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# Probabilities as Ratios of Ranges in Initial-State Spaces

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**Abstract** A proposal for an objective interpretation of probability is introduced and discussed: probabilities as deriving from ranges in suitably structured initial-state spaces. Roughly, the probability of an event on a chance trial is the proportion of initial states that lead to the event in question within the space of all possible initial states associated with this type of experiment, provided that the proportion is approximately the same in any not too small subregion of the space. This I would like to call the "natural-range conception" of probability. Providing a substantial alternative to frequency or propensity accounts of probability in a deterministic setting, it is closely related to the so-called "method of arbitrary functions". It is explicated, confronted with certain problems, and some ideas how these might be overcome are sketched and discussed.

**Keywords** Deterministic chance · Method of arbitrary functions · Initial-state space · Objective probability

## 1 Objective Probability

Objective interpretations of probability imply that probability statements are made true or false by reality, by facts independent of our state of mind or information. What are those facts, what are the "chancemakers"—if we take "chance" to be synonymous with "objective probability"? I will start by briefly discussing the two most popular types of answer to this question, namely, frequency and propensity accounts. This

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discussion will be sketchy; its purpose is to introduce some ideas, to highlight some major shortcomings of the standard accounts, and thus to motivate the search for an alternative approach to objective probability.

According to the various frequency interpretations, the chancemakers are relative frequencies of outcomes of random experiments: either hypothetical ones or actual ones, perhaps a bit rounded off by simplicity requirements. The latter is done by David Lewis in his much discussed and currently very influential best-system analysis of laws of nature, including probabilistic laws, and consequently including chances.[1] The laws of nature are those theorems that achieve a best combination of simplicity, strength, and fit in a description of the vast mosaic of local matters of particular, occurrent fact that constitutes the world according to the doctrine of Humean supervenience. Although chances are linked to relative frequencies only in a roundabout way, Lewis's theory of objective probability is a kind of frequency analysis. Chances supervene on what actually happens, on events that can ex post, once the probabilistic laws according to the best-system analysis are in place, be classified as outcomes of random experiments.

The main problem of any kind of frequency analysis of probability is that it assumes non-probabilistic, "absolute" connections between probabilities and relative frequencies, and views the former as being supervenient on the latter.[2] This does not fit to the notion of a chance or random experiment, which, if repeated, should be able to yield each possible outcome with any relative frequency in the short or long run. If you throw a die once, any number may turn up, if you throw it two times, any combination of two numbers may appear, and if you throw it $n$ times, any outcome series of length $n$ may result, in particular, say, the series consisting of $n$ times "six". If the die together with the throwing procedure is unbiased, all ordered outcome series of length $n$ occur with the same probability $(1/6)^n$. The series consisting of $n$ consecutive sixes has the same probability to appear as any other particular outcome series of length $n$. But as $n$ increases, the *proportion* of outcome series in which each of the numbers 1, 2,…, 6 occurs with a relative frequency of approximately 1/6 grows larger and larger within the totality of all possible series, and approaches 1 as $n$ goes to infinity.

This purely combinatorial fact is generalized by the so-called weak law of large numbers, which explains probabilistically why we observe relative frequencies close to the respective outcome probabilities if the number of repetitions of a random experiment is large and the repetitions are independent. Deviations of arbitrary size are possible, only their probability decreases and approaches 0 as $n$ increases. The transition to countably infinite series of repetitions does not change the situation essentially. It gives rise to the strong law of large numbers which maintains the probabilistic nature of the connection between the probabilities and the limiting relative frequencies. Deviations of any size remain as possible as ever, but taken all together form a set of probability measure 0.

We consider the preconditions of the laws of large numbers to be fulfilled in many cases; it is a natural starting point to model the repeated operation of a random

[1] See Lewis (1980, 1986, 1994); Loewer (2004).

[2] See Hájek (1997); Hájek (2009) for comprehensive lists of objections to frequency analyses.

mechanism by independent, identically distributed random variables. This constitutes a kind of default view, partly because it captures what we *mean* by "repetition of an experiment". It is part of our normal understanding of such a phrase that the relevant circumstances do not change, that each trial can be viewed as if the others were not conducted, that the individual repetitions do not influence one another etc. What the laws of large numbers then imply is what one would expect from the outset: On the one hand, that series of results with extremely "deviant" frequencies remain possible, no matter how often the experiment is repeated, and on the other hand, that all such series taken together become more and more improbable with increasing length, which is sufficient to explain why we do not have to reckon with them.

All this strongly suggests that it should be possible that probabilities and relative frequencies fall radically apart. Frequency interpretations, however, allow for such differences only in a limited sense: According to them, probabilities are ultimately *constituted* by relative frequencies. It is not the chances that underlie the frequencies, but the other way round. This means in particular that emerging relative frequencies cannot be explained via reference to chances, and that the chance of a certain outcome on an individual trial depends, as a matter of principle, counterfactually on the outcomes of other trials. The laws of large numbers, which are part and parcel of the probability calculus, also hold under frequency interpretations, but they receive, so to speak, an unnatural reading. Their supplementation by a principle that goes beyond them so as to yield strict connections between probabilities and relative frequencies is even liable to create tensions.

Take once again Lewis's account. In case one sticks to the independence of repeated trials of typical random experiments, one faces the peculiar phenomenon of "undermining futures". If the chance of getting a "six" upon a normal throw of a symmetric and balanced die is 1/6, if the various throws are probabilistically independent, and if there are many, but only finitely many of them during the history of the world, then there is a positive chance that all or almost all such throws yield "six". The actual occurrence of something like this would, however, imply that the probability of this outcome upon a throw is *not* 1/6, after all, but close or equal to 1, given Lewis's best-system analysis. Thus, the assumptions of independence and of "probability 1/6" together imply definite positive probabilities for certain events the actual occurrence of which is ruled out by these very assumptions.[3] This is certainly unpleasant. The neat solution to the problem is to give up on the possibility that many repeated trials of a random experiment are probabilistically independent in the full sense. They may be nearly so, but gradually cease to be when ever larger numbers of them are taken into account.[4] But it is of course also unpleasant to view typical repetitions of typical random experiments as probabilistically dependent on purely conceptual grounds. Their dependence or independence should be an empirical question, to be detected by specially designed statistical tests. [5]

[3] This peculiarity has fueled a debate of its own, starting with Lewis (1986, 1994); Hall (1994) and Thau (1994).

