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Johannes von Kries's Range Conception, the Method of Arbitrary Functions, and Related Modern Approaches to Probability

Abstract: A conception of probability that can be traced back to Johannes von Kries is introduced: the "Spielraum" or range conception. Its close connection to the so-called method of arbitrary functions is highlighted. Possible interpretations of it are discussed, and likewise its scope and its relation to certain current interpretations of probability. Taken together, these approaches form a class of interpretations of probability in its own right, but also with its own problems. These, too, are introduced, discussed, and proposals in response to them are surveyed, some of which also go back to von Kries. The structure of the paper is as follows:

i) The range conception introduced

ii) Scope of the approach

iii) Interpreting the range probabilities

iv) Refining the range conception

v) The problem of the measure

Key words: arbitrary functions, chance, probability, ranges, von Kries

i) The range conception introduced

By the end of the 19th and the beginning of the 20th century several writers developed accounts of probability that refer in some way or other to the specific properties of set-ups that allow empirically successful ascriptions of probability. Of course, there are many and varied such phenomena, but one may ask what it is about them that enables us to attach definite and precise numbers to possible outcomes of certain otherwise unpredictable processes and to operate effectively with them when forming expectations. That this should be possible at all is a remarkable fact that calls out for explanation. There must be some underlying objective features of the world captured by those numbers, and the common opinion of such writers as Émile Borel, Eberhard Hopf, Johannes von Kries, Henri Poincaré, Hans Reichenbach, Marian von Smoluchowski, and others (see von Plato 1994, ch. 5) was that an account of probability is incomplete without an investigation into these features, or should even be built around them. Furthermore, they all shared the basic idea about what these features are. There is no established name for this idea, but the most widespread term would presumably be the "method of arbitrary functions". It is to be taken in a wide sense here that admits of many varieties. One could well say that this approach to probability was in the air at that time. The aforementioned writers all explicated it in somewhat different ways, and largely independently from one another. I am going to present it after a few further preliminary remarks, with special emphasis on von Kries (1886; see also 1916, ch. 19, 26). Unless stated otherwise, all references are to von Kries (1886).

The received and still dominating conception of probability at this time was the classical one, culminating in Laplace, according to which probabilities are ascribed to cases using a "principle of indifference" or "principle of insufficient reason". This conception was first and foremost meant to be an epistemic or subjective one, although there had all along been evidence that something is missed in this way. To rightly attach equal probabilities to the possible outcomes of, e.g., a throw of a coin or die, these outcomes must be "equally possible" and that, in turn, is also a matter of the physics of the coin or die and not just of our symmetric ignorance with respect to the outcomes.

With a displaced center of gravity it would just be wrong to ascribe equal probabilities, whether one knows about the bias or not – wrong, that is, if the ascription of probabilities is to have empirical success. This was recognized by several writers in the classical tradition, so the latent idea was that there is also an objective aspect to statements of probability. But what this aspect could be was largely underdeveloped.

It was the advent of statistical mechanics that made fully clear that it would not do to pursue a purely epistemic or subjective interpretation of probability. From this time on the development of a concept of probability that was objective in some clear sense was definitely on the agenda. The general presupposition, however, was still that the world is deterministic, that every event occurs due to sufficient causes that necessarily produce it under the given circumstances, and that the chains of such causes can in principle be followed arbitrarily far into the past. This deterministic outlook had also figured most prominently in Laplace and prompted his conception of probability. The thoroughgoing determinism was not questioned, and statistical mechanics, in particular, was no reason to question it. Rather, the challenge was to provide a sufficiently objective interpretation of probability within a deterministic framework. Pulte (this volume) provides a comprehensive account of the historical background of the von Kriesian ideas.

Before reviewing the work of von Kries and other authors we have to be clear about the relation of their accounts of probability to frequentism. The phenomena that allow for empirically successful applications of the calculus of probability combine unpredictability in the single case with characteristic relative frequencies of outcomes upon repetition. We have a certain type of process the result of which cannot be foreseen in single instances, but repeated operation shows a random sequence of results with the possible outcomes approximately occurring with certain characteristic relative frequencies.

The repetition is mathematically modeled as a sequence of independent, identically distributed random variables, a kind of modeling that can be called standard or fundamental with respect to applications of the probability calculus. Thus Kolmogorov writes, in his "Foundations of the Theory of Probability" (1933): "Historically, the independence of experiments and random variables represents the very mathematical concept that has given the theory of probability its peculiar stamp." (Kolmogorov 1956, p. 8) Using the weak law of large numbers and related theorems, the probabilities can be estimated from observed frequencies, on the one hand, while they can be used to predict frequencies of all kinds of events, on the other hand, both in a rigorous way.

So, why not simply say that ascriptions of probability are about relative frequencies of outcomes within sequences of random experiments? Some of the aforementioned writers would indeed be prepared to say such things. But still, the question is why such sequences occur in nature. What is it about a set-up that gives rise to the typical probabilistic patterning of outcomes? To put it another way: It certainly seems possible that frequencies emerge out of sheer coincidence, so that it won't be appropriate to let one's expectations be guided by them. The frequencies ought to be counterfactually robust to yield "true" probabilities, and again the question arises which features of reality account for this robustness. It is then only a small step to the view that it is these features, rather than the frequencies themselves, on which an account of objective probability must be based.

This line of thinking is reinforced by the following considerations. In all models with independent, identically distributed random variables there is a positive probability that the relative frequency of an outcome that has a non-trivial probability of occurring deviates from this probability as much as you like, even in very long sequences. This is, furthermore, not an artefact of the mathematical modeling, but to be expected from the very concept of independent repetitions of a random experiment. Therefore it is strange, to say the least, to assume a strict, non-probabilistic connection between probabilities and relative frequencies. For example, in the repeated independent tossing of a fair coin all possible outcome series of a fixed length n occur with exactly the same probability, namely 2^{-n}. Thus, when in a series of n throws the fair coin comes up "heads" n times, this particular outcome series is probabilistically speaking as good as any other outcome series of length n. This does not even change if one replaces the integer n by infinity (provided that probabilities are modeled to be not only finitely, but countably additive). In this case, every particular outcome series (of infinite length) occurs with the same probability 0. Relative frequencies can be typical or atypical for a given set-up only in the sense that, upon repetition, they occur with a higher or a lower probability. Thus, again, it is not actual frequencies of outcomes but the underlying features of the set-up which are the proper truthmakers for ascriptions of probability.

