The scheme natural disasters – numerical probabilities, human action – no numerical probabilities is not that much the result of insight into the reasonableness of the use of statistics by different types of insurance companies, but corresponds to a philosophical prejudice which is quite fundamental to the Austrian approach – the rejection of methodological unity of science, and the strong prejudice against applications of the methods of natural sciences in economics.

I think the Austrian rejection of the "methods of natural sciences" is largely misguided, caused by a misunderstanding of the methods of natural sciences. Many of the arguments are justified – as arguments against empiricism, applicable in the natural sciences as well. I agree here with Popper's concept of unity of science, based on critical rationalism as a common base. Unfortunately, Popper's critical rationalism remains quite unknown, hidden behind a positivistic, trivialized fake version of his teachings, which leads, for example, to arguments against the unity of science as the following one made by Hoppe:

> In the natural sciences, success means that so far your hypothesis has not been falsified; apply it again; and failure means that your hypothesis as it stands is wrong; change it. In our dealings with our fellow men, the implications are not, and never can be, as clear-cut. Maybe our prediction was wrong because some people, as can happen sometimes, acted out of character – in this case we would want to use our hypothesis again even though it had been apparently falsified. ([5] p. 73)

Unfortunately for this argument, Popper has never claimed that falsification is certain. Instead, he correctly insists that all scientific statements, including the basic statements which falsify theories, have always hypothetical character (cf. [16][2]). So I simply don't see a categorical difference: Before the quote given above, Hoppe

[2]For example "...daß die wissenschaftlichen Sätze, da sie intersubjektiv nachprüfbar sein müssen immer den Charakter von Hypothesen haben" (p. 19), or "sollen auch die Basissätze intersubjektiv nachprüfbar sein, so kann es in der Wissenschaft keine "absolut letzten" Sätze geben, d.h. keine Sätze, die ihrerseits nicht mehr nachgeprüft und durch Falsifikation ihrer Folgesätze falsifiziert werden können" (p. 21)

gives a description of the method of understanding which is, at least for me, indistinguishable from a description of theory-building about human behaviour from point of view of critical rationalism.

The problem of unity of science is nonetheless a complex one and deserves to be considered in detail elsewhere. The aim of this section was merely to show that this particular point about a categorical distinction between natural sciences and economics fails.

6. Plausibilities

Even if one does not exclude (as required by frequentism) approximate randomness, the domain of applicability of propensities is quite restricted to repeatable situations where the notion of an observable (at least in principle) frequency makes sense. This is far too restrictive for human decision-making.

First, there is the problem of missing information. A theory can make detailed predictions, but these depend on initial values not available for us in real decision-making.

More serious is that we have to make decisions about single events – given all the known and probably relevant information, a similar event has never happened in the past and will probably never happen again. So, statistics are imaginable only in principle.

But even more serious is the situation of uncertainty about our theories what is true. In the actual world, one theory is true, and remains true forever, and no frequencies are even imaginable here.

All these domains are, nonetheless, covered by the concept of plausibility.

Plausibilities can be assigned to everything which has a truth value, which may be true or false. This covers single events as well as the truth of our theories or hypotheses. They have to follow the logic of plausible reasoning, which is an extension of classical logic. A derivation of the logic of plausible reasoning from simple first principles of consistency and agreement with common sense has been given by Jaynes [13].

6.1. **The necessity to decide.** There exists also another justification for plausible reasoning – by derivation from principles of decision theory. From a philosophical point of view, the independent derivation, which relies fundamentally on consistency of thinking and common sense principles, seems more satisfactory than a derivation from pragmatical principles of decision-making: Logic is something more fundamental.

But from point of view of economics, from praxeology, a derivation from decision-making seems more powerful: Given the necessity of decision-making, a rejection of the best available method for solving this problem becomes indefensible.

It is not the aim of this paper to present this justification. The algorithm which appears, according to decision theory, the only rational, consistent algorithm (as defined by agreement with some rationality principles), is a quite simple one: One has to optimize the expectation value $E(u|d) = \int_X u(x)p(x|d)dx$ of some utility function $u(x)$ in dependence of our possible decisions d. This expectation value depends on a mathematical probability distribution $p(x|d)dx$, which describes the probabilities of the possible outcomes x of our decisions d. While the utility function $u(x)$ may be arbitrary (that means, is not restricted by rationality principles), the values of $p(x|d)$ have to fulfill the rules of probability theory. Else, the decision-making becomes logically inconsistent.

With this concept of justification, the very idea that the probabilities $p(x|d)$ have to be observable by frequencies becomes nonsensical. We have to make decisions, even if we don't have such frequencies. So we have to make some choices. To compute $E(u|d)$ may be easier if the $p(x|d)$ are simply observable as frequencies. But if they are not, we nonetheless have to make some choices for the $p(x|d)$, and we would prefer to make them in a consistent way, given the information available for us.

But this is already all we need to justify the application of numerical plausibilities for everything which may be (or become) true or false.

Imagine some quite arbitrary question: Will Barca win the next game against Real or not? Then one can always imagine that there appears somebody who makes an offer: "I bet 10:1 that Barca wins". Then you have a decision-making problem under uncertainty where the otherwise possibly uninteresting question becomes interesting. May be it is reasonable to accept the bet? The rational way to decide this is to assign some numerical plausibility to this single event.