[4] See also Arntzenius and Hall (2003, sect. 5).

[5] As a reviewer pointed out to me, the criticisms of this paragraph cannot be carried over to other varieties of the frequency theory. Richard von Mises's account (1928, 1957) in particular, which has arguably

Frequency analyses confound assertibility conditions for probability statements with truth conditions. Frequencies constitute evidence for objective probabilities. The probabilities are derived from them by point or interval estimation and hypothesis testing as practiced in mathematical statistics. Conversely, relative frequencies are predicted or explained by reference to objective probabilities. All these connections, captured by the laws of large numbers, are probabilistic in themselves, and cannot therefore be used to interpret probability statements.[6] Nor will it do to distinguish first-order and second-order probabilities and to analyze only the first-order probabilities via reference to relative frequencies. Apart from the obvious fact that also the second-order probabilities are in need of an interpretation, the appearance of different types of probability in the laws of large numbers is misleading. If one looks not only at the theorems, but also at their proofs, it becomes obvious that the "second-order" probabilities emerge from simple combinations of "first-order" ones. There is no reason why they should receive a different interpretation.

Thus, the best strategy for frequency accounts is to reject the idea of truth conditions for probability statements altogether. Perhaps there are no such conditions, and all one can say about the meaning of statements of probability is to refer to assertibility conditions, i.e., to give a detailed description of what counts as evidence for or against such statements, to describe under which conditions we would be inclined to uphold or withdraw them, under which circumstances we would say that the objective probabilities have changed etc. There is no doubt that the answer to questions of *this* type is primarily to be given in terms of relative frequencies. If one sticks to assertibility conditions, the meaning of "probability" may be fully captured by the statistical methods and practices alluded to.

In case we insist on an answer to the question what feature of reality is picked out by purportedly objective ascriptions of probability, what in the world makes them true or false, the main alternative to relative frequencies are *propensities*. The term and the associated interpretation of probability were introduced by Karl Popper and subsequently discussed and explicated by many writers. As "propensity" is even more in need of an interpretation than "probability", the introduction of this concept does not achieve anything by itself, and one could as well do without it. If propensities are analyzed via reference to relative frequencies, as in the so-called long-run propensity accounts, one is back with frequency theories of probability—in fact, one has never abandoned them. The same is true if propensities connect to frequencies in a more roundabout way, like in Lewis's best-system analysis. Certainly, some of the problems of standard frequency analyses can be solved by these varieties, but not the main ones sketched above.

This is not to say that Lewis's "Principal Principle" by itself somehow leads to a kind of frequency theory of objective probability, but it does so combined with

Footnote 5 continued
been the most influential one for several decades, is not liable to them. This is possible because it relies on the demanding notion of a collective. See Hájek (2009) for a detailed survey of the problems of such hypothetical infinite frequency accounts.

6 That frequency approaches to probability confuse evidence with what it is evidence for was already put forward by Kneale (1949, Sect. 33).

his commitment to Humean supervenience. To make a real departure from frequency accounts, the propensity theory has to insist that objective probabilities are irreducible, primitive non-Humean entities that attach to single operations of random mechanisms and give us the appropriate degrees of belief for the various possible outcomes. Propensities are supposed to be objective probabilities which belong to the fundamental ontological equipment of the world and therefore cannot be analyzed any further. Essentially, the propensity theory in this restricted sense amounts to the claim that objective probabilities are entities in nature about which little can be said except that they constrain degrees of belief. There are genuine open possibilities in nature for the further course of events at a certain point in time—i.e., determinism is wrong,—but these open paths may have different "weights", occur with different objective probabilities, which in turn means that certain *subjective* probabilities, certain degrees of belief, are the appropriate ones.

This connection to degrees of belief is basically everything we are left with regarding propensities or objective probabilities according to a single-case propensity theory. To be sure, *any* concept of objective probability must exhibit a connection to adequate degrees of belief—an entity in nature would not deserve the label "probability" without it. Most importantly, one ought to reckon with what is (judged to be) objectively highly probable, at least under normal circumstances. Consequently, any conception of objective probability has to assume a principle that links the entities that are to play the role of objective probabilities to appropriate degrees of belief. The exact form of such a principle is highly contentious and depends on the respective conception. David Lewis and, in a less transparent way, Hugh Mellor were the first to highlight the importance of such a principle, and Lewis even claimed that everything one can say about objective probabilities is captured by and implicitly contained in it.[7] According to propensity theories in the narrow sense of the word, objective probabilities are fundamental entities in nature that constrain rational credence "just so". They have a normative power that cannot be explicated further. Every attempt to do so by reference to relative frequencies leads back to a frequency theory of probability.

Thus, we are left with a principle of the indicated kind and the claim that there are entities that conform to it. The question *why* our degrees of belief should respect these entities, where their normative power comes from, how they achieve it to make certain degrees of belief "right" or "appropriate", is unanswerable—the entities in question are simply *introduced* this way. One can, to be sure, talk about "dispositions of certain strengths", "graduated possibilities", "weights of possibilities", "tendencies", even (as Popper also did) "generalized forces", and, well, "propensities", but all this adds nothing substantial.[8] As we have no clue what these expressions might mean unless we are informed that they are supposed to be or to ground objective *probabilities*, they cannot provide an independent basis for the latter concept. Not surprisingly, the propensity theory of probability is confined to philosophical discussions and rarely mentioned by, e.g., physicists. Scientists who apply the probability calculus in their everyday work are inclined to frequency accounts, if they pose interpretational questions at all. This

[7] Lewis (1980, 1986); Mellor (1971, 1995, ch. 2–4).

[8] See Popper (1959, 1990).

does not mean that some variety of the frequency theory is right, after all, but only that users of probability hardly distinguish between assertibility conditions and truth conditions.[9]

What one would like to have is an interpretation of probability that serves to explain the typical relative frequencies that emerge in the long run upon typical repetitions of random experiments. As an alternative to the characterization of objective probabilities as those features of the world that make certain degrees of belief appropriate, one could introduce objective probabilities or chances as the very features of experimental set-ups that explain, using the laws of large numbers, the characteristic frequencies with which the various possible outcomes occur. "Propensity", too, could be understood not as a label for rather mysterious fundamental normative entities, but as an umbrella term for whatever features of experimental set-ups are suitable to play the role of objective probability in this sense. But then, again, the term "propensity" is superfluous, being a mere synonym for "objective probability". It would have to be cashed out, perhaps differently in different contexts. What could those features be? Again, we are looking for the "chancemakers".