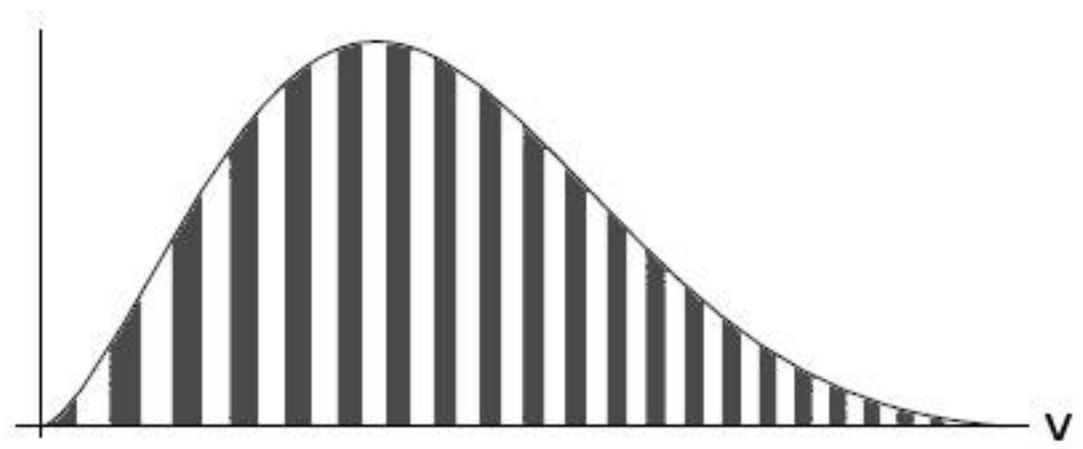
These considerations are quite explicit in von Kries. For him, actual frequencies taken as such are pure contingencies (p. 168). The question is not what the frequencies are, because they could have been and can be any other way, but rather what we should expect them to be, and this, in turn, is governed by certain properties of the set-up that produces the frequencies. The easiest example with which von Kries illustrates his "Spielraum" or range conception of probability is the "Stoss-Spiel" (push-game) similar to a simplified Roulette (see ch. III, sect. 2).

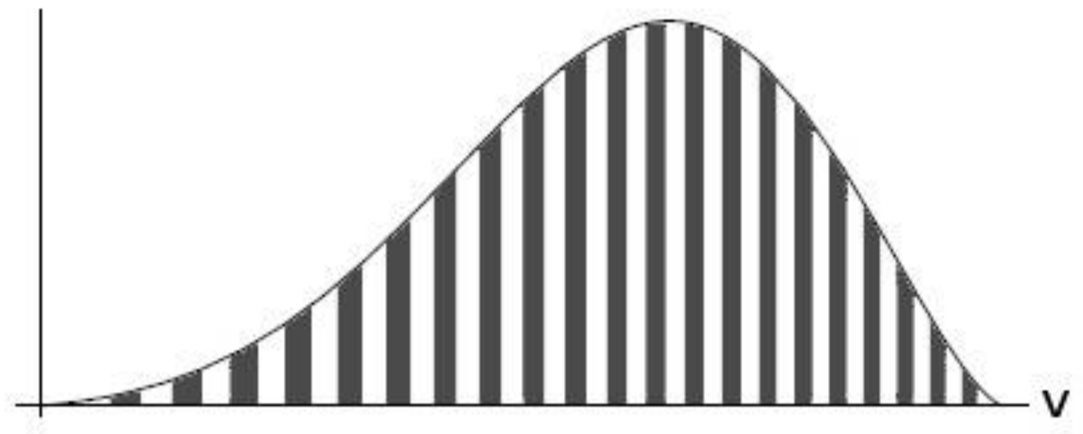
Imagine a very long smooth horizontal channel which is subdivided into narrow alternating black and white vertical stripes of equal size. A ball is pushed down the channel and eventually comes to rest with its center either on a black or a white segment. The probability of “black” in this “Stoss-Spiel” is ½, and so is the probability of “white”. Why that? The whole setting is deterministic, with the final position of the ball depending on the initial impulse or the initial velocity only, if we take all other parameters that influence the final position, like air pressure or the properties of the ball and the channel, as fixed boundary conditions. Also, the channel is assumed to be perfectly even, so that the frictional forces that slow down the ball do not depend on the ball’s position during the movement.

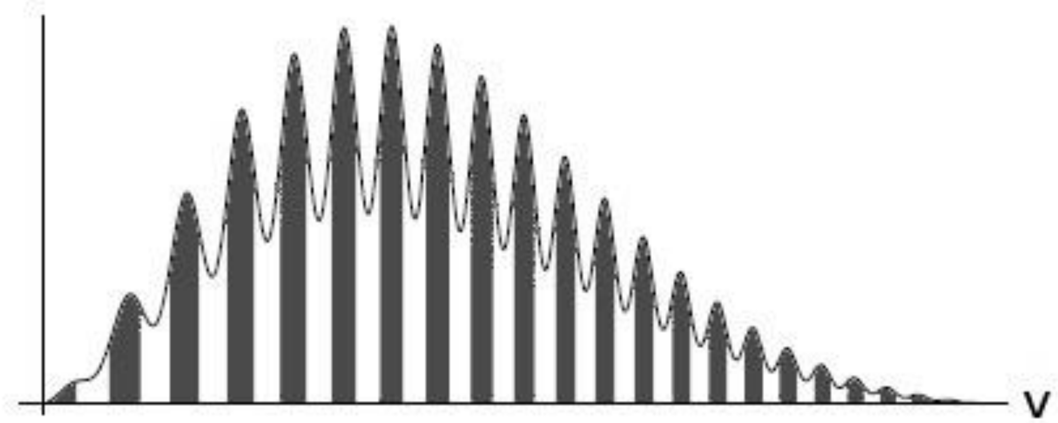
Von Kries rejects the principle of indifference or insufficient reason that underlies classical ascriptions of probability: We can neither say that black and white are equally probable (as these are the two possible outcomes of the game), nor that each stripe is equally possible as the one on which the ball finally rests (as all the stripes are of equal size). Von Kries notes that there is no reason to judge that the ball may come to rest on the 500th segment as easily as on the 10.000th. Quite the contrary: we have every reason to suppose that, when ordinary humans play this game, the 500th and the 10.000th segment are *not* equally likely to contain the ball’s final position. But, and this is the crucial insight, all we need to ascribe equal probabilities to white and black is to assume that *adjoining* segments are *almost* equally likely to cover the final position. Whatever the probability that the ball comes to rest on stripe no. n may be, it is certainly only a little different from the probability that it rests on stripe no. $n+1$. This is sufficient to conclude that black and white are approximately equally probable.

According to von Kries, every continuous probability attachment (“stetige Wahrscheinlichkeits-Ansetzung”, p. 51) to the possible final positions of the ball gives the outcomes “white” and “black” approximately equal probability. He notes that the notion of continuity employed by him is not the mathematical one, which roughly means that there are no jumps in the distribution as represented by a density function. Here, the requirement is rather that the density is of appropriately bounded variation, meaning that it must not oscillate too quickly. Only sufficiently smooth density functions are considered. About all this von Kries is quite clear, although he lacks some modern terms like “density function” or “bounded variation”. Instead of final states one can as well consider initial states, like initial velocity or impulse or kinetic energy of the ball when it is pushed down the channel. Given the dynamics of the system, any sufficiently smooth distribution over possible initial states transforms into such a distribution over possible final positions.

The following pictures show two density functions of appropriately bounded variation and one quickly oscillating one over the one-dimensional initial-state space that can be associated with the “push-game” (cf. Strevens 2011, p. 347–348). The space consists of all possible initial velocities v of the ball and is represented by the v-axis. The black and white columns are given for better visualization. They highlight the partition of the v-axis induced by the outcomes “white” and “black”. One would get a very similar picture, just with columns of equal instead of decreasing width, when one takes the possible final positions of the ball as constituting the state space. Each sufficiently smooth distribution gives the outcomes “white” and “black” a probability of approximately ½. Not every quickly oscillating one yields deviating probabilities, but some do. In order to rule out all these, one may, for example, require that the density functions under consideration are differentiable with the absolute value of the derivative below a suitable threshold throughout.