A practical decision may depend on epistemological questions as well. On your mountain trip, you have to use the left path or the right path. You see a mountain before you, and your decision depends on the correct identification of this mountain on your map. If it is point X on your map, you have to turn left, else you have to turn right. There are some hints, like points you have identified with more or less certainty before, expectations about the distance you have walked from the last certainly identified point on the map, and so on. This is not a single case probability, but epistemological probability: This mountain before you is marked as point X of your map, or it is not, no frequency makes sense here.

The possibility not to decide often does not exist. Not to follow one of the paths would be as stupid as the behaviour of Buridans ass, who stands, hungry, equidistant from two equally attractive bales of hay, unable to decide between them, does not decide, and starves to death. From point of view of praxeology this is also a decision – the decision not to decide between them (cf. [2] p. 310).

You don't like the algorithm proposed by decision theory? Present a better one. Do you have such a better proposal?

This is, of course, a rhetorical question. You don't have. This is the very point of decision theory – every alternative is inconsistent, violates common sense and simple rules of rationality.

But I do not have to rely on this derivation. The burden is on your side: It's you who has to propose a better method if you think the decision-theory method is utter nonsense. This is the point I want to make here: There is a practical necessity to decide. And therefore we need some methods to make decisions.

The question is not if one or the other method of decision-making is nice. The only relevant question is which of the *available* methods is the best one. To reject all available methods as "utter nonsense", without proposing an alternative, is – utter nonsense.

The method of decision theory is, because it depends on plausibilities, only a partial one. The word "algorithm" would be misleading. It presumes that we can start the algorithm, without thinking ourself, and the algorithm returns some optimal result. It is only a part of the problem which works that way. Nonetheless, we know at least that this part is consistent. We know the rules of logic of plausible reasoning which should not be violated – else we become inconsistent, and our decisions irrational. This is not a complete answer. It is only a set of helpful rules.

But for a set of helpful rules the same principle holds: Once we have to make decisions, to reject available sets of helpful rules as "utter nonsense", without proposing better sets of helpful rules, should be qualified as utter nonsense.

7. Plausibilities for single cases

Hoppe, following the positivist position, has a clear and certain opinion about numerical probabilities for single events:

> This, then, brings us to our final conclusion. Frank H. Knight and Ludwig von Mises are entirely correct in insisting that the use of numerical probabilities is impossible in our daily endeavors of predicting our own and our fellow mens actions. As Richard von Mises, the originator of the frequency interpretation of probability, has unambiguously stated: the application of the term probability to a single event is "utter nonsense." It is possible to speak about numerical probabilities only in reference to a properly defined collective. ([3] p. 19)

But Knight has to be defended as innocent in this accusation. Here is what Knight thinks:

> In the first place, nothing in the universe of experience is absolutely unique any more than any two things are absolutely alike. Consequently it is always possible to form classes if the bars are let down and a loose enough interpretation of similarity is accepted. Thus, in the case above mentioned, it might or might not be entirely meaningless to inquire as to the proportion of successful factory extensions and the proportion of those which are not. In this particular case it is hard to imagine that anyone would base conduct upon a judgment of the probability of success arrived at in this way, but in other situations the method could conceivably have more or less validity. We must keep in mind that for conduct a probability judgement based on mere ignorance may be determining if it is the best that can be had. ([14] p. 227-228)

At least I see a large distance between the frequentist's rejection of any numerical plausibility as "utter nonsense" and Knight's recognition that it might be meaningful, or sometimes even "the best that can be had". The following quote illustrates the meaningfulness of numerical plausibilities from Knight's point of view even better:

> Take the case of balls in an urn. One man knows that there are red and black balls, but is ignorant of the numbers of each; another knows that the numbers are three of the former to one of the latter. It may be argued that "to the first man" the probability of drawing a red ball is fifty-fifty, while to the second it is seventy-five to twenty-five. Or it may be contended that the probability is "really" in the latter ratio, but that the first man simply does not know it. It must be admitted that practically, if any decision as to conduct

> is involved, such as a wager, the first man would *have to* act on the supposition that the chances are equal. ([14] p. 218-219, emphasis added)

The point is that human beings have to make decisions, and this necessity does not disappear if they do not have enough information. And the rational way to decide in such a case is to base the decision on plausibility, as obtained from the available information. Note, in particular, the "have to": A clear recognition of the obligatory character of the symmetry principle of plausible reasoning once no distinguishing information is available.

The frequentists leave the poor man without any recommendation for reasonable decision-making. All they tell him is that the rational method of plausible reasoning is "utter nonsense". Not really helpful. What do the frequentists recommend him? Probably to leave the decision to the government, or to government-paid scientists, to rely on the states consumer protection laws. Clearly, Knight does not make this error – he recognizes very well that decisions have to be made, and, once the information we would like to have is not available, one has to base the decision on insufficient information.

Not misguided by frequentism, Knight was also well-aware of what insurance companies really do to estimate their risks:

> Thus in the case of fire risk on buildings, the fact that the cases are not really homogeneous may be offset in part by the use of judgment, if not calculation. It is possible to tell with some accuracy whether the "real risk" in a particular case is higher or lower than that of a group as a whole, and by how much. This procedure, however, must be treated with caution. It is not clear that there is an ultimate separation between the calculation of departures from a standard type and more minute classification of types. There is, however, a difference in form, and insurance companies constantly follow both practices, that of defining groups as accurately as possible and also that of modifying or adjusting the coefficient applied within a class according to special circumstances which are practically always present. ([14] p. 216)

So, far away from rejecting numerical probabilities, the proposal is to use the *additional* information available in single cases to *improve the numerical estimates*. Note, in particular, the specification of what is possible: To tell if the risk is "higher or lower than that of a group as a whole, and by how much". These are not only clearly numbers ("how much"), these are numbers relative to those for the group. So to compute the risk itself, one needs both these relative numbers and the numbers for the whole group. Far away from becoming meaningless, the numbers for the whole group are *necessary*, as the base for improvement by the additional information available.