## 2 The Range Conception

If we look at the classical applications of the probability calculus, namely, to games of chance, an idea as to what the "chancemakers" could be readily suggests itself. Such a game consists of a physical mechanism that generates a certain outcome from a range of possible outcomes in an unpredictable way. The unpredictability derives from the instability of the mechanism: The result in a given case depends on the exact circumstances that obtain, and even small variations of those can lead to a different outcome. Thus, any attempt on our part to control or predict the result is in vain. The mechanism may therefore be called a random mechanism. On the other hand, there are physical symmetries that are supposed to guarantee the fairness of the game: we use decks of equally sized cards, urns with balls of equal size and weight, balanced roulette wheels with slots of uniform size, balanced dice etc. Consequently, although the mechanism operates in an unstable way so that we are de facto unable to predict or control the result, at least we are entitled to ascribe equal probabilities to the different possible outcomes. These ascriptions are true because of the obtaining physical symmetries. So, a natural idea would be that in a classical game of chance the "chancemakers" consist in the instability plus the physical symmetries of the mechanism.

This idea bears a certain similarity to the classical conception of probability according to which the probability of an event is the ratio of the number of "favourable cases" to the number of "equipossible cases". The similarity is no accident, of course, because the classical view is linked to the emergence of the probability calculus from the 17th century onwards, which was developed precisely to deal with games of chance. There is, however, an important difference. The fundamental "equipossible cases" the classical conception invokes to derive numerical ascriptions of probability are

[9] See Eagle (2004) for a comprehensive survey of further criticisms of propensity theories, and Suppes (2010) for an answer to the question why so much in-depth research can be conducted concerning various applications of the probability calculus without taking a stance on interpretational issues.

judged to be "equipossible" by the so-called "principle of indifference" or "principle of insufficient reason" which is an *epistemic* principle. Consequently, classical probabilities are epistemic probabilities, whereas the above-mentioned idea attempts to derive probabilities from physical symmetries which are a fully objective feature of the underlying mechanism. Not because we are ignorant do we attach equal probabilities to the different possible outcomes in a typical game of chance, but because we know or assume that the pertinent symmetries obtain and the mechanism is "fair" in this sense. A statement of probability is true or false in virtue of these symmetries, i.e., in virtue of an objective and contingent feature of the world. Thus, the respective probabilities are neither epistemic nor, for that matter, logical probabilities. The residual element of ignorance that is involved here concerns the fact that the mechanism appears "unstable" or "random" *to us*, which undoubtedly has to do with our epistemic capacities and cannot be called an objective matter. But this has nothing to do with the numerical values of the chances. These come from the symmetries, which are, to repeat, a purely physical matter.

But what if the mechanism displays no symmetries, but is biased in favour of certain possible results? Intuitively, it should be as easy to ascribe objective probabilities to an unfair game of chance as to a fair one, and equally intuitively, it should be the physical properties of the underlying random mechanism that make such ascriptions true or false. Thus, it will not do to straightforwardly take physical symmetries as chancemakers. It is not too difficult, however, to find an alternative way to characterize and ground the chances. If we take games of chance to be in principle describable by classical mechanics, the result of an operation of the random device is determined by the particular initial conditions that happen to obtain on this trial. We can represent each possible constellation of initial conditions by a vector of real numbers, the components of which correspond to the different physical parameters that are relevant to the result. Call such a vector an "initial state". In this manner, we can attach to each random experiment—"experiment" taken in the 'type'-sense—with a deterministic dynamics an initial-state space, which can be viewed as a subspace of the $n$-dimensional real vector space (for some $n$). Each point in the initial-state space uniquely fixes a result of the experiment. Now, if we could survey this space, we would find on the one hand that in the neighbourhood of each point leading to a certain result, there are points associated with other results, so that slight changes in the initial conditions may alter the outcome of the experiment. This is the reason why the experiment appears random to us. On the other hand, the different possible outcomes of the experiment are represented within each not too small subregion of the state space with approximately constant proportions. These proportions may be different for different outcomes, but they do not depend on where we are within the space. They explain the typical relative frequencies with which the various possible outcomes occur upon repetition of the experiment.

So, the basic idea to get chancemakers is to survey the initial-state space of a random experiment. "Experiment" is to be taken in the 'type'-sense, and, moreover, in a wide sense that does not presuppose the literal presence of experimenters. We leave the restricted domain of games of chance behind; any repeatable deterministic process will do. Assuming that the initial-state space has a structure of the aforementioned kind, we take the respective proportions of the different outcomes as their probabilities.

We may speculate that if a deterministic process can successfully be described by probabilities with an objectivist flavour, this is due to such an initial-state space. To repeat: On the one hand, in each (not too) small vicinity of an initial state leading to a given outcome $A$ there are initial states leading to other outcomes, which explains why the result of (a single instantiation of) the process cannot be controlled or predicted. On the other hand, for each outcome, the proportion of initial states leading to it is the same in any not-too-small segment of the space, which explains why there are certain stable characteristic relative frequencies with which the different outcomes appear upon repetition. If the proportion of an outcome $A$ differed in different subregions of the space, we would be able to influence in a systematic way the frequency with which $A$ occurred, in which case we would not be able to assign a definite probability to $A$. While we cannot use the frequencies themselves to define the probabilities, we can use the features of the underlying initial-state space to do so. Its structure gives us the chancemakers.

(RC) Let $\boldsymbol{E}$ be a random experiment and $A$ a possible outcome of it. Let $\boldsymbol{S}$ be the initial-state space attached to $\boldsymbol{E}$, and $\boldsymbol{S}_A$ be the set of those initial states leading to $A$. We assume that $\boldsymbol{S}$ and $\boldsymbol{S}_A$ are measurable subsets of the $n$-dimensional real vector space $\mathbf{R}^n$ (for some $n$). Let $\mu$ be the standard (Lebesgue-) measure. If there is a number $p$ such that for each not-too-small $n$-dimensional (equilateral) interval $\boldsymbol{I}$ in $\boldsymbol{S}$, we have

$$\frac{\mu\,(\mathbf{I} \cap \mathbf{S}_A)}{\mu(\mathbf{I})} \approx p$$

then there is an objective probability of $A$ upon a trial of $\boldsymbol{E}$, and its value is $p$.