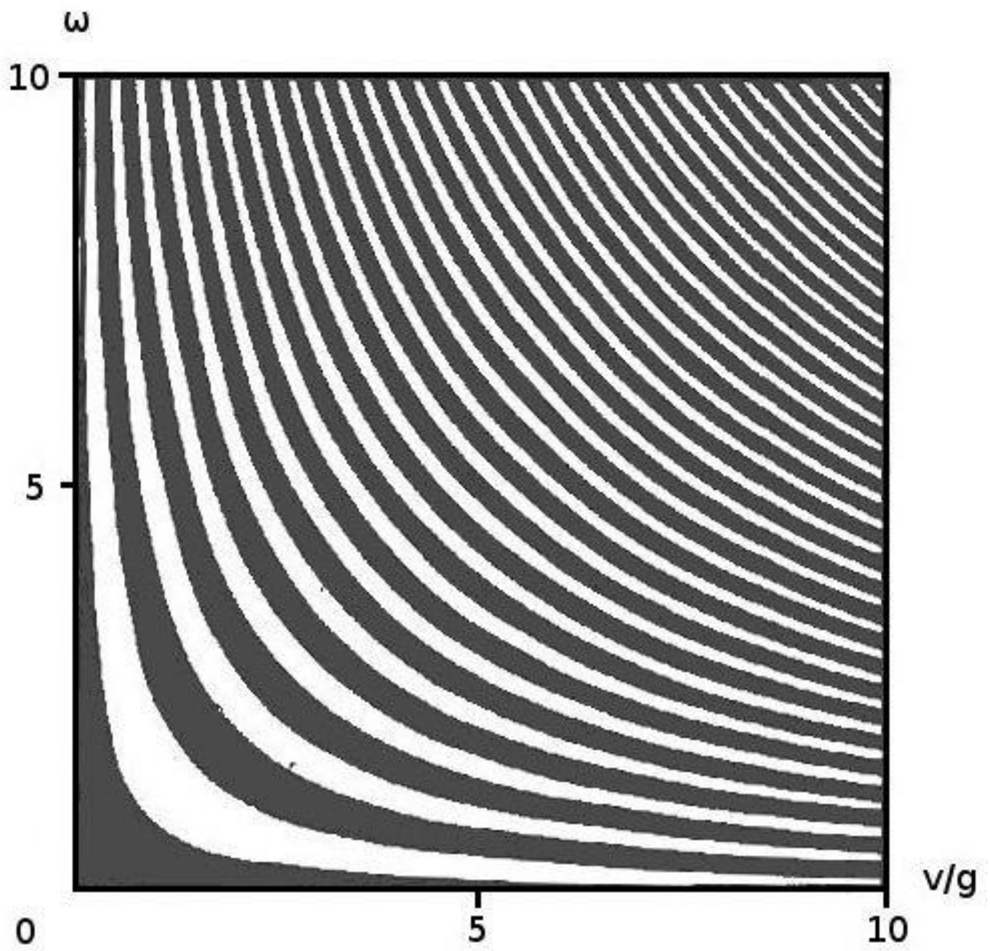




The requirement that every sufficiently smooth distribution over possible system states should yield the same outcome probabilities is an important feature of von Kries's conception. It is not as if one could already justifiably ascribe probabilities when the relative sizes of the ranges that correspond to the possible outcomes are known. That would amount to the choice of a uniform distribution which is what the classical conception of probability does. Rather, the point is that the ranges of states that correspond to the respective outcomes are entangled in a way such that any sufficiently "regular" or "smooth" or "well-behaved" distribution over possible states yields the same outcome probabilities (at least approximately).

The thing that remains as an assumption of equipossibility that might remind one of the Laplacean conception is the *approximate* equiprobability of small *adjoining* ranges *of equal size* – an assumption which seems quite innocent in comparison to the various uses and misuses of a Laplacean principle of indifference. But still, there is such a supposition. When introducing his example, von Kries already makes use of the concept of probability in assuming that proximate segments of the channel are almost equally likely to cover the final position of the ball. We will treat the meaning of such an assumption and the problems arising from it in section iii).

Let's have a look at Keller's analysis of a coin toss as a second example for this approach to probability (Keller 1986; cf. Engel 1992, pp. 44–48). In the Stoss-Spiel of von Kries, the final position of the ball may be viewed as depending on its initial velocity only, given fixed boundary conditions. Thus, the initial-state space is one-dimensional. The coin in Keller's analysis is supposed to be flipped from a fixed position straight into the air. The toss ends abruptly when the coin's center of gravity reaches the initial height. The coin is not allowed to bounce on a surface, but caught when it reaches its initial position, and the side which is up at that moment counts. In this setting, the coin shows "heads" or "tails" after the toss depending on two parameters: the initial vertical and the initial angular velocity, so the initial-state space is two-dimensional here. Again, several boundary conditions are assumed to be constant. The mathematical analysis shows a partition of the state space induced by "heads" and "tails" that looks like this:



Cf. Engel (1992, p. 46). ω denotes initial angular velocity, v/g initial vertical velocity divided by the constant gravitational acceleration of the Earth.

Keller actually derived the structure of the space from the dynamics of the example. It can be extended by including further variables that characterize the coin's initial position and by including the bouncing of the coin on a perfectly flat surface (see Engel 1992, pp. 47–48, but also Diaconis et al. 2007). The requisite mathematics is difficult, and it is not possible to rigorously treat cases that are much harder than this.

Von Kries holds that whenever precise probabilities attach to a process, there is an underlying state space of this kind, although its structure can only be made explicit in comparatively simple examples. We can meaningfully ascribe *numerical* probabilities to certain events and apply the mathematical calculus of probability if and only if such a situation obtains in the background (see pp. 73–74, p. 113, p. 127, ch. IX sect. 14). He was the first to introduce this kind of approach to probability. His “Die Principien der Wahrscheinlichkeitsrechnung” is a careful and in-depth analysis of the matter, but almost entirely informal. The requisite mathematics was in a comprehensive way done by Hopf (1934, 1936) for the first time, who deepened and extended results obtained by Poincaré (1896; see also 1902, ch. 11). The term “method of arbitrary functions” is due to them. Hopf has done a lot to push rigorous treatments of examples to their limit, but there is still some work done nowadays (see the monograph Engel 1992). Taking the work of von Kries, we may say:

> (Arbitrary Functions) Let ***E*** be a random experiment with an associated continuous state space ***S***. Let *A* be a possible outcome of ***E***. If every not-too-quickly oscillating density on ***S*** yields roughly the same value *p* when integrated over the subset of states associated with *A*, then there is an objective probability of *A* upon a trial of ***E***, and its value is *p*.

Equivalently:

> (Range Conception) Let ***E*** be a random experiment with an associated continuous state space ***S***. Let *A* be a possible outcome of ***E***. If *A* is represented in each bounded and not-too-small interval within ***S*** with roughly the same proportion *p*, then there is an objective probability of *A* upon a trial of ***E***, and its value is *p*.

These definitions are meant to be preliminary and will be explicated and modified subsequently. Definitions of such a kind are not contained in von Kries’s writings, but they may be said to capture his ideas in modern terms. See Zabell (this volume) for an analysis of von Kries's account on his own terms.

ii) Scope of the approach

(Range Conception) is meant to establish truth conditions for statements of numerical probability. The term "random experiment" has to be taken in a wide sense, referring to any clear-cut kind of situation where this kind of consideration could reasonably be applied. As only relatively simple examples can actually be analysed mathematically, we often have to be content with the warranted assumption that a given process can be associated with a state space of the indicated kind. Not being able to directly assess the relative sizes of the "ranges" that correspond to the different possible outcomes, we have only indirect evidence for them, namely, observed actual frequencies of outcomes or apparent symmetries. These serve as conditions of warranted assertibility, but not as truth conditions for the respective probability statements.