7.1. **Independence.** We have already mentioned the symmetry principle of plausible reasoning: If one has no information distinguishing the cases A_i, one has to assign equal plausibility to them. A particular but very important application is that of independence. If we have no information about a particular connection between some condition A and another condition B, then we have to assign equal plausibilities to B given A or given $\not A$:

$$P(B|A) = P(B|\neg A) \tag{1}$$

But this is the condition of statistical independence:

$$P(A \wedge B) = P(A)P(B). \tag{2}$$

Independence is, by the way, also the central element of the condition of true randomness. That an experiment with outcome property B is random means that probability $P(B)$ does not depend on any other property A. That means, the relative probability for B under condition A, which is $P(A|B) = P(A \wedge B)/P(A)$, should be unchanged, thus, equal to $P(B)$. Thus, true randomness means the hypothesis that there does not exist any observable specification A which distinguishs between the elements of the class which is not independent.

Or, in other words, true randomness is simply an extremal variant of the independence assumption: If I do not have any information about conditions which may have an influence on the outcome, the logic of plausible reasoning prescribes to assume true randomness.

But what to do if I have non-trivial information which connects the additional condition A with the outcome of interest B? The interesting information is a correction factor for the original plausibility $P(B)$:

$$c = \frac{P(A \wedge B)}{P(A)P(B)} = \frac{P(A|B)}{P(A)} = \frac{P(B|A)}{P(B)} \neq 1. \tag{3}$$

With this correction factor, I have the new plausibility of B given the additional information A:

$$P(B|A) = cP(B) \tag{4}$$

This is straightforward probability theory, without any additional assumptions.

7.2. **The case of a lot of different conditions.** So far for a single bit of additional information. The problem of a single case is that I have a lot of different additional information A_i, and every bit of it is known to be relevant for the outcome B. That means, we have a lot of interesting information of the form

$$c_i = \frac{P(A_i \wedge B)}{P(A_i)P(B)} = \frac{P(A_i|B)}{P(A_i)} \neq 1. \tag{5}$$

But what if we combine them all? How does their combination $A = A_1 \wedge \ldots \wedge A_n$ influence the outcome B? Let's assume that we have no information at all about the connection between the different A_i. But that we have no such information makes the principle of independence applicable: It tells us that the A_i have to be independent from each other. The consequence is surprisingly simple:

$$c = \frac{P(A \wedge B)}{P(A)P(B)} = \frac{P(A|B)}{P(A)} = \frac{P(A_1|B)\cdots P(A_n|B)}{P(A_1)\cdots P(A_n)} = c_1 \cdots c_n. \tag{6}$$

This describes nicely how the insurance companies can obtain information how much a particular condition A_i makes the risk higher or lower than that of the whole class – this is precisely the factor c_i, and a possible way to identify this factor is to use the statistics of the whole class, by formula (5). And this way works for all the conditions A_i taken separately.

Once (by assumption) the insurance company does not have any additional information which suggest a correlation between the particular conditions A_i, formula (6) precisely prescribes how to obtain the plausibility for their combination.

This formula is applicable independent of the number of different additional conditions. So it may be applied to the single case as well – the case where we have so much additional information that all the conditions taken together – the combined condition $A = A_1 \wedge \ldots \wedge A_n$ – is so specific that such a case has never happened in history and will probably never happen again, but describes only the particular single case we care about.

So it is not only wrong that single case probabilities do not make sense at all. We have even found a quite general formula, a formula which even *has to* be applied if we do not have information about dependencies between the various specific conditions of the special case, which computes this single case probability based on available frequencies for large groups.

7.3. **The case of dependent additional conditions.** But what if the independence assumption is not justified – if we have additional information that two conditions, say A_1 and A_2, are connected with each other?

If you are an insurance company with a large enough database you can check this – you have to look at the number of cases where A_1 as well as A_2 are present and check if the corresponding frequency of events with B is sufficiently close to the product c_1c_2. There are more complex possibilities to be checked – are there combined effects between three conditions A_1, A_2, A_3 or is the corresponding frequency sufficiently close to $c_1c_2c_3$? And so on: One can check if four or five conditions, taken together, fulfill the independence condition in a sufficiently close way. But this method has a natural boundary of applicability: The number of relevant events stored in the database of your company becomes smaller and smaller, and correspondingly the accuracy of your frequencies becomes smaller and smaller.

Let's give a partial answer to the question posed by Knight – about the subdivision between the two methods he has considered: To consider homogeneous subgroups as specific as possible, and to correct the result using individual factors. If the conditions are *really* independent, the two methods give the same result. But, given that the database for the factors c_i is greater, these factors themself are more accurately computed separately, instead of using the whole database to compute the combined factor $c_{12} \approx c_1c_2$. Instead, it is useful to consider a subdivision into homogeneous groups if we have dependent parameters $c_{12} \not\approx c_1c_2$.

But are we restricted to these two methods? Certainly not – these methods are the methods which are simple enough to be handled without any computer power, with all computations made by hand. And we have introduced here only a few basics of Bayesian mathematics, to give an impression about the very idea how this possibly works.