As usual in measure theory, we lay down the condition for intervals. (The somewhat annoying qualification "equilateral" can be dropped, but to the effect that the resulting notion of probability no longer applies in certain cases where it intuitively should.) The condition then trivially carries over to unions of finitely or denumerable infinitely many pairwise disjoint not-too-small (equilateral) intervals, and we may assume that any not-too-small subregion (any not-too-small bounded and connected measurable subset) of $\boldsymbol{S}$ of reasonably "well-behaved" shape can be approximated by such unions. Thus, (RC) states that the proportion of $A$ in every subregion of $\boldsymbol{S}$ of "ordinary shape", given that it is not extremely small, roughly equals $p$. Given the close connection between measure and integration, we can express (RC) also in terms of integrals:

(AF) Let $\boldsymbol{E}$ be a random experiment and $A$ a possible outcome of it. Let $\boldsymbol{S}$ be the initial-state space attached to $\boldsymbol{E}$, and $\boldsymbol{S}_A$ be the set of those initial states leading to $A$. We assume that $\boldsymbol{S}$ and $\boldsymbol{S}_A$ are measurable subsets of the $n$-dimensional real vector space $\mathbf{R}^n$ (for some $n$). If there is a number $p$ such that for any real-valued density function $\delta$ on $\boldsymbol{S}$ that is approximately constant on (equilateral) intervals up to a certain appropriate size $k$, we have

$$\int_{\boldsymbol{S}_A} \delta(\mathbf{x})\mathrm{d}\mathbf{x} \approx p$$

then there is an objective probability of $A$ upon a trial of $\boldsymbol{E}$, and its value is $p$.

I would like to call this the "natural-range conception of probability", for short: "range conception" (RC). According to it, probabilities are given by ranges in initial-state spaces. Its second formulation uses "arbitrary functions" over the initial-state space and is therefore abbreviated by (AF). Several remarks are in order now.

## 3 Remarks and Specifications

(A) Obviously, (RC) and (AF) are vague in several respects. There is talk about "small" and "not too small" intervals, about density functions that are "approximately constant" on intervals "up to a certain appropriate size", about subregions of "reasonably well-behaved" or "ordinary" shape, and last but not least, the main equation holds only approximately. One could be tempted to overcome these somewhat unpleasant features by transition to limiting cases, in which the equation holds *exactly* for *every* interval and *any* continuous density, and define objective probability via reference to such ideal cases. Of course, one would have to add that these ideal circumstances are never fully instantiated in reality. Unfortunately, however, there are no such cases even in theory: The supposed "ideal case" is in fact a non-case. According to Lebesgue's density theorem, since $\boldsymbol{S}_A$ is measurable, for almost every point $x$ in $\mathbf{R}^n$ the proportion of initial states leading to the outcome $A$ in an $\varepsilon$-neighbourhood of $x$ converges either to 0 or to 1 as $\varepsilon$ goes to 0. Thus, we always have plenty of (super-small) intervals in $\boldsymbol{S}$ in which the proportion of initial states leading to $A$ is not even close to $p$, but close to either 0 or 1.[10] Anyway, as $\boldsymbol{S}_A$ is a measurable set which contains only initial states leading to $A$, it cannot be that in every interval the proportion of such states equals $p$, for that feature would straightforwardly carry over to arbitrary measurable sets. From physical considerations it is often plausible to assume that an initial-state space consists of small "patches" as maximally connected sets of initial states giving rise to one and the same outcome, and that these patches are the smaller the more unstable the underlying mechanism is. But it is erroneous to conclude from this that there is a limiting case, no longer physically realistic but mathematically representable, in which perfect randomness as well as a perfectly homogeneous distribution of the initial states all over the initial-state space obtains. Thus, the mentioned vagueness cannot be removed.

(B) (RC) and (AF) are supposed to yield an objective interpretation of probability in the sense of providing truth conditions for probability statements that are independent of our state of mind and our state of information. The main problem with this claim I defer to the next section. Intuitively speaking, the structure of the initial-state space associated with a given experiment (in the type-sense), the distribution of the different

[10] Thanks to Teddy Seidenfeld for pointing this out and for drawing my attention to the Lebesgue density theorem.

possible outcomes all over the space, is clearly an objective matter. The probabilities according to (RC) and (AF) are, however, not single-case probabilities. They may provide us with appropriate degrees of belief even in single cases, to be sure, but in themselves they attach to a *type* of process rather than to its individual realizations, because in the single case there simply is no initial-state space to be had (only, so to speak, an initial state). In a deterministic setting, on a particular realization only one outcome is objectively possible. Owing to our limited capacities of measurement and control, several outcomes *seem* possible to us, and it will in general be the case that we can do no better than to take the respective proportions within the initial-state space as guidance, but that does not mean that (RC) provides us with objective single-case probabilities. The whole space, and consequently the proportions of the various possible outcomes in it, is something associated with a type of process, and thus presupposes a notion of repetition. This should not come as a surprise. The range probabilities constitute a variety of "deterministic chance",[11] and it is natural to assume that in a deterministic context, insofar probabilities can rightly be called objective, they do not attach to single cases, and conversely, insofar they attach to single cases, they are epistemic in character.

(C) This has the further consequence that there is no reference class problem for range probabilities as such. The problem may arise, however, in connection with the epistemic probabilities derived from them, insofar a single case can be viewed as instantiating several different random experiments with correspondingly different initial-state spaces. I am not clear whether this could turn out to be a real difficulty as far as, say, games of chance are concerned, but there may be applications of (RC) that are more problematic in this respect, e.g., to biological or social contexts.[12] I have little to say about this, but take comfort in the fact that "the reference class problem is your problem too".[13]

Whether the objective probabilities according to the range conception should actually guide our degrees of belief on a particular occasion depends on the epistemic capacities and the powers of control we possess or are willing to exercise. These capacities determine how unstable the mechanism must be, and for intervals down to which size the condition (RC) must hold, in order to sensibly equate the epistemic probabilities attached to single trials with the respective proportions within the initial-state space. The proportions cannot be supposed to yield appropriate degrees of belief under the assumption that we know too much about the obtaining circumstances, let alone everything about the past. Thus, the range probabilities are not chances in the sense of David Lewis. They do not obey the "Principal Principle" insofar this implies that information about the past is generally admissible. They do, however, obey a similar principle, with the admissible information further constrained to the effect that only sufficiently coarse-grained descriptions of past events are admissible.