With this qualification, the domain of applicability of the approach is larger than it may at first seem. The paradigm examples are all from the realm of classical mechanics, the most important being the application to statistical mechanics. It was already sketched by von Kries (ch. VIII; cf. Strevens 2003, ch. 4.8). Other writers have developed ideas how to apply the approach to probabilities in biology (Strevens 2003, ch. 4.9; Abrams in preparation), and the social sciences (Abrams 2012b). These are necessarily sketchy. Strevens (2003) and, more streamlined, Strevens (2013) are two books that explore the whole scope of an approach of this kind.

Von Kries was rather restrictive, however: He wanted numerical probabilities to be meaningful in application to single cases, that is, not only to a type of random process or experiment, but to its particular instances. According to him, probability statements are first and foremost about single cases, and only derivatively about types (see ch. V, sect. 6). Therefore, he disdained the probabilities given by physiology, psychology, and the social sciences (see ch. IX, sect. 6–13). This is noteworthy in particular because physiology and psychology were his main professions, so he argued for a view of probability not suited to his everyday work. Numerical values for, e.g., the probable course of a disease or the effectiveness of a certain treatment come by averaging over very different individual cases from which a global statistic has been compiled. Presumably there are several "hidden parameters" that influence the outcome in single cases. A physician who was about to treat a specific person would be ill advised to base his expectations *just* on these statistically compiled numbers and not to take into account what he knows about the constitution of the particular patient. His ensuing expectations may be justified, but the respective probabilities cannot be given a definite numerical value.

What should one think about this? Contrary to what von Kries says, it seems clear that a state space always relates to a type of process or experiment, because in an individual case there is also just one particular (initial) state. The continuous space emerges from surveying the possibilities for how a process of a certain *type* can go. The "ranges" come from the leeway left open by the laws of nature, and von Kries himself states that the notion of such a leeway, namely, of a quantified objective or physical or, as he puts it, "ontological" possibility, is meaningful only if applied to a "general case". With respect to an individual case, talk about possibility as well as probability is no more than an expression of ignorance (p. 87). Thus, it seems that he was simply in error when claiming that probability statements primarily refer to single cases. We have to know what counts as a repetition of the experiment, or, equivalently, a process of which type is considered in order to meaningfully talk about an initial-state space with definite ranges of the various possible outcomes.

Nevertheless, von Kries's point regarding medical, psychological or socio-economic probabilities stands. And one could as well include certain applications of probability in biology which were not yet considered by him. As far as these probabilities are extracted from results of statistical surveys, they very often come from averaging over cases that are quite diverse also in *nomological* respects. These applications are not comparable to repeatedly throwing the same dice, or to drawing balls from the same urn (with replacement and after due shuffling). Rather, they are like drawing balls from urns with varying compositions, or to throwing dice with unequal centers of gravity. The relevant state space or the partition induced by the outcomes under consideration may be quite different each time, and consequently it is impossible to make an inference from observed actual frequencies to certain definite "ranges".

This, however, does not mean that (Range Conception) does not apply here. One should rather say that there *are* range probabilities in these contexts, only that biological, medical, psychological, or socio-economic statistics normally do not yield them. But they may at least give upper or lower estimates for them, and, furthermore, one would approach the numerical probabilities appropriate in a particular case when investigating many strictly comparable cases, e.g., when one gave a certain medical treatment to people of very similar constitution.

In addition to this, the probabilities given by biological etc. statistics might even receive a straightforward reading in terms of the range conception, namely, when one relates them to the idea of a probabilistic mix of varying circumstances. The point can be illustrated thusly: Imagine six urns of varying composition. One of them is selected by a regular throw of a die, and a ball is drawn from that urn after careful mixing. What the probability of drawing a ball of a certain colour is depends on what counts as the experiment conducted here: Either, drawing a ball at random from a specific urn, or randomly selecting an urn and then drawing a ball from it. Either way, the probabilities can be given a range interpretation. The numbers given by biological etc. statistics can be related to a set-up of the latter kind, and insofar there is no in-principle obstacle to regarding them as range probabilities.

We have to be careful, though, not to base claims about objective probabilities on mere fictions. It may be very difficult to get a firm grasp on some clear-cut random mix of varying circumstances that has to be tacitly assumed for the applications under discussion. Which process, exactly, is considered in such a case, what is the well-defined "experiment type" corresponding to a specific state space? As long as this is not fleshed out, it remains doubtful whether certain statistically compiled numbers can be said to mirror probabilities that are in any sense objective. Although difficult to assess in its scope, the scepticism expressed by von Kries is partially warranted. This, however, does not only concern the range conception, but objective probability in general, so I need not take a definite stance on the issue.

iii) Interpreting the range probabilities

The questions concerning the scope of application of the range approach may be tricky but do not pose fundamental interpretational problems. We now turn to these.

As mentioned in section i), when introducing his approach von Kries relies on the principle that adjacent small regions of equal size within the state space are approximately equally likely to cover the actual state of the process. One may ask: equally likely in what sense? Or take (Arbitrary Functions): What do the density functions mentioned here mean, what do they stand for? A natural idea would be that they represent probability distributions on the state space. Given such a distribution for initial states, the outcome probabilities are thereby fixed – numerically, but also in their meaning. How to interpret outcome probabilities entirely depends on the proper interpretation of input probabilities, i.e., of the probability distributions given by the density functions on the state space. With typical random experiments the space is structured in such a way that the exact form of the density does not matter, so we need not care what the probability distribution on the state space in fact is. As long as it is not very eccentric, the outcome probabilities are virtually independent from its exact form. Yet, the independence is not perfect, because different smooth densities on the state space may well lead to slightly differing outcome probabilities. This does not matter much, but clearly, in this view the interpretation of the outcome probabilities is dependent on the meaning of the input probabilities given by the density functions, and vice versa. When introduced in this way, the account seems to presuppose a conception of probability instead of providing one.

On the other hand, (Range Conception) neither mentions nor seems to presuppose in any other way a notion of probability. It simply takes partitions of state spaces as providing truth conditions for probability statements. Why not take the range conception as it stands as a self-contained interpretation of probability? (I have already tried to argue for this in my 2010 and 2012.) As (Arbitrary Functions) is equivalent to (Range Conception), there is *a fortiori* no circularity involved even here. One has to be very clear that according to these definitions probabilities are given by proportions in appropriately structured state spaces, and that's it. They do not derive from a state space *in connection with* a distribution, or even a class of distributions, on it. The state space, to be sure, must be such that any probability distribution on it that is not extremely "irregular" yields approximately the same outcome probabilities, but whether the space is that way depends on the space and the partition induced by the possible outcomes alone.

But probability statements do not ordinarily mean to refer to proportions in suitably structured state spaces. One does not arrive at these truth conditions by conducting some piece of meaning analysis. This fact may prompt the feeling that one has to say more about probability than is contained in (Range Conception) or (Arbitrary Functions). Why *should* proportions in suitably structured state spaces associated with certain processes count as the probabilities of the respective outcomes? Whatever entities are given the label "objective probability", they deserve their name only if they can make a justified claim to guide our expectations. So, the problem discussed here is not about circularity in the sense that some notion of probability would be presupposed by the range approach, but about the conditions under which it is appropriate to let one's expectations be guided by ranges in continuous state spaces.