Modern insurance companies have powerful computers, large databases, and can use much more sophisticated Bayesian software programs to obtain better results. This does not make the formula (6) crap. It remains a reasonable approximation, and it remains obligatory if no information about the dependencies is available. It depends on the situation (on the importance of the dependencies) how accurate it is. And its accuracy does not decrease even if modern, better methods of estimating the plausibilities of single cases give more accurate results.

But with the improved methods the insurance companies using them will obtain better estimates, and this would allow them to offer their policies for better prices and to avoid some bad risks. In this – and only in this – sense it would be nonsensical to use formula (6) today: Not because it is a numerical probability for a single case, but because the competitors probably have a better method, which gives them a more accurate value for the numerical plausibility of the same single case.

And, to repeat it again: Once we have no information about the dependencies between the conditions A_i, formula (6) is the formula we *have to* use, if we don't want to violate the logic of plausible reasoning. This is a proven theorem. And if we don't have any information about the value c_i for a particular A_i – if it isn't even clear if it increases or decreases the plausibility of B – we *have to* assume independence and to use, correspondingly, $c_i = 1$. Nobody *without* additional information about this can do anything better, and any departure from these simple rules is irrational.

8. Epistomological plausibilities

There is another class of problems where common sense is not afraid of assigning numerical plausibilities but the frequentists becomes horrified and considers this to be even more nonsensical than single case numerical plausibilities – plausibilities for the truth of various theories. It is this type of uncertainty, and not single event probabilities, where frequencies can be used to to obtain plausibilities by formula (6) and its improvements, which really deserves to be considered separately. And, in fact, Knight has proposed to separate this type of uncertainty:

> The confusion arises from the fact that we do estimate the value or validity or dependability of our opinions and estimates, and such an estimate has the same form us a probability judgment; it is a ratio, expressed by a proper fraction. But in fact it appears to be meaningless and fatally misleading to speak of the probability, in an objective sense, that a judgment is correct. As there is little hope of breaking away from well-established linguistic usage, even when vicious, we propose to call the value of estimates a third type of probability judgment, insisting on its differences from the other types rather than its similarity to them. It is this third type of probability or uncertainty which has been neglected in economic theory, and which we propose to put in its rightful place. ([14] p. 231-232)

Indeed, one may think that the situation here is much worse, conceptually completely different. Single case probabilities have, at least, some similarity to other events, and it is this similarity which we have used to obtain our estimates – our central formula (6) was based on frequencies.

But, first, the argument of necessity of decision-making works for epistemological uncertainty too. Different theories lead to different predictions of the consequences of our actions, so different beliefs of their validity lead to different decisions. And, correspondingly, common sense does assign numerical plausibilities to these questions too, so that the usual decision-making procedures can be applied.

Second, the logic of plausible reasoning works in exactly the same way – it does not make any difference between statements about properties of single events and statements about the truth of theories. Statements are statements, A and B, and what is relevant for reasoning are the logical connections between them.

This seems to be a good place to introduce Bayesian updating: The rule known as "Bayes' theorem" is a simple standard theorem of standard probability theory, and it gives a simple algorithm how to update plausibilities if one receives new information, new evidence, new data:

$$P(A|D,O) = P(A|O)\frac{P(D|A,O)}{P(D|O)} \tag{7}$$

The new plausibility $P(A|D,O)$ of A given new and old data D,O depends on the plausibility $P(A|O)$ of A given the old data O and the relation between two plausibilities of the new data D given the old data O – with an without the assumption that A holds. What is the A here? It doesn't matter. If A is some single event, Bayesian updating describes how to improve the single case plausibility. If A is the assumption that a given theory is true, Bayesian updating describes in a quantitative way if and how much the data support the theory. The logic of plausible reasoning is the same.

And already by looking at the formula we see some of the qualitative properties of scientific methodology, as described by Popper [16]. Compare for example a theory A which makes an accurate prediction with an alternative B which does not. If the data are close to the prediction of theory A, $P(D|A,O)$ will be much larger than $P(D|B,O)$, which distributes the plausibility over a much larger domain of possible results, thus, cannot have a large plausibility for these particular data. In Popper's language, above theories are not falsified, but we should prefer the one which makes the more accurate predictions. But, on the other hand, if the data do not fit, the prediction of A, $P(D|A,O)$ will be much lower than $P(D|B,O)$. In Popper's language, A is falsified by this observation. In other words, the logic of plausible reasoning gives a quantitative version of Popper's logic of scientific discovery. And it also answers a question Popper was unable to solve: The question when a statistical theory should be considered as falsified. For numerical plausibilities, the problem does not appear at all – all the "falsified" theories formally survive, only with extremely small plausibilities. And so they are in principle ready for revival, in the case when some extremally strange data falsify all other theories.

In this sense, falsification is always only hypothetical – again in agreement with Popper. It would be interesting to consider this correspondence in much more detail, but this is beyond the scope of this paper.

But it is not only the decision between a discrete set of different theories where numerical plausibilities work nicely. Usually physical theories have continuous parameters – natural constants. And their values have to be established by observation. Unfortunately, measurements are inaccurate, and so the natural constants can be identified only inaccurately. This problem of parameter estimation is also an epistemological one – we do not observe frequencies of worlds with different parameters for the natural constants. So, from the point of view of a consistent and consequent frequentism, parameter estimation should be rejected as utter nonsense, leaving our physical theories without any values for the natural constants.

Real frequentists have not been that rigorous. What they have done was to leave parameter estimation to common sense and intuition. Here I can only recommend to read Jaynes [13].

The conclusion is that the domain of epistemology is also not at all defining a limit of numerical plausibility. Its methods work there too, in agreement with the requirements of consistency and common sense. So in this case I disagree with Knight: The same logic of plausible reasoning is applicable to this type of uncertainty too.