(D) As indicated, the range conception is supposed to deliver a concept of probability according to which the probabilities are those objective features of chance set-ups

[11] About which there is a vivid ongoing discussion. See Loewer (2001); Hoefer (2007); Schaffer (2007); Glynn (2010); Frigg and Hoefer (2010).

[12] Strevens (2003) envisages such applications (ch. 4.9 and 5.4).

[13] See Hájek (2007).

that *explain* why we cannot predict the outcomes of single trials, on the one hand, and the characteristic relative frequencies with which the different outcomes occur upon large numbers of repetitions, on the other hand. Of course, the explanation of the long-run relative frequencies is a probabilistic one. The second-order probabilities that appear when series of trials are viewed as a new type of random experiment, consisting of $k$ repetitions of $\boldsymbol{E}$, are also interpreted according to (RC) and (AF). Call this new experiment $\boldsymbol{E}^k$. An initial state of $\boldsymbol{E}^k$ has no longer $n$ components, but $k \cdot n$, and consequently the initial-state space associated with it is the $k$-fold Cartesian product $\boldsymbol{S}^k$ of $\boldsymbol{S}$ with itself. Intuitively speaking, however, the proportions with which the different possible outcomes of $\boldsymbol{E}^k$, that is, the different possible series of length $k$ of outcomes of $\boldsymbol{E}$, are represented within not-too-small intervals of $\boldsymbol{S}^k$ can only be equated with their probabilities if the repetitions of $\boldsymbol{E}$ within $\boldsymbol{E}^k$ are independent. If they are not, while $\boldsymbol{S}^k$ can still be viewed as the initial-state space of our new, complex experiment $\boldsymbol{E}^k$, this space does not "get the probabilities right". This highlights the fundamental problem of the range conception of probability, to be discussed in the next section.

(E) The range conception views objective probability as a high-level phenomenon that arises in deterministic contexts which are structured in a particular way. Moreover, it reduces probability to the notion of distance between initial state vectors. Again, further discussion of this point is deferred to the next section. With these features, (RC) and (AF) are in sharp contrast to propensity interpretations in the narrower sense of the word. Propensities are linked to indeterminism, and they are low-level, fundamental entities in nature. It may be that we need both concepts of objective probability. On the one hand, the range conception cannot on its own handle cases of genuine indeterminism, while on the other hand it is implausible to suppose that every context that calls for an objective concept of probability implies genuine indeterminism and must somehow be linked to quantum physics. The often-noted paradigm examples in this respect are statistical mechanics and the above-mentioned games of chance. Of course, it is contentious if probabilities in such contexts are really objective, but the stark contrast between subjective (or epistemic) and objective (or ontic) probabilities seems to me to be a kind of red herring. There can be little doubt that the probabilities in classical statistical physics are objective in *some* sense, which is not to deny that there is also an epistemic aspect to them. Both aspects are captured by the range conception.

(F) It is important to emphasize that (RC) and (AF) are assumed to provide truth conditions for probability statements and to explain the typical patterns that emerge upon the repetition of random experiments, but that the conditions mentioned are typically not of a kind we could directly use to ascertain the probabilities in the first place. Assertibility conditions for objective probability statements are on all accounts, and quite apart from any interpretational questions, frequency data or, in important special cases, symmetry considerations. That objective probability in a deterministic context can always be traced back to an initial-state space of the indicated kind is a plausible supposition, but to actually construct this space, survey it, and derive the outcome probabilities from it, is a task that can only be accomplished in special cases. Engel (1992) gives an up-to-date treatment of such cases and the requisite mathematics, which is by no means trivial. The situation is the same as in the empirical sciences generally: Complex cases can theoretically be treated only "in principle". Strevens (2003) tries

to substantiate the claim that (RC) and (AF) yield the background for objective probabilities not only in physics, but also in biology and the social sciences, and so the conception may rightly be dubbed "natural-range conception" of probability.

(G) The ideas presented here essentially appear for the first time in Johannes von Kries's treatise "Die Principien der Wahrscheinlichkeits-Rechnung" (The principles of the probability calculus) from 1886. The book was quite influential at its time as witnessed by a second edition in 1927. Von Kries's treatment is, partly due to the lack of appropriate mathematical tools by the end of the 19th century, entirely informal. He speaks about the "Spielraum" (play, leeway, range) of initial conditions, but also about continuous probability assignments, which correspond to what was later called the "method of arbitrary functions". Von Kries is already well aware of the tension between subjective and objective interpretations of probability, and tries to reconcile them by his idea of "ranges". On the one hand, any ascription of probability mirrors a limitation in epistemic capacities (subjective aspect), whereas on the other hand, the ranges are a matter of actually obtaining circumstances and not of our opinion (objective aspect). The first rigorously treated example is due to Poincaré (1896), namely, the so-called wheel of fortune: a rotating disc divided into alternating red and black segments of equal size eventually comes to rest, and yields the outcome "red" or "black" according to a fixed pointer outside. This case, which appears in similar form as "Stoss-Spiel" (push-game) in von Kries, has served as a standard example ever since, because the initial-state space is only one-dimensional. If the initial position of the wheel is fixed, its final position, and thus the outcome "red" or "black", depends on the initial angular velocity only. Poincaré is also the first one to use the term "arbitrary function". von Smoluchowski (1918) expresses similar ideas about probability. They can also be found in Reichenbach (1935). Hopf (1934, 1936) gives the first systematic treatment of the method of arbitrary functions and constructs some more demanding examples in which the probabilities are actually derived via the method and not known beforehand from symmetry considerations. von Plato (1982, 1983) rediscovers the systematic importance of the subject and gives concise historical overviews (1983; 1994, ch. 5). The most comprehensive up-to-date treatment of the subject matter is given by Strevens (2003, see also his 2008, part 4).