A first idea would be that it is appropriate if and only if the distribution of actual initial conditions emerging upon repetition of the experiment looks random and is such that it can be approximated by some non-eccentric smooth density. But then the whole approach becomes a variety of frequentism. The connection to frequencies is not straightforward, to be sure, because there is an idealizing step involved: the replacement of a discrete empirical distribution of actual initial states by an integrable density. Nevertheless, interpreted in this way, the range approach yields a sort of frequentist probabilities. They primarily attach to types of experiments or processes and only derivatively, if at all, to particular instances. This line of thought would be favoured by Hopf (1934, 1936), Reichenbach (1920; 1935, § 69), and von Smoluchowski (1918). Modern advocates of (considerably different) varieties of it are Abrams (2012a) and Strevens (2011).

Alternatively, the densities can be seen as reflecting our uncertainty or ignorance with respect to the obtaining conditions. This is Savage's (1973) and Myrvold's (2012) approach (see also Engel 1992, p. 4). Then the resulting probabilities are subjective or epistemic ones, distinguished by their robustness: As a matter of fact, it is not possible for us to improve on them in our expectations of outcomes of experiments. Brian Skyrms's (1980) notion of resiliency is relevant here: Probabilities that appear to be objective are nothing but resilient subjective or epistemic probabilities. The resiliency may have different sources, but the kind of situation described by the method of arbitrary functions is certainly one of them. In this view, one starts from a subjective or epistemic conception of probability and enriches it by objective aspects as captured by (Range Conception). The resulting probabilities attach to single instances as well as to types of processes or experiments.

Von Kries should be read as advocating something like this, although he is not easy to understand in this respect. His "stetige Wahrscheinlichkeits-Ansetzungen" (continuous probability attachments) are not supposed to mirror actual or hypothetical frequencies. This is clearly shown by some of his considerations concerning the proper applications of probability. As we have seen, he is rather restrictive, on the one hand, but on the other hand he mentions the idea of attaching probabilities to the possible values of natural constants, like the specific weight of a substance (pp. 24–25). Although he subsequently rejects such applications as misguided (pp. 30–31), essentially because of Bertrand-like paradoxes, the fact that he considers them at all shows that at least his starting point is a reading of probability that has nothing to do with repeatable physical processes. In a similar vein, Wolfgang Pietsch (in preparation) attaches his more general "causal probabilities" even to hypotheses.

It is worth noting that on an epistemic reading, a striking vagueness of (Range Conception) receives a natural resolution. The definition speaks about arbitrary bounded intervals within the state space that are "not too small". The condition laid down there cannot hold for *all* intervals, to be sure, but it has to hold for all intervals above a certain minimum size. What fixes this size? On the epistemic reading, it depends on our capacities of measurement and control. For the emerging probabilities really to be "resilient", it is important that we are not able to deliberately aim at or foresee the occurrence of a subset of the state space in which the proportion of an outcome is significantly dissimilar to its overall share in the state space.

It may be due to his basically epistemic outlook concerning probabilities, which he is inclined to call "logical" (in a sense of this word no longer in vogue today), that von Kries received a somewhat one-sided reception. Primarily, he was read and cited by forerunners or advocates of a logical theory of probability (see Heidelberger 2001). Here, von Kries's continuity requirement is dropped. What is left is the idea of deriving probabilities from ranges of possibilities that are now interpreted as logical possibilities. This, however, is not what von Kries himself had in mind. The idea of an underlying state space with the characteristic that any continuous attachment of probabilities to it yields the same probabilities for the events in question is central to his approach. It means that there is a distinctly "ontological" aspect to his account of probability that gives it a very different flavour from a logical conception. His "ranges" are ranges of possibilities left open by the laws of nature, and are of a certain special structure that is a matter for natural science to determine. Therefore, the term "natural range conception" would be more precise than "range conception" with regard to the approach.

Let's come back to our main interpretational problem. The aforementioned writers all share the idea that structures of the indicated kind underlie the successful applications of the probability calculus. As a matter of historical fact, each of them tended to or took as a starting point either an epistemic or a frequentist understanding of probability, and passed this on to the range probabilities, with von Kries belonging to the former group. Thus, the range approach does not appear to be an interpretation of probability in its own right, but rather to be supplementing either a basically frequentist or a basically subjectivist or epistemic outlook. In order to improve on this, we first have to confront another difficulty.

iv) Refining the range conception

A suitably eccentric distribution on the state space, interpreted as a probability distribution, would yield deviating outcome probabilities. Only when we are considering reasonably well behaved distributions can we equate the outcome probabilities with the respective proportions within the space. Now, the method of arbitrary functions, as developed by Poincaré (1896; 1902, ch. 11) and Hopf (1934; 1936) really works for *arbitrary* densities. They need not even be continuous, just (Lebesgue-)integrable. How can this be?

Poincaré and Hopf choose a density and then vary certain parameters of the physical setting. Poincaré considers a roulette wheel with a fixed density for initial conditions (a joint density for initial position and initial angular momentum) and imagines narrowing the wheel's sections more and more. This means that the state space is constantly changing: not with respect to the variables, but with respect to its patterning in view of the results "red" and "black", while the density, or rather its shape, is kept fixed. No wonder that in the limit *every* density yields the probability ½ for "red" and likewise for "black". In a similar manner, Keller (1986), who analyzed the coin flips, shows that any joint density for initial angular velocity and initial vertical velocity whose functional form is chosen once and for all and which is then shifted away from the origin of the state space along any straight line, yields probability ½ for "heads" as well as for "tails" in the limit.

In this way, the problem of quickly oscillating densities is solved – but at the price of constantly changing the physical circumstances, while keeping the shape of the density. With Poincaré's roulette wheel, the number of red and black sections is increased beyond any limit, and the coin in Keller's analysis is flipped ever more vigorously, with ever higher angular and vertical velocities. In the same vein, Hopf proves general theorems to the effect that the distribution of certain outcomes is completely independent from the distribution of initial conditions, as long as this distribution can be expressed by a density at all.

It is, however, not clear what bearing exactly the reasoning of Hopf and Poincaré is supposed to have on the philosophical problem of determining truth conditions of probability statements. Real coins are not flipped ever more vigorously, and real roulette wheels have compartments of a certain fixed width. The moving-about of a density of a fixed functional form over a state space, or the changing of physical circumstances towards limiting cases does not admit of a realistic construal. It clearly remains possible that in repeating a random experiment, actual initial states obtain that approximately match a density which is periodic in a critical way or puts extreme weight on just one "patch" of the state space. If, moreover, this specific density emerged repeatedly, i.e., if always when the experiment was often repeated, actual initial states followed suit, we would reasonably judge it to be reliable or counterfactually robust.

This judgment could well be wrong, everything we observed could be a huge coincidence. But provided there is something behind the specific eccentric distribution that (probabilistically) explains its stability, expectations about outcomes should be guided by it rather than by the proportions of the outcomes within the state space. The fact that if we moved the eccentric density far enough about the space, we would get the probabilities we expected, is simply irrelevant here. It won't do to dig one's feet in and stick to (Range Conception) if this means to ignore a mechanism that tends to produce deviating frequencies of outcomes. Objective probabilities may be equated with proportions in a state space only if these connect to appropriate degrees of belief, and assuming a feature that reliably produces deviating relative frequencies, one cannot reasonably maintain this connection. Thus, an ascription of probability with a claim to objectivity can definitely not be made true by the *mere* fact that our experiment relates to a state space with such-and-such a structure.

