What is Probability?

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Preface

In October 2009 I decided to stop doing philosophy. This meant, in particular, stopping work on the book that I was writing on the nature of probability. At that time, I had no intention of making my unfinished draft available to others. However, I recently noticed how many people are reading the lecture notes and articles on my web site. Since this draft book contains some important improvements on those materials, I decided to make it available to anyone who wants to read it. That is what you have in front of you.

The account of Laplace's theory of probability in Chapter 4 is very different to what I said in my seminar lectures, and also very different to any other account I have seen; it is based on a reading of important texts by Laplace that appear not to have been read by other commentators. The discussion of von Mises' theory in Chapter 7 is also new, though perhaps less revolutionary. And the final chapter is a new attempt to come to grips with the popular, but amorphous, subjective theory of probability. The material in the other chapters has mostly appeared in previous articles of mine but things are sometimes expressed differently here.

I would like to say again that this is an incomplete draft of a book, not the book I would have written if I had decided to finish it. It no doubt contains poor expressions, it may contain some errors or inconsistencies, and it doesn't cover all the theories that I originally intended to discuss. Apart from this preface, I have done no work on the book since October 2009.

Chapter 1

Inductive probability

Suppose you know that a coin is either two-headed or two-tailed but you have no information about which it is. The coin is about to be tossed. What is the probability that it will land heads? There are two natural answers: (i) 1/2; (ii) either 0 or 1. Both answers are right in some sense, though they are incompatible, so "probability" in ordinary language must have two different senses. I'll call the sense of "probability" in which (i) is right *inductive probability* and I'll call the sense in which (ii) is right *physical probability*. This chapter is concerned with clarifying the concept of inductive probability; I will return to the concept of physical probability in Chapter 2.

1.1 Not degree of belief

It has often been asserted that "probability" means some person's degrees of belief. Here are a few examples:

> By degree of probability we really mean, or ought to mean, degree of belief. (de Morgan 1847, 172)
>
> Probability measures the confidence that a particular individual has in the truth of a particular proposition. (Savage 1954, 3)
>
> If you say that the probability of rain is 70% you are reporting that, all things considered, you would bet on rain at odds of 7:3. (Jeffrey 2004, xi)

I will therefore begin by arguing that inductive probability is not the same thing as degree of belief. In support of that, I note the following facts:

- Reputable dictionaries do not mention that "probability" can mean a person's degree of belief.[1] Also, if you ask ordinary people what "probability" means, they will not say that it means a person's degree of belief.

[1] I checked *The Oxford English Dictionary*, *Webster's Third New International Dictionary*, *Merriam-Webster's Collegiate Dictionary*, and *The American Heritage Dictionary of the English Language*.

- If inductive probability is degree of belief then, when people make assertions about inductive probability, they are presumably making assertions about their own degrees of belief. In that case, statements of inductive probability by different people can never contradict one another. However, it is ordinarily thought that different people can genuinely disagree about the values of inductive probabilities.

- If inductive probability is degree of belief then assertions about the values of inductive probabilities can be justified by producing evidence that the speaker has the relevant degrees of belief. For example, my assertion that the inductive probability of heads in my coin example is 1/2 could be justified by proving that my degree of belief that the coin will land heads is 1/2. However, people ordinarily think that probability claims cannot be justified this way.

- Inductive probabilities are usually assumed to obey the standard laws of probability but people's degrees of belief often violate those laws. For example, if A logically implies B then B must be at least as probable as A, though there are cases in which people nevertheless have a higher degree of belief in A than in B (Tversky and Kahneman 1983).

These facts show that ordinary usage is inconsistent with the view that inductive probability is degree of belief. Since the meaning of words in ordinary language is determined by usage, this is strong evidence that inductive probability is not degree of belief. Is there then any cogent argument that inductive probability *is* degree of belief, notwithstanding the evidence to the contrary from ordinary usage?

Some statements by de Finetti could be taken to suggest that, since our assertions about inductive probabilities express our degrees of belief, they can have no meaning other than that we have these degrees of belief.[2] However, this argument is invalid. All our sincere intentional assertions *express* our beliefs but most such assertions are not *about* our beliefs. We need to distinguish between the content of an assertion and the state of mind which that assertion expresses. For example, if I say (sincerely and intentionally) that it is raining then I am expressing my belief that it is raining but I am not asserting that I have such a belief; I am asserting that it is raining.

Subjectivists often claim that objective inductive probabilities don't exist (Ramsey 1926; de Finetti 1977). However, even if they were right about that, it wouldn't show that inductive probability is a subjective concept; a meaningful concept may turn out to have an empty extension, like phlogiston. My concern here is with meaning (intension) rather than existence (extension); I am simply arguing that the concept of inductive probability is not the same as

[2] I am thinking, for example, of de Finetti's statement that "the only foundation which truly reflects the crucial elements of" the relationship between inductive reasoning and analogy is "intuitive (and therefore subjective)" (1985, 357). Also his statement that "we can only evaluate the probability according to our judgment" (1972, 188).

the concept of degree of belief. I will discuss whether inductive probabilities exist in Chapter 5.

I have thus found no cogent argument to offset the evidence of ordinary usage, which tells us that inductive probability isn't degree of belief.