9. Do we need propensities at all?

Given the results we have found – namely that almost all economically relevant probabilities are plausibilities and not propensities – one may wonder if we need propensities at all. In fact their domain of applicability seems rather small: Even if one can use the notion of propensity for approximate theories too, given that they are predictions of particular theories, the very fact that additional information – that about more accurate theories – is available suggests that they are more accurately classified as plausibilities.

If we follow this argument, the notion of propensity will be restricted to the most fundamental theories, and remains only a possibility: At the current moment, we have two most fundamental theories – quantum theory and general relativity. One of them – general relativity – is deterministic, the other one – quantum theory – is indeterministic. So the notion of true propensities is, today, reduced to a single theory, and it is an open question if a better, deterministic theory may replace this theory too.

But we would not give up the notion of a "true theory" only because at a given moment of time only a single theory can make this pretension. We wouldn't give it up even if no theory would be able to make this pretension. In the same way I see no reason to give up the philosophical idea of true, fundamental randomness. And propensity is the notion of probability appropriate for the description of this true, fundamental randomness.

I see no argument which would exclude the possibility of true, fundamental randomness. Crovelli argues that "human action presupposes a causally deterministic world" [8]. That's amusing. The necessary conclusion, that human action alone is sufficient to prove that quantum theory in insufficient, that it has to be replaced by deterministic theory, is ridiculous. And, in fact, Crovelli's argumentation reduces to confusion of notions: Quantum theory, even if it predicts only probabilities, is a time-invariant theory and its fundamental equation – the Schrödinger equation – is a causal evolution equation, and has even some deterministic aspects: The quantum state, as described by the wave function $\psi(q,t)$, is uniquely defined by its initial value. As a consequence, in quantum theory the *propensities* are defined by time-invariant, causal, deterministic laws. Not the events themself. For human action this is completely sufficient – to act, one does not have to presuppose certainty, it is sufficient to presuppose that the action *probably* leads to a more satisfactory situation.

Propensities may be the appropriate notion also in another situation – the case of deterministic chaos. This is the quite typical situation where, even if the fundamental equations are deterministic, small errors in the initial values increase exponentially, so that after a short time prediction is impossible because the accuracy with which initial values can be known is bounded. Here objective laws of nature prevent us from knowing the initial values with high enough accuracy.

Whatever, in economics the notion of propensity is quite irrelevant. The discussion if our most fundamental theory – quantum mechanics – shows true randomness or not has to be left to physicists and philosophers with a suffcient background of quantum physics.

10. Risk and uncertainty

Knight [14] has distinguished the two different types of randomness, risk and uncertainty, as having different economic consequences. The point of the distinction is that the first one, risk, is computable and can be managed, if necessary insured. As a consequence, risk leads to additional costs, but in a similar way for all market participants, and, therefore, does not lead to profit. Instead, uncertainty is the type of randomness which cannot be handled in such a way. There is no standard algorithm what to do, how to handle this, and different market participants will handle uncertainty in different ways. Some of them are more successful, and this leads to profits for them, and, on the other hand, to losses for those less successful.

With the frequentist interpretation, it appeared natural to connect the two types of randomness in economics with the two different types of unpredictability distinguished by the frequency interpretation. The idea is simple: Risk is where we have frequencies and can apply the frequency interpretation to measure probabilities. For everything else, in particular for plausibilities of single events or theories, numerical probability is nonsensical, and, therefore, one cannot use numbers for economic computation too. This is the domain of uncertainty.

This was a quite nice, simple scheme: On the one hand, risk = probabilities observable by relative frequencies = related costs can be expected, if necessary pooled = fixed costs for all market participants = no profit. On the other hand, uncertainty = no frequencies = no numerical probabilities = no way to compute expected costs = market participants follow intuitions or whatever else = different abilities to handle this lead to profits and losses.

Unfortunately, as I have shown, it has nothing to do with reality:

- Contrary to frequentism (but in rough agreement with Knight) there are a priori objective probabilities – propensities – which are derived from statistical theories. But Knight correctly notes "that the first, mathematical or a priori, type of probability is practically never met with in business" ([14] p. 215).
- For economically relevant insurable risks one cannot derive propensities from fundamental theory and has to use observable frequencies. But there is not even a minimal hope that the frequentist principle of non-existence of a gambling strategy holds. What the observed frequencies allow to estimate are therefore plausibilities, not true propensities. What insurance companies do in almost all their time is, therefore, to insure risks which, according to frequentism and Austrian economics, are uninsurable.
- This holds for natural accidents as well as for risks related with human behaviour. In contradiction with the Austrian ideas about a fundamental conceptual distinction between uncertainty of human action and natural accidents, uncertainties related with human actions are also insurable and actually insured by insurance companies, and the related risks are estimated using numerial plausibilities.

- The situations classified as "case probability" by Austrian economics are in principle insurable, with risk estimates based on observable frequencies for classes which do not fulfill the requirements of frequentism.
- The accuracy of the estimates of plausibilities depends on the available information. The access to this information is related with costs. Sometimes, the use of the information may be also connected with moral costs. Because of these costs, the existence of better information ("gambling strategies") may be sometimes economically irrelevant.

But maybe there is nonetheless something which survives from this scheme?