There are real-world cases in which robustly periodic distributions of actual initial states are to be expected. An easy example is provided by a wheel of fortune or carnival wheel. A rotating disc divided into alternating red and black segments eventually comes to rest, and yields the outcome "red" or "black" according to a fixed pointer outside. In contrast to the roulette wheel and to von Kries's "Stoss-Spiel", the frictional forces slowing down the wheel also depend on its position. The positional component of the force is due to nails or pins that are arranged on the wheel's rim exactly on the borderline between any two adjoining segments. The pointer outside bumps against those pins, and the wheel is slowed down rather quickly in this way.

Now, if the segments are not of equal size, but, say, the black ones are twice as large as the red ones, the probability of the outcome "red" is not 1/3, but considerably higher, because it is mainly the pointer bumping against the pins that stops the wheel. Examples of this kind were treated for the first time by Hopf (1936; see also Engel 1992, pp. 103–106). They are remarkable because here the exact probabilities are not to be guessed in advance from symmetry considerations, but have to be derived by applying the method of arbitrary functions. Any sufficiently smooth distribution over initial velocities (or initial angular velocities) is transformed into a "critically periodic" distribution over final positions. If *these* are taken to constitute the state space associated with the experiment or are taken as initial states for some further physical process, we have a state space that naturally comes equipped with distributions that are periodic in just the critical way. If we apply the range conception to this latter state space, it gets us the outcome probabilities wrong.

Of course, there is an obvious explanation why the distributions of final positions are periodic in this case. We just have to take the initial (angular) velocities of the wheel as initial states to get the probabilities right. The portions of velocities leading to "red" are considerably larger than 1/3 within the space of possible initial (angular) velocities. This example illustrates von Kries's answer to the problem of a robust empirical distribution with high oscillation over the state space. It must be due to a special physical setting, and we have to take the initial states of this setting as establishing the probabilities. In von Kries's wording, the ranges must be "ursprünglich" (original, primordial) to give us the true probabilities (pp. 34–35, 70–71).

The idea is the following: Taking the ranges in a state space to determine the probabilities of the possible outcomes of a type of physical process means to carry out a cut in time that is in principal arbitrary. This point is not to be confounded with the above-mentioned one that we should view the range probabilities as attaching to a type of experiment, and only derivatively to its particular instances. Even if this is agreed, we can ask: Why not take later or earlier possible states of the same (type of) process as constituting the relevant space? Nothing hinges on this choice as long as it does not change the proportions of the outcomes in question, but if it does, it is the prior space that gets the probabilities right, because the later states depend on the former, not vice versa. If any reasonably well-behaved distribution on a prior state space transforms into a quickly oscillating one on a space downstream in time it is the ranges in the former one that yield the true probabilities. Ranges are called "original" if all the various state spaces upstream in time would yield the same probabilities for the possible outcomes of the process. Given this, the consideration of the coming-about of actual initial states does not change the probabilities.

If one thinks of classical or logical probability and (mistakenly) reads von Kries as advocating something like this, one might have the idea that an "original" or "primordial" state space is a maximally refined one, like Rudolf Carnap's language-dependent set of "state descriptions" to which any assignment of logical probability is to be reduced. In contrast to this, von Kries has temporal or causal, not logical, precedence in mind. He says that "the probability of a present or future state is to be judged from the former modes of behavior that are suited to bringing it about" (p. 34), and much more to the same effect.

Interpreting von Kries, we have to say the following. If, upon repetition of the experiment, an "extreme" density emerges on the initial-state space, if, that is, actual initial states match a quickly oscillating density function, either this phenomenon is capable of an explanation in terms of a specific mechanism, or, if no such explanation is forthcoming, it has to be viewed as a mere coincidence not affecting the probabilities. Either the ranges upon which we base our ascriptions of probability are not "original": then we have to go back in time and consider something else to be the "true" initial state space of the experiment, at the same time discovering a mechanism that explains the peculiar empirical distribution on the state space downstream in time. Or the move back in time does not change anything with regard to the respective proportions of the different outcomes: then we have to view this peculiar distribution as being purely accidental and consequently as irrelevant for our expectations regarding the possible outcomes of the type of process. As long as there is "nothing behind" the eccentric distribution, we should not consider it to be robust, even if it appears to be so, and stick to the range probabilities given by the state spaces. This follows from the basic idea of the range conception, namely, that we should not take brute actual frequencies as truthmakers for probability statements, but those features of set-ups that account for them (if there are any), as the frequencies themselves can be purely accidental and thus misleading with regard to the probabilities.

Von Kries is inclined to view the "principle of ranges" as he calls it, as a synthetic a priori principle, although he does not take this idea too seriously and does not make much of it (pp. 170–171). This line of thought is due to Neo-Kantian inclinations he shared with many German philosophers and scientists of the time. The idea would not be that probabilities can be ascribed a priori, as it is an empirical question what the laws of nature are, which determine the structure of state spaces. Rather, the thought is that any true numerical probability must come from a proper primordial initial-state space, and that actual distributions of outcomes gain their counterfactual robustness and their credentials in guiding expectations entirely from there. If there is a conflict between original ranges and actual relative frequencies, it is the latter that have to give way:

(Range Conception Refined) Let $\boldsymbol{E}$ be a random experiment with an associated continuous n-dimensional state space $\boldsymbol{S}$. Let A be a possible outcome of $\boldsymbol{E}$. If A is represented within each equilateral n-dimensional, bounded, and not too small interval of $\boldsymbol{S}$ with roughly the same proportion p, and if, furthermore, $\boldsymbol{S}$ is original with respect to A in the sense that every prior state space displays a similar structure with respect to the outcome A and contains it with the same ratio p, then there is an objective probability of A upon a trial of $\boldsymbol{E}$, and its value is p.

The qualification "equilateral" is due to the fact, also noted by von Kries, that a state space may well consist of very long, very thin "stripes" (in the two-dimensional case), or very large, but at the same time very thin "layers" (in the three-dimensional case), etc. (see pp. 66–67, fn. 1). The stripes or layers etc. give rise to bounded intervals with a considerable n-dimensional volume in which all initial states lead to the same outcome, but the range conception should, intuitively speaking, nevertheless be applicable. It is impossible for us to deliberately aim at one particular "stripe" or "layer", and it is in need of explanation when actually occurring states fit a density that is quickly oscillating in at least one dimension.

The notion of an original or primordial state space is evidently problematic. It ultimately points to circumstances that obtained at the beginning of the universe. The initial state of our world, in every detail, is supposed to be part of a truly global initial-state space. It contains some states that give rise to trajectories on which the experiment of interest $\boldsymbol{E}$ is conducted, and if we survey just these states, they have to form a subset with the indicated structure. This requirement is implicit in the notion of an original state space, but von Kries did not think about it. The idea of a beginning of the universe was not around at his time and would presumably have been alien to him. He notes the severity of the requirement that all the earlier state spaces one might conceive of as representing the "initial states" of a given type of process yield the same probabilities, but thinks he cannot do without it (p. 35).