1.2 Form of statements

I claim that every inductive probability is a probability *of* some proposition H *given* some proposition E. In my coin example, H is that the coin lands heads while E is that the coin is either two-headed or two-tailed and is about to be tossed. If either H or E is changed then the value of the inductive probability may also change. For example, if E' is that the coin has a head on one side, then the inductive probability of H given $E.E'$ (the conjunction of E and E') is 1, not 1/2.

A proposition is not a sentence but rather the meaning of a sentence that has a truth value. Thus we speak of the probability *that it will rain*, given *that it is cloudy*, and these "that"-clauses refer to propositions; it is not in accord with ordinary language to speak of the probability of the sentence "it will rain" given the sentence "it is cloudy."

It is customary to call H the *hypothesis* and E the *evidence*; my choice of letters reflects this. However, H need not be a hypothesis in the ordinary sense, that is, it need not be a tentative assumption. Similarly, E need not be evidence in the ordinary sense, since it need not be known, or even believed, by anyone. Both H and E can be any propositions whatever. In discussions of inductive probability, the terms "hypothesis" and "evidence" simply mean the first and second arguments, respectively, of an inductive probability.

In ordinary language, the evidence to which an inductive probability is related is often not stated explicitly. For example, someone may say: "Humans probably evolved in Africa." In such cases, the evidence is determined by the context of utterance; usually it is the evidence (in the ordinary sense) possessed either by the speaker or by a relevant community.

Statements of inductive probability are commonly expressed as a conditional. For example, someone may say: "If it rains tomorrow then I will probably stay home." A more accurate statement of what is meant here would be: the probability that I will stay home tomorrow, given that it rains (and other things I know), is high. The conditional form may give the false impression that the value of an inductive probability can depend on the truth of a contingent proposition (Carnap 1950, 32).

So far I have been arguing that inductive probability takes two propositions as arguments. I now turn to the *values* of inductive probabilities. Sometimes these are real numbers; in the coin example with which I began this chapter, the inductive probability has the value 1/2. However, there are also many inductive probabilities that do not have any numeric value; for example, the inductive probability that humans evolved in Africa, given what I know, is high but does not have a numeric value. In cases where an inductive probability lacks a numeric value we may still be able to express inequalities regarding it; for example, the inductive probability that humans evolved in Africa, given what I know, is greater than 1/2, even though it lacks a numeric value.

1.3 Logical probability

Let an *elementary sentence* for any function be a sentence that says the function has a specific value for specific arguments. For example, the following is an elementary sentence for inductive probability:

> The inductive probability that a coin landed heads, given that it either landed heads or tails, is 1/2.

By contrast, the following is *not* an elementary sentence for inductive probability:

> The inductive probability of my favorite proposition, given the evidence I now have, equals the proportion of Chicago residents who have red hair.

In this latter sentence, the propositions that are the arguments of the inductive probability are not specifically stated and neither is the value of the inductive probability.

I call a function *logical* if all true elementary sentences for it are analytic.[3] By an *analytic sentence* I mean a sentence whose truth is entailed by the semantic rules of the relevant language.[4] So, for example, if a function f is defined by specifying its values for all possible arguments then the truth value of all elementary sentences for f follows from the definition of f and hence f is a logical function.

It is possible (and not uncommon) to define a function by specifying its values in such a way that the function satisfies the mathematical laws of probability. Therefore, there demonstrably are logical functions that satisfy the mathematical laws of probability.

I claim that inductive probability is also logical. In support of this I note that, since inductive probability isn't degree of belief, the truth of elementary sentences of inductive probability doesn't depend on facts about the speaker's psychological state. Also, it doesn't depend on external facts, as physical probability does; in my coin example, the inductive probability has the value 1/2 regardless of whether the coin in fact is two-headed or two-tailed. Thus there do not appear to be any empirical facts on which the value of an inductive probability could depend.[5]

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[3] This conception is similar to that of Carnap (1950, 30).

[4] For a refutation of Quine's criticisms of analyticity, see Carnap (1963b, 915–922).

[5] Burks (1963) argued that the concept of inductive probability includes a non-cognitive component and for that reason isn't logical, though he agreed that the cognitive component

There are only two concepts of probability in ordinary language and the other one, physical probability, isn't logical. Therefore, we can characterize inductive probability by saying that it is *the logical probability concept of ordinary language.*

1.4 Rational degree of belief

Authors who recognize that inductive probability is not *actual* degree of belief often identify it with *rational* degree of belief. For example, Keynes (1921, 4) said:

> Let our premisses consist of any set of propositions h, and our conclusion consist of any set of propositions a, then, if a knowledge of h justifies a rational belief in a of degree α, we say that there is a *probability-relation* of degree α between a and h.

So I will now consider this claim:

R: The inductive probability of H given E is the degree of belief in H that would be rational for a person whose evidence is E.

One problem with this is that the term "rational degree of belief" is ambiguous.

In one sense, a degree of belief is rational for a person if it is a good means to the person's goals. If the term is understood in this sense, then R is false. For example, a competitor in a sports event may know that believing he can win will help him perform well; in that case, having a high degree of belief that he can win may be a good means to his goals, even if the inductive probability