There is. The first thing which survies is the purely economic part of the argument. In other words, I consider the definitions "risk is the type of randomness which leads to no randomness-related extra profits or losses" and "risk is the type of randomness where all market participants have a straightforward algorithm which allows to estimate their uncertainty-related costs in an objective way" as equivalent. The same holds, correspondingly, for the analogical two definitions of uncertainty.[3]

Then, the simple intuition "observable frequencies ⇔ risk" remains valid – as a first approximation. The amusing point is, of course, that the economically relevant observable frequencies do not fit into the criteria for applicability of frequentism. So, despite being seemingly quite similar, the distinction between class probability and case probability proposed by Ludwig von Mises [1] has to be given up. It has everything wrong – the border where observable frequencies may be used to estimate plausibilities (which includes single events) as well as the meaninglessness of numerical plausibilities in the case of approximate uncertainty.

What survives are, in other words, the original ideas presented by Knight, undistorted by frequentism.

10.1. **The boundary between risk and uncertainty.** The disagreement with Knight about the applicability of numerical plausibilities to our opinions and estimates has been already discussed in section 8. But there is a modification of Knight's rejection which remains at least approximately acceptable: "no observable frequencies ⇔ no *algorithm to obtain* numerical plausibilities ⇔ uncertainty".

In fact, the rules of Bayesian probability theory, or the logic of plausible reasoning, give us, in the general case, only incomplete algorithms: There are, in particular, evolution equations for plausibilities defined by various scientific theories, but they need initial values for plausibilities. There is the algorithm of Bayesian updating, but it needs prior plausibilities of the new data. There is the decision-making algorithm, but it needs the plausibilities of the outcomes in dependence of the possible decisions. Sometimes this is sufficient, and leads to sufficiently straightforward algorithms. Sometimes not. The case where we have no simple, straightforward algorithm is the one which can be characterized by true uncertainty.

To characterize the domain of "true uncertainty", we have to recognize the following:

- As explained above, almost all economically relevant observable frequencies are not "frequencies" in the frequentist's definition. But their observation can be nonetheless used (via Bayesian updating) to obtain in many cases

[3] The only point I see is the case of insider knowledge: Accurate information may be in principle accessible (it doesn't matter if as certainty or propensity) but known, as a trade secret, to only one participant. While we are therefore, in principle, in a situation of insurable risk, the insider gains extra profit because the competitors have no access to the objective truth.

quite reasonable and accurate plausibilities in a quite straightforward way. So there are some algorithms (not in contradiction with the "no frequencies – no algorithm" intuition) which work if common sense frequencies are available.

- It has to be recognized that single case plausibilities fit into the "frequencies available" part: The single cases are elements of various larger classes, and the rules of plausible reasoning allow to combine the frequencies observable for these larger classes to obtain specific plausibilities for single events.
- There is also a domain of epistemological questions where algorithms exist: Estimates for the values of natural constants (unknown parameters of our theories) by measurements. Here, statistics about the results of different measurements are available. These are not frequencies of the natural constants – the constants are always the same – but nonetheless the standard methods applicable to frequencies are applicable here too.
- Even in the domain where frequencies are available unpredictable new inventions are possible: It may be found, for example, that some parameter previously considered to be irrelevant appears relevant. So there are aspects of true uncertainty also in this domain.

Nonetheless, the domain of true uncertainty has been identified quite accurately by Knight.

10.2. **The abilities of good entrepreneurs.** The understanding that true uncertainty is nonetheless part of the domain of applicability of numerical plausibilities, and that this domain is covered by strong logical rules of plausible reasoning, allows a much better characterization of the abilities of good entrepreneurs:

- The problems in the domain of true uncertainty can be at least as complex as mathematics: Classical mathematical logic is a particular case of the logic of plausible reasoning. Thus, the better thinker has an advantage in plausible reasoning.
- It can be at least as complex as science, because new experimental data change the plausibilities of scientific theories, proposing a new scientific theory changes the plausibilities of existing scientific theories, and the plausibilities of everything else depend on the plausibilities of scientific theories. Thus, the better scientist has an advantage in plausible reasoning.
- In particular new theories can completely change the whole picture, and there is no algorithm to find new theories. Thus, the better inventor has an advantage.
- It depends on the available information. So the ability to obtain information, and to distinguish relevant from irrelevant information, gives an advantage.
- Irrational people, who violate the logic of plausible reasoning, end up with inconsistent plausibilities.

So the intuition that decision-making under true uncertainty is an art, where some people appear more successful than others, remains to some degree valid. But, based on the logic of plausible reasoning, we can obtain some insight into this art. We can obtain an understanding which qualities are relevant for decision-making under uncertainty and why and how they matter. Not much room is left for mysticism about almost magical abilities of successful entrepreneurs. A rational mind, the thinking abilities one needs to be a good mathematician, a good scientist, a good inventor, appear useful for the good entrepreneur too. Of course, intuition plays a large role. But it is not a mystical, magical kind of intuition, but an ability to think faster, better than other people. Different from mystical abilities, it can be developed by exercise.