The problem of how to interpret a probability distribution for possible initial states of the universe is known from statistical physics (see Sklar 1993, ch. 8; Albert

2000, ch. 4). If we simply stick to ranges in original state spaces without considering probability distributions on them, and insist that they in themselves give us the probabilities of events, the recurring question is how we can be entitled to interpret these ranges probabilistically. Is this not just a stipulation? To get this problem better into focus, we turn to a final and fundamental objection to the range conception von Kries was unaware of.

v) The problem of the measure

The structure or patterning or partition of a state space induced by the different outcomes depends on the choice of variables that characterize the initial states. There are continuous changes of variables that give rise to different patternings with arbitrarily chosen proportions of the respective outcomes. This problem is best known under the label "Bertrand's paradoxes" and seems to befall any symmetry-guided attachment of probabilities to a continuum (see, e.g., van Fraassen 1989, ch. 12).

In the typical paradoxical examples different representations that seem to be equally natural lead to very different probability assignments. Essentially, any non-linear transformation of variables will do. This is definitely not the case with the range conception, due to the special structure of the state space. Here, any ordinary choice of variables leads to the same outcome probabilities (cf. von Kries 1886, p. 54, fn. 1). We have to consider very peculiar periodic changes of variables if we really want to alter the ratios with which the outcomes are represented within the space. This mirrors the fact that all "well-behaved" distributions on the state space, when interpreted as probability distributions, yield the same outcome probabilities.

In the "Stoss-Spiel" of von Kries, e.g., nothing changes with respect to the range probabilities, if the initial velocity (or momentum) of the ball is replaced by its square (or initial kinetic energy). Such simple nonlinear transformations give rise to different probability attachments in the original Bertrand's paradoxes and similar cases, but here they pose no threat. Only if one expands by means of a suitable periodic transformation the sections of the initial-state space leading to "white" as outcome and shrinks the "black" ones to the same degree, one gets ranges of a very different size. The physical quantity which is represented by means of an interval scale in this way does not play any role in physical theory. The same holds in general: Transforming the space in a way that changes the proportions of the outcomes means that the resulting vector components do not correspond to anything like familiar physical quantities.

The range conception of probability proposes to trace back probabilities to state space volume. One can either tinker with the quantities that are used to characterize the possible states, or with their representation, that is, with their mapping onto mathematical space, or with the notion of volume therein, to cause trouble for the range conception. The state space is a mathematical representation of physical possibilities, each dimension representing one of the quantities. If n quantities are used to characterize the state of the system, the corresponding space is the n-dimensional real vector space, or some proper part of it. Talk about proportions within this space draws on a notion of n-dimensional volume or measure. The standard choice in this regard is the so-called Lebesgue measure which generalizes the intuitive notion of area or volume in a unique way.

Now, the question is: Why apply the *Lebesgue measure* to a state space arising from a *linear mapping* of *standard physical quantities* onto mathematical space in order to determine the outcome probabilities? That the range probabilities are fixed in *this* way is the tacit assumption behind everything I have described so far. One could, instead, use some non-standard quantities to characterize the state of the system. Or one could map the standard ones in a distorted way onto mathematical space, so that they are no longer represented by interval scales. Or one could use an extraordinary measure on the state space instead of the Lebesgue measure.

These three possibilities amount to the same thing, though. With respect to the probabilities, they are nothing but three different ways to perform the same transformations. Thus, one can without loss talk about "the problem of the (right choice of) measure (on the state space)". Many transformations, and, for that matter, all half-way normal ones, do not change the relevant proportions, but some rather far-fetched do. It does not matter whether one calls into question or demands a justification for the use of ordinary quantities, or the straightforward ways of mapping them onto a mathematical space, or the standard measure thereon. Either way, one has to make some extremely weird or "unnatural" choices to affect the proportions with which the different outcomes are represented within the state space.

(Range Conception Completed) Let ***E*** be a random experiment with an associated continuous *n*-dimensional state space ***S***, using natural modes of representation. Let *A* be a possible outcome of ***E***. If *A* is represented within each equilateral *n*-dimensional, bounded, and not too small interval of ***S*** with roughly the same proportion *p*, and if, furthermore, ***S*** is original with respect to *A* in the sense that every prior state space arising from natural modes of representation displays a similar structure with respect to the outcome *A* and contains it with the same ratio *p*, then there is an objective probability of *A* upon a trial of ***E***, and its value is *p*.

This may not seem very satisfying. One could object the following: *Prima facie* the weird transformations amount to nothing more than unfamiliar representations of possible states. But the reasons not to choose such a representation are merely pragmatic. In principle, it is as good as any other. To press the objection this far, however, means to turn the problem into a skeptical one. One should not concede too quickly that *all* the choices made in representing possible states are pragmatic and that in principle every alternative would be as good. The conditions laid down for the range probabilities guarantee that there is no easy or normal way to different probabilities, and this is the crucial difference to Bertrand's paradoxes and similar cases that are rightly viewed as causing deep trouble for the idea of assignments of probability distributions from symmetry considerations.

As long as all natural modes of representation lead to the same proportions for the possible outcomes, this should be enough objectivity. It would be unreasonable to demand more of a probability to grant it the label "objective". Or so I will try to argue in what follows. From this, it is again evident that the term "natural range conception" would be most appropriate for this approach to probabilities. The assumption that certain modes of representing the set of possible states are "natural", while others are not, is an indispensable element of it. What could justify this assumption?

First, one would certainly regard the sort of partitioning of a state space as given by (Range Conception Completed) at least as a partial explanation as to why the calculus of probabilities is successfully applied to the phenomenon in question and the probabilities are such-and-such (see pp. 167–169). The structure of the state space seems to explain very well the typical probabilistic patterning of the series of results on repeated trials and the robustness of the phenomenon. This kind of explanation, whatever its exact status, and whether or not one is ready to take the underlying structure as providing truth conditions for probability statements, would be worth nothing if weird representations of the space of possible states would be as good as ordinary ones. This is hardly credible.

At this point approaches to probability in the von Kriesian manner may be fruitfully compared with David Lewis's best-system analysis of laws of nature in general and of chance in particular (Lewis 1994, see Beisbart (this volume) for a full-fledged comparison of the approaches). It is no accident that we operate with something like the standard physical quantities and not with weirdly transformed ones, and we should not take it as simply being a matter of convention when we also base our ascriptions of probability on these standard quantities. One could say about the range conception, paraphrasing David Lewis: "All this would be worse than useless if we couldn't distinguish natural from gerrymandered kinds; we could get the analysis to yield almost any answer we liked. But we can distinguish. If we could not, puzzles about chance would be the least of our worries." (see Lewis 1994, p. 477)

Lewis says this when introducing his own ideas, but we can apply it to our problem as well. We have to resist the idea that any mode of representation of initial states is as good as any other, that we cannot distinguish natural from gerrymandered ways. From a somewhat different angle one could say: That explanations of the indicated kind of probabilistic patterns in series of outcomes are good explanations should be taken as a starting point. The explanations offered may not be complete, they do not point to something like a necessity, but they are undoubtedly illuminating. We should not go so far as to completely deny this by allowing arbitrarily weird modes of representation on a par with ordinary ones.