(H) In the foregoing paragraph I skipped several differences between the mentioned writers. In particular, they differ on what exactly is achieved by considerations concerning the initial-state space of a deterministic process that gives rise to ascriptions of probability. A modest claim would be that the structure of the space *explains* (probabilistically) why we sometimes apply the calculus of probability successfully in such contexts. (RC) and (AF) then explain how it can seem that there are objective single-case probabilities "out there in nature", while in fact there are none. It need not be claimed that *all* successful applications of the probability calculus to empirical phenomena have such a background, nor, more importantly, that (RC) and (AF) yield an *interpretation* of probability. That they can be viewed in this way, i.e., as providing truth conditions for probability statements, is my main concern here (cf. Rosenthal 2004, ch. 3, and 2010). There are, however, good reasons to be cautious in this respect, which we will encounter in the next section. Most of the mentioned early writers seem to adhere to frequency views of probability. Strevens (2008) also favours a frequency account (sect. 10.26), whereas in (2003) he deliberately remains neutral

towards interpretational questions (sect. 1.32, 1.33, 2.14, 2.3). There he is primarily interested in the "physics", not the "metaphysics", of probability. In this view, conditions like (RC) and (AF) do not settle anything concerning the question what probability statements are actually about, but rather notice a possible source of probabilistic phenomena that can be fully acknowledged no matter how one interprets "probability".

In contrast to this, there can be little doubt that von Kries had something like (RC) in mind as a proposal for the "metaphysics" of probability. Anyway, once you recognize conditions of this kind as a possible origin of probabilistic phenomena, the idea to use them also as an interpretation of probability is fairly straightforward. Recently, several writers have spelled out this idea in interestingly different ways, usually referring to Poincaré and Hopf. Marshall Abrams's "mechanistic probability" (2010) and Michael Strevens's "microconstant probability" (2011) maintain more or less close connections to actual relative frequency views, while Myrvold (2011) takes rational credence as starting point to develop a notion of "epistemic chance". I will address these accounts later, if only in a sketchy manner that cannot do them full justice. For the time being, I elaborate on the core idea, which is to take just the ranges within the initial-state spaces as truthmakers of probability statements, without additional elements. I thereby try to stay away from the standard interpretations of probability, maintaining, so to speak, equal distance to all of them.

## 4 The Main Problem of the Range Conception

Why should we take the proportion with which the outcome $A$ is represented within (not extremely small) intervals or within (not extremely small) ordinarily shaped subregions of $\boldsymbol{S}$ as the probability of $A$? "Nature's choice" of initial conditions need not follow anything like a uniform distribution over the initial-state space, so that if there were significantly different proportions of $A$ in different subregions of the space, we would not be entitled to attach a definite probability to $A$. But if, on the contrary, $A$ is represented with roughly the same proportion all over the initial-state space, we should expect that upon repetition $A$ occurs very likely with a relative frequency close to $p$. Thus, we have identified an objective feature of the set-up that underlies the unpredictability of the outcome in the single case as well as the observed relative frequency of $A$ in the long run, and this clearly seems to license a degree of belief of $p$ that $A$ occurs upon a trial of $\boldsymbol{E}$. What more could be expected from a thing in order to call it "objective probability"?

The first and obvious difficulty is that nature could choose initial states in accordance with an unusual, very eccentric distribution. If we recall the picture of the initial-state space consisting of "patches", a patch being a connected set of maximum size of initial states leading to one and the same outcome, it is clearly possible that upon repetition of the experiment a distribution emerges with an extreme peak on one patch, or a distribution that is periodic with a period length of the size of the patches. I skip the delicate question what it exactly means that upon repetition of $\boldsymbol{E}$ a certain distribution "emerges" over the state space, or what it means that nature chooses initial states "in accordance with" a particular distribution. It is common scientific practice to use continuous functions to represent finite numbers of measurement results. In our

case a continuous distribution is used to capture in an idealized way the initial states that occur as a matter of fact in finite numbers of trials. Whatever problems this step may pose as a matter of principle, they do not concern us here. An eccentric distribution of one of the indicated kinds (which constitute the most obvious possibilities for "problematic" distributions) would put greater weight on certain outcomes than is to be expected by the structure of the initial-state space, and if we relied on it in ascertaining the outcome probabilities, they would deviate from what (RC) and (AF) prescribe.

We would, to be sure, not necessarily give credit to such a distribution, nor should we. It could as well come about "by accident". But it is certainly conceivable that an eccentric distribution would appear and prove to be stable, so that we, perhaps after considerable resistance, would accept it as yielding the objective probabilities of the outcomes and relied on it for further predictions. That the "true" distribution (if suchlike exists) over initial states is eccentric in one of the indicated ways is all the more a clear possibility as we do not mean to discuss epistemological questions here, but the metaphysics of probability. The mentioned possibility deepens a worry that could have been present from the very beginning, namely, that (RC) and (AF) merely describe certain interesting cases of the transformation of probability distributions, but for this very reason cannot possibly be used for an analysis of probability. Given an initial-state space, and given a probability distribution on it, the outcome probabilities are fixed, but interpretational tasks of any sort are simply pushed back from the latter to the former probabilities. The space may have a structure that implies that very different probability distributions on it lead to the same outcome probabilities, but the situation is still "probabilities in—probabilities out". This is obvious with (AF), where density functions on the initial-state space are explicitly considered, but (RC) is equivalent to (AF), after all. Given this picture of (RC) and (AF), Strevens (2003) seems to be exactly right: They concern the "physics", not the "metaphysics", of probability.

This objection is undoubtedly strong, but it underestimates the achievements of (RC) and (AF). Their point is precisely that under certain conditions the outcome probabilities do *not* depend on probability distributions on the initial-state space. This space has a structure that implies that *no matter* which distribution we choose, the outcome probabilities are not affected. And why should we even suppose that there is such a thing as a probability distribution over the space according to which nature selects initial states? We need not. It is the space itself and its structure that determines the outcome probabilities. Consequently, in (RC) there is no talk about density functions or distributions over the initial-state space. This is, of course, only the governing idea. A closer look at the matter has already revealed that the independence from probability distributions over the initial-state space is not *that* far-reaching, but holds just for "well-behaved", "reasonable" distributions. The point stands: Given a suitably eccentric distribution, (RC) and (AF) get the outcome probabilities wrong, and this shows that the claim that (RC) and (AF) provide truth conditions for probability statements is doubtful at least.