So plausible reasoning appears useful for improving our understanding, our "verstehen", of entrepreneurship. Such a better understanding of entrepreneurship as rational, based on rational, logical reasoning instead of magical intuitions, is important for the clarification of the role of entrepreneurship in economics. The importance of this Austrian understanding for libertarian thinking has been nicely demonstrated by Dempster in a comparison of the Austrian and the Post-Keynesian approach to uncertainty:

> These very different views on the nature of entrepreneurship give rise to corresponding differences in policy prescriptions. For an Austrian economist, it is the presence of the entrepreneur that ensures a competitive "discovery process" in which economic actors attempt to change the current state of affairs, thereby "systematically contributing to the coordination of plans" (p. 93). The absence of the entrepreneurial function in the Post Keynesian framework, however, leads to the recognition of uncertainty in economic life without its market-based solution. It is unsurprising, therefore, that dealing with this rampant uncertainty becomes the foremost problem for Post Keynesian policy to solve. Their solution, which they naturally attribute to the brilliance of Keynes, is that government must step in with policies that reduce uncertainty and/or mitigate its effects. ([15] p. 79)

But if one starts with an understanding of uncertainty as a situation where we have nothing, no rational base, to guide us, the successful entrepreneur becomes automatically something like a magician, able to do things which other people are completely unable to do, based on mystical intuitions instead of scientific, rational thinking. And with such magicians as entrepreneurs it becomes much harder to defend the positive, organizing, coordinating role of entrepreneurship in economics recognized in Austrian economics. The post-Keynesian ignorance of this role – together with its pro-government consequences – becomes more plausible.

11. Against verificationism

The comparison between Knight [14], on the one hand, and the Austrians Ludwig von Mises [1] and Hoppe [3] on the other hand, has shown a lot of differences, and they all have been in favour of Knight. The reason for this is also quite clear: Ludwig von Mises, as well as Hoppe, have been aware of the ideology of frequentism proposed by Richard von Mises [12], and they have been influenced by this ideology in a sufficiently strong way to classify them as frequentists. In particular, they have accepted the frequentistic rejection of numerical probabilities for single events as well as for epistemological problems.

But frequentism in itself is also not without a background itself. In fact, frequentism is a quite natural result of the application of empiricism, or, more accurate, verificationism, to the domain of probability theory.

Indeed, the key idea of empiricism is the derivation of scientific theories from observation. But in the domain of plausible reasoning there is nothing to observe: It is about the question what can be concluded and derived by plausible reasoning from a given set of available information. This is an extension of logic, and has nothing to do with observation. The only point where observation plays a role, where it allows to "measure" probabilities, is the domain where frequencies are involved.

But, as we have seen, not only plausible reasoning has been excluded, even most of the reasonable applications of frequencies have been excluded. The reason is the other key idea of empiricism, a key idea it shares with classical rationalism – verificationism, the hope for certain, proven, verified knowledge. But the frequencies which we usually observe – all of them at best approximate – are obviously not good enough for defining true, verifiable, final knowledge. This is possible only in the case of true randomness. All the cases where randomness may be caused by insufficient knowledge have to be excluded – here, the progress of science may lead a progress of knowledge and, in this way, invalidate a theory "derived" from these frequencies.

But behind this I see a general danger of verificationism, independent on the particular application in probability theory: The hope for certainty, the focus on certain knowledge naturally leads to a rejection of hypothetical knowledge, of plausible reasoning, and of everything we need in an uncertain world.

According to Popper, there is no certain knowledge in the domain of empirical science. So a consistent, consequent verificationist has to reject all empirical science, and consequently ends in scepticism. In practice, verificationists are not that consistent. They accept some theories as verified, while other theories are rejected as hypothetical, speculation. The choice is rather arbitrary. Sometimes, the results are even paradoxical, as in the case discussed here: It is the part of application of probability theory which is at least possibly certain, which has mathematical, logical character – the logic of plausible reasoning – which has been rejected by frequentism, while empirical frequencies, which in principle can never give absolute certainty, have been embraced as the only certain thing.

11.1. **The connection between etatism and verificationism.** The rejection of plausible reasoning seems to me a powerful method of improving the power of the state.

In fact, plausible reasoning is what the common man has to use in his decision-making. The world around him is uncertain, his knowledge incomplete, and the information available to him not optimal. So, to improve his power of reasoning, one would have to improve, first of all, his abilities in plausible reasoning – the very base of his reasoning abilities.

This is, of course, not what the state needs. The ideal citizen of a state is a helpless citizen, a citizen who asks politicians to help him, to protect him from the uncertain, dangerous world outside, from the unpredictable decisions of other people.

So what could be a better policy for the state than discreditation of common sense, of plausible reasoning?

And verificationism is an ideology which rejects plausible reasoning. What cannot be verified, what is uncertain, should be rejected as nonsensical. So verificationism is an ideal philosophy in support of the state.

It is not my intention to suggest that some evil philosophers have invented verificationism in some conspiracy to support an almighty state. The fear of uncertainty is a quite natural fear, so verificationism is a quite natural philosophy – it is completely rational to prefer certain knowledge to uncertain, hypothetical knowledge. This natural preference, in combination with wishful thinking, leads quasi automatically to verificationism, supports its popularity, among supporters of the state as well as among libertarians and anarchists.

But this does not change two facts: First, that despite its attractiveness, verificationism is simply wrong, a result of wishful thinking. And, second, the point I make in this section: That verificationism is an ideology which, quasi automatically, strengthens the state. People indoctrinated by verificationism will be afraid of uncertainty much more than justified by the natural preference for certainty. As a consequence, they have much less use for freedom.

And it does not help much to explain them that the state cannot hold its promises to guarantee certainty, that regulation by the state makes, in the long run, the situation even more uncertain. Because the expectation which of two alternatives gives more certainty, given that above are in principle uncertain, is already part of plausible reasoning – the very thing which is rejected as utter nonsense by verificationism.