In this manner, the objection is not answered, but rather rejected as a problem that has to be dealt with, except perhaps in some skeptical contexts. But second, one can give more substance to the reply by providing specific arguments in favour of the ordinary modes of representation. There is a broad discussion of what justifies the choice of the Lebesgue measure on the phase space in statistical mechanics. This can be viewed as the most important special case of the problem discussed here, and physicists came up with independent considerations that speak in favour of this measure. First, the Lebesgue measure of a set is invariant given the dynamics of the statistical mechanical system (Liouville's theorem). A set of a certain measure may evolve into a set of very different shape as time passes, but the volume remains the same. Thus, the measure has the physical significance of a conserved quantity. Second, the Lebesgue-measure generalizes the notion of finite number in a natural way, so that one can say that sets of very large Lebesgue measure are "typical" (typicality approach).

But still, these objectively distinguishing characteristics of the Lebesgue measure bear no obvious connection to probability – at least in the sense of probability as actual frequency, which is the sense that seems to be most conspicuous to many physicists. It is one question what the Lebesgue measure or "natural volume" of a certain subset of the phase space is, quite another, how frequently one finds the microstate of a statistical mechanical system being actually within this subset when one considers real-world systems. For a recent discussion, see Ben-Menahem and Hemmo (2012, chs. 3, 4, 6, 8). The widespread feeling among physicists seems to be that even if one can give compelling arguments that a certain measure is the natural one for a continuous state space due to its generalizing the notion of number or respecting well-established physical symmetries or conservation laws, the case for its probabilistic interpretation remains shaky.

In contrast to this, I would like to maintain, in the spirit of von Kries, that one should not tie probabilities too close to actual relative frequencies. If there are naturally distinguished modes of representation of initial states that give the outcomes of processes definite proportions within the set of possible initial states, and if, moreover, the respective state spaces are original in the sense that there is no mechanism that generates special inputs to them, we are entitled to view deviating relative frequencies as mere coincidences that should not guide our expectations.

Most modern philosophers who hold views similar to those of von Kries share the physicists' doubt, however. They are not satisfied with pure "range considerations", but put in one way or other constraints on the distributions of actual initial conditions. They are only prepared to take the "ranges" as truthmakers for probability statements when actual occurrences of initial states follow suit. See Roberts (this volume) for a comprehensive discussion of this "input problem".

Marshall Abrams's (2012a) account turns everything I have said here upside down by deriving the measure on the state space from those initial states that appear in actual experiments. Thus, for him there is neither a problem of justification of the choice of measure nor of eccentric distributions on the state space. The difficulties with which I have struggled here at length simply dissolve because they go into the construction of the measure with which the state space is equipped. This move, however, brings actual frequentism back into play. Abrams's ranges – the "bubbles", as he calls them – are constructed out of actual frequencies. Abrams, to be sure, uses the partition of the state space obtained in his way also to explain actual frequencies and as a basis for counterfactuals, but *ultimately* it is global actual frequencies that are in the driver's seat, because they determine the choice of the measure. It is therefore impossible that probabilities and actual relative frequencies fall apart globally.

An intermediate position is taken by Michael Strevens (2011). His account operates with ranges given by standard physical quantities combined with constraints on actual distributions of initial conditions. These must approximately match a sufficiently smooth density – Strevens prefers to talk about "macroperiodic" densities – and robustly so. This additional qualification is very important. Strevens is not satisfied with actual initial states being the right way, in addition, this right way must not be a brute fact. If the random process were to be repeated more and more often, very likely a density of appropriately bounded variation would emerge for initial states. This counterfactual robustness is spelled out in terms of possible worlds: In most nearby possible worlds the distributions of initial conditions in most long series of repetitions of the experiment can be approximated by a density of appropriately bounded variation. Having said this much, one can even exempt the actual world. The ranges give us the true probabilities, provided that in most nearby possible worlds the distributions of occurrent initial conditions are as indicated, no matter how they are distributed in the actual world. In this way, full frequency tolerance of the probabilities is restored and we have a fundamental parting from actual frequentism.

To my mind, this kind of move is a very natural one. But it is also very demanding: It presupposes a measure on the set of close possible worlds. The task of justifying such a measure seems to lead back precisely to the kind of "range considerations" that prompted von Kries's account. There is no other way to judge the counterfactual robustness of actual distributions of initial states than to analyze how the initial states come about, by what mechanism they are produced. This means in effect to go further back in time and consider prior state spaces, like von Kries does with his idea of original ranges. A measure on the set of (close) possible worlds may be a useful formal tool, but ultimately it must be based on facts about our world. So, which features of the actual world justify the chosen measure for close possible worlds? What could the facts be that underlie robustly smooth initial condition distributions? The only alternative to an approach in the spirit of von Kries seems to be the path taken by

Abrams, namely to base counterfactuals and counterfactual robustness of frequencies on measures derived from global actual frequencies. But the latter are taken as brute facts, then, and any explanation of them or their counterfactual robustness is shallow, because *they* are the ultimate source of everything else mentioned.

Again, it is instructive to take a look at David Lewis's best-system account of chance here (see Lewis 1994). It refers to nothing like the von Kriesian "ranges", but links objective probabilities indirectly to global actual frequencies of events – like Abrams and Strevens do, but in a very different manner. The basic idea is that objective probabilities come from probabilistic laws of nature, where laws of nature are those universal statements that describe our world most efficiently. They may well contain probabilities, because probabilistic characterizations can be comparatively simple while still being very informative. The objective probabilities are what the laws of nature (thusly understood) say they are. Barry Loewer (2001, 2004) and David Albert (2000, see also ch. 2 of Ben-Menahem and Hemmo 2012) have applied this approach to the probabilities of statistical mechanics. Although there is nothing like a direct or easily explicable connection of probabilities and actual frequencies in this approach, there must be *some* connection, and a rather close one too, as probabilities supervene on what actually happens and help to describe it in an optimal way.

Abrams's, Strevens's (in one version) and the Lewis-Albert-Loewer approach can hardly be called varieties of actual frequentism, but in all of them actual frequencies play an important role in fixing objective probabilities. With all of them, it is impossible that probabilities and actual relative frequencies fall apart across the board. The empirical success of ascriptions of objective probabilities is thereby guaranteed, at least in the long run, but I find this position uncomfortable. There seem to be only gradual differences between short, medium-sized, and long runs, or between three tosses of a coin, a thousand tosses, or all such tosses ever conducted during the history of the universe. Even if they all yielded “heads”, this could just be a huge coincidence, not affecting the objective probability of “heads”. This, at least, is a definite possibility given the usual mode of application of the probability calculus. Admittedly, the coincidence would be still greater than with three or thousand “heads” in a row, and consequently, the probability of such an event is even closer to 0, but ultimately the difference between these cases is a matter of degree, not of principle. It seems unduly *ad hoc* to allow frequencies that deviate from objective probabilities in short, but not in long runs, or locally, but not globally.

According to von Kries, it is the nomological aspect of reality in the sense of the dynamical laws of physics that delimits the ranges and thereby fixes the probabilities. These ranges, or their relative sizes, allow us to genuinely explain and predict actual frequencies of outcomes, using the weak law of large numbers or related theorems. All these explanations and predictions are, as it should be, probabilistic in themselves, and the emerging second-order-probabilities can in turn be interpreted according to the range conception. The state space associated with k independent repetitions of an experiment $\boldsymbol{E}$ is simply the k-fold Cartesian product of the state space associated with $\boldsymbol{E}$ with itself. But at no stage is there anything like a strict connection between probabilities and actual events. The price to pay is that the probabilities emerging in this way provide no *definite* guide to what actually happens. Ascriptions of objective probabilities are not *guaranteed* to be empirically successful. But who would have expected that much from probabilities?

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