So, we somehow have to deal with these cases of deviance. A first idea would be this: If there really was a "true" eccentric distribution over the initial-state space, i.e., one that does not come about merely by accident, one that ought to be relied on for predictions etc., we would certainly assume that there is an explanation for this

peculiar phenomenon. We would not believe that nature in its selection of initial states followed such a distribution just out of the blue. The tacit background assumption is the following: Nature fixes the result of a particular instantiation through its laws, given the initial state, but it does not care for the initial states themselves, while we cannot control them sufficiently. Thus, we expect either no genuine distribution over initial states at all, or an "ordinary" one, like a reasonably flat $n$-dimensional normal distribution. But if an eccentric distribution is the true one instead, then nature *does* care for the choice of initial states, contrary to what we thought. This means that we must have overlooked a nomological factor relevant to the appearance of initial states and thus (indirectly) for the experimental outcomes. If we step further back and look how the initial states themselves come about, we should be able to discover this additional factor and to re-model the experimental situation, this time explicitly paying attention to the neglected nomological influence. If the new initial-state space still did not get the outcome probabilities right when (RC) and (AF) are applied to it, i.e., if there was also a genuine stable eccentric distribution of initial states with respect to the new space, we would re-apply our consideration, and at some point, when we had taken all nomological factors relevant to the experimental result into account, we would finally arrive at a space in application to which (RC) and (AF) yield the correct outcome probabilities.

How do we know this? We know it in the peculiar sense already indicated: Reliably eccentric distributions call for nomological explanations that in turn give rise to a re-description of the experimental situation. This is already basically von Kries's line of reasoning (1886, ch. II and VII). It makes the range conception of probability to a certain extent un-empirical (which is noted by von Kries: p. 170). Any attempt to refute the claim that objective probabilities are grounded in initial-state spaces can supposedly be countered by transition to what von Kries calls a "primordial" space (1886, ch. II).

There are two problems with this idea. First, it adds an a-priori-flavour to the conception. It assumes that we always can and should draw the line between initial conditions and laws of nature in a way that enables us to uphold the range interpretation. This is rather committing, and the consequences of this move are difficult to assess. Second, it may be unpleasant to have to dislocate what counts as the initiation of the experiment in order to save the range conception. The "primordial" initial-state space need not be an intuitive choice, i.e., no longer comprise what one would naturally call the initial states of the experiment. What are, e.g., the initial conditions of a throw of a die? Usually we would look at the parameters at the moment when the die leaves the gambler's hand. It would be quite embarrassing to have to switch, e.g., to a description of the whole body of the gambler including its close environment at the moment when he grabs the die. It is clearly conceivable that nature's choice of what we are inclined to take as initial states of the experiment is governed by a genuine, stable and eccentric probability distribution, and even if evasion by transition to another state space should always be possible, the conditions of application of (RC) and (AF) are blurred in this way.

Of course, the possibility to dislocate what counts as the start of the experiment, with the consequence of switching to another initial-state space, has been there from the very beginning. As the range conception is compatible with a thoroughly deterministic

outlook, in which not only the initial states fix the result, but are themselves fixed by former conditions, we have to reckon with the fact that our choice of an initial-state space depends on a conventional cut in time that could also be placed earlier or later. In many cases, this does not affect the resulting outcome probabilities, i.e., it does not matter to which of all these successive spaces (RC) is applied. What the objection rightly highlights is that it may very well matter in extraordinary cases, and thus the range conception is not as straightforwardly applicable as one might hope. It is no longer proportions in suitably structured initial-state spaces that yield objective probabilities, but proportions in *primordial* spaces, and it is unclear what these are and how to detect them.

An alternative way out would be to enrich the truth conditions for probability statements. It is no longer the structure of the initial-state space by itself that makes such a statement true or false, but this structure *plus* certain facts that guarantee, or *tend* to guarantee, the non-eccentricity of the distribution over the space. This strategy is employed in Strevens (2011). As with the foregoing idea, there are two problems with this one. First, the explicit talk about distributions over the initial-state space calls for an interpretation of these, and thus threatens to render the range conception worthless as an interpretation of probability. There is, to be sure, always the possibility to express (RC) in terms of density functions. But we *need not* talk about such densities or distributions on the initial-state space, it is just an option. With the idea in question it ceases to be a mere option and becomes a central element of the range conception. For Strevens, the distributions represent actual occurrences of initial states. Nevertheless, he allows for arbitrarily big differences between probabilities and relative frequencies of outcomes. It is not the outcome frequencies or the frequency distribution over the initial-state space that grounds the probabilities, but rather the facts that *tend to guarantee* the smoothness of this distribution together with the structure of the space. By this major move Strevens evades the above-mentioned central shortcomings of actual frequentism, but only to make the following problem worse.

Second, the governing idea behind the range approach has been to identify the features of chance set-ups that explain probabilistically the typical outcome patterns that emerge when random experiments are repeated, and to call these features "probabilities". We could also use "propensity" as an umbrella term for them, but a propensity interpretation that did no more than this would merely be a promissory note. The range conception claimed to cash it out in a deterministic context, i.e. to identify the features in question provided that outcomes are determined by initial states. But now, in addition to the structure of the initial-state space, there are further relevant explanatory facts, namely those that (tend to) guarantee the non-eccentricity of whatever distribution emerges over the state space upon repetition of the experiment. What are those facts, and how do they do their work? Strevens (2011) refers to frequency distributions in close possible worlds, where slightly different series of random experiments are conducted. He is clear about the significance of this "counterfactual robustness" of the emerging frequentist probabilities, which is not captured by usual brands of frequentism. But what makes it the case that only "very few" close possible worlds display wildly deviant outcome frequencies? Being confronted with the task of providing and justifying a measure over a certain set of possible worlds at this stage means that one has not succeeded in identifying the "chancemakers".

A potentially relevant observation in this context is that the eccentric distributions that considerably influence the probabilities are very sensitive towards disturbances. Not every distribution that is somehow peculiar or unexpected implies a change in the probabilities—intuitively speaking, most of them do not. The distribution has to be eccentric in a very special way, e.g., to put considerable weight on just one "patch", or to be periodic in just the right manner. Even very small changes in such a distribution tend to reinstate the probabilities according to (RC).[14] This confirms the above-mentioned point that a nomological influence is needed to give such a distribution its stability, an influence so far neglected in the description of the experimental situation and therefore calling for a re-modeling. We would not accept a reliable eccentric distribution on the initial-state space without further ado. It is, however, very doubtful whether such disturbance considerations can do any fundamental work. They implicitly rely on a rough idea of distance between distributions or densities on the initial-state space that can itself be called into question. What is a "small" change in a density function? It should in principle be possible to furnish the space of all possible densities defined on a certain subset of the $\mathbf{R}^n$ with a metric that is eccentric in itself (judged by intuitive standards) so as to render wrong what we tend to think about the effects of "small" alterations in a distribution. The full force of the problem appears in connection with the following more fundamental criticism of the range conception.