It is, instead, the very nature of verificationism that some parts of our knowledge are accepted, despite their hypothetical character – this hypothetical character is simply ignored. This ignorance is natural, it is part of the fear of uncertainty. Accepting the hypothetical character of these parts of knowledge would lead to scepticism, the rejection of all knowledge as uncertain. So the very fear which makes verificationism attractive also supports the dogmatization of some parts of our knowledge. But which parts of our knowledge are dogmatized in this way, and which are rejected? This is unpredictable in principle, in the sense that it has nothing to do with the real scientific value of the theories, or with their degree of plausibility or certainty – the choice is based on fear of uncertainty, not on rational evaluation, because rational evaluation would recognize, at first, that all the alternatives are uncertain.

So other, irrational, factors will play a decisive role. Such irrational factors are, in particular, the promises made. It seems quite plausible that a theory which makes unjustified promises, but gives at least some hope, will be preferred in comparison with a theory which does not even promise certainty.

And so it is not strange at all that theories idealizing the state, the democratic decision-making process and so on will be preferred in comparison with much more rational theories about markets. Of course, libertarian theories have a much better rational foundation, make predictions about the failures of various state regulations which have been corroborated a lot of times, but they do not promise certainty. The state promises certainty. Does libertarian theory have a chance as long as people are heavily infected by the wishful thinking of verificationism? Certainly not as long as the libertarians themself are not free of this irrational ideology.

11.2. **The role of plausible reasoning in libertarian education.** From the point of view of this insight, the content of education today, in government-controlled schools, seems quite natural and reasonable – for the state.

Indeed, what could be a better support for the state than teaching children the results of Big Science – about physics, chemistry, biology, mathematics? Results which are impressive and interesting, but in no way necessary or even useful for almost all of them, instead of the methods of plausible reasoning? What could be a more powerful source of helplessness of the people than teaching them classical logic instead of the logic of plausible reasoning? Classical logic shows them all the points where insufficient knowledge, simplified assumptions, insufficient information prevent us from obtaining certain results. But it remains silent about what to do if our knowledge is insufficient or our information is restricted.

Indeed, think about what you learn if you learn classical logic. Take the simple example of independence. What classical logic tells you is that independence is an *additional assumption*. One cannot simply assume that two conditions are independent. No, this is an open scientific question and has to be studied. You obviously cannot do this – you don't have the time and the access to the data to do the statistics. This has to be left to Big Government Science.

Plausible reasoning teaches something very different: You don't have any information which suggests any dependence? So you *have to* assume independence. Clear and simple. It may appear that, in fact, there is a dependence. But so what? Even if you fail, you have made the most rational decision given the information available to you. So there is no reason to blame yourself.

In government schools children learn that plausible reasoning – what they learn in everyday life, outside the school, on the street – is faulty, uncertain. What they learn are some results of Big Government Science. Results which are themself also uncertain in principle, which need the same plausible reasoning for their justification. But this is not what is teached. Big Science, as it is teached in the school, is Certain. And what the children themself learn, in fact, is that they are stupid in comparison with Big Science, unable to survive in the uncertain world without the help of Big Government Science, so that it is much safer if they will be protected by Big Government from the evils of an uncertain world.

What should be teached, instead, to children in a libertarian world? Various abilities which are really useful for them. In the modern world there is, in fact, not much *everybody really* needs – a reason to reject the very idea of obligatory schools. But among these abilities useful to everybody is – together with the ability to read, write, and some elementary mathematics – a domain of knowledge which is not teached today even at universities: The ability of plausible reasoning, the ability to make rational decisions based on the available information in an uncertain world. It may be the most important ability at all.

Of course, given the aversion caused by obligatory schooling against the things children are forced to learn, it may be even better that plausible reasoning is not teached there, but left to the family and the street. But this is already a different question.

Plausible reasoning is an ability which is dangerous for Big Government, because it makes the people less helpless, more confident in their own abilities, more certain that they can survive without Big Brother, more independent, and therefore more opposed to government control of their own lifes.

12. Conclusions

Frequentism, especially in its original, positivistic version, has a fatal influence on economic reasoning, because it rejects an important part of scientific, rational reasoning – the whole domain of plausible reasoning. Once plausible reasoning is used by human actors, in particular entrepreneurs, as well as by insurance companies, its understanding is important for economics.

In particular, such an understanding is necessary to distinguish correctly cases of insurable risks from true uncertainty. The influence of frequentism has been fatal here: Even if there seems to be, at a first look, no big difference between the risk-uncertainty distinction proposed by Knight and the class probability-case probability distinction proposed by von Mises, the latter one, motivated by frequentism, is completely misguided and economically irrelevant. Instead, there are simple rules of plausible reasoning for computing numerical plausibilities for single events derived from oberservable frequencies. These data can and have been used by insurance companies to estimate risks related with natural accidents as well as human actions in such "single cases".

The class probability-case probability distinction is not only irrelevant for the distinction between insurable and uninsurable risks, but completely misguided for economic considerations, because the condition of true randomness is never fulfilled in economically relevant situations, with a few irrelevant exceptions for lotteries.

The logic of plausible reasoning requires also some modification of Knight's position on uncertainty. Even in the domain where no simple formulas based on frequencies are avaiable, the domain named uncertainty by Knight, rational reasoning as well as numerical plausibilities are far away from being nonsensical. In particular, in the domain of epistemology, plausible reasoning allows to derive a quantitative version of the rules of falsificationist methodology of science. It also allows to handle, in agreement with common sense, the problem of estimation of natural constants by uncertain measurements.

Nonetheless, there remains an important domain where no straightforward algorithm is available to derive the interesting plausibilities. This is the domain of scientific, mathematical, technological as well as economical invention – a domain which is indeed worth to be distinguished as a domain of true uncertainty, even if numerical plausibilities are meaningful here too.

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