(RC) provides truth conditions for probability statements that make essential use of a notion of distance between initial states: In each (not too) *small vicinity* of an initial state leading to a given outcome *A* there are initial states leading to other outcomes, which explains why the result of a single trial cannot be controlled or predicted. Furthermore, for each outcome, the *proportion* of initial states leading to it is roughly the same in any (not too) *small subregion* of the space. Thus, the initial-state space has to come along equipped with a metric and a measure built on it. In this way, probabilistic notions are traced back to topological relations between vectors of physical quantities. This may seem innocent enough, if an initial-state space can be viewed as a suitable part of the $n$-dimensional real vector space $\mathbf{R}^n$, for which there are topologically equivalent standard notions of distance (arising from the so-called $p$-norms) as well as a standard notion of volume (the Lebesgue measure). There are, however, infinitely many possibilities to map physical initial states into the $\mathbf{R}^n$, i.e., infinitely many ways to represent initial states by vectors of real numbers, or, equivalently, infinitely many ways to choose physical quantities that play the role of initial conditions that determine the result of the experiment.

Take, for example, the turning of a wheel of fortune in which the outcome depends on the initial angular velocity. One can transform this quantity in many ways, and some of these yield very different proportions of the outcomes "red" and "black" within the corresponding one-dimensional initial-state space. Initial angular velocity determines the result, well, but so does initial angular* velocity. If angular* velocity is the result of a transformation of angular velocity such that those intervals of angular velocity that lead to the outcome "red" are shrunk and those leading to "black" expanded, or vice versa, one gets arbitrarily deviant proportions of "red" and "black" in the

[14] See Michael Strevens's "perturbation argument" (2003, ch. 2.53).

corresponding initial-state* space. A transformation of this kind can either be framed as the distorted representation of a standard physical quantity or as the standard representation of a distorted quantity. Through such tinkering with physical quantities or their mathematical representation the outcome probabilities according to (RC) can be set to arbitrarily chosen values. Consequently, the objectivity of the range probabilities presupposes that some ways to set up initial conditions are objectively distinguished. Angular* velocity has to be an "unnatural" quantity, or else there is no way to get something like objective probabilities out of the range approach.

Although eccentric transformations of physical quantities or their representation have the same effect on the outcome probabilities as suitably eccentric probability distributions over the initial-state space, they pose a deeper problem. Without an initial-state space with definite proportions of the various outcomes the range conception does not get off the ground. It is one thing how to exclude or how to cope with the possibility that nature follows a definite, stable, eccentric distribution in its choice of initial states, but quite another thing that any distribution whatsoever can appear to be ordinary or eccentric, depending on which physical quantities are chosen to represent the possible initial states of the system in question. The distorted quantities may play no role in physics as it is practised. Physicists usually assume the existence of "natural" metrices and corresponding measures on state spaces, e.g., when they talk about volume in phase space in statistical mechanics. The exact status of such assumptions is, however, very difficult to assess. The difficulty carries over to Strevens's "perturbation argument" and to his attempt to invoke a measure over close possible worlds to ground the probabilities in the actual world. No matter how the actual occurrences of initial states are distributed, it is always possible to choose this measure in a way that the actual frequencies of outcomes appear to be "counterfactually robust" to a very high or to a very low degree, just as one pleases.

In order to make a clean sweep concerning this bunch of problems, it is tempting to explicitly construct input measures from actual occurrences of initial states. This is Abrams (2010) route. With it, the outcome probabilities are ultimately based on actual frequencies which come as a matter of brute fact. These basic frequencies just happen to be the way they are, there is no further explanation for them, no deeper structure to be revealed. But the ranges in initial-state spaces were assumed precisely to provide such an underlying structure that explains (probabilistically, using the laws of large numbers) emerging actual frequencies. If this very structure again rests on actual frequencies, nothing much seems to be gained. Thus, while Strevens's approach fails to fully identify the sought-after "chancemakers", or, for that matter, to justify the required measure over close possible worlds, Abrams's account seems to share the central shortcomings of actual frequentism.

Myrvold (2011) takes a different direction. According to him, the arbitrary functions should be interpreted as reasonable distributions of personal probability, ruling out the eccentric ones as irrational. We have no reason to distribute our credence very unevenly between nearby initial states. It is clear, however, that this presupposes that the problem of eccentric measures on the initial-state space is already solved. The choice of strangely transformed physical quantities in the representation of the experimental situation is ruled out from the start. Taking personal probability as starting point, one could of course simply say that we choose the standard physical quantities

because they suit *us*, but this would completely rid the conception of its supposed objectivity.

Thus, I am reluctant to follow either of these routes. The best course to take would still seem to be a justification of standard units and metrices as "naturally distinguished" in the manner of, e.g., North (2010). She argues that these modes of representation yield the simplest overall formulation of physical theories, and thus the associated measures are objectively distinguished. This would then have to be supplemented by considerations with the mentioned a-priori-flavour to rule out reliably eccentric distributions on the initial-state space. As the idea of a "simplest overall formulation" also figures in Lewis's best-system analysis of laws of nature, there have to be connections between these approaches to objective probability.

I cannot push these matters further here, but have to be content with two concluding remarks. First, due to the structure of the initial-state space in a typical random experiment, a great many modes of representation pose no problem at all. It is only, intuitively speaking, relatively few and quite special distortions that have to be excluded. Unfortunately, it is not clear whether this observation can substantially alleviate the task for a defender of the range conception. Arguments against the mentioned very special distorted quantities would presumably come as arguments in favour of the choice of standard quantities, and thus, if successful, prove much more than is required to make the range conception work. Second, if this problem of the metric cannot be solved, not only the range conception as an objective interpretation of probability is in trouble, but also the *explanation* of frequency patterns via reference to the structure of initial-state spaces. I mentioned above that many writers, for whatever reason, do not go so far as to employ range- or arbitrary-functions-considerations as an interpretation for probability statements, but use them as an explanation of why the calculus of probability is successfully applicable to certain deterministic contexts. Unless we subscribe to a thoroughly subjective metaphysics of explanation, however, this more modest approach also fails to the extent the proportion with which a certain outcome is represented within the initial-state space depends on conventional decisions. Thus, the range- or arbitrary-functions-approach, if it can do any explanatory work, should as well be applicable to provide truth conditions for probability statements, i.e., a "metaphysics" of probability. How exactly this is to be accomplished remains very much an open question, after all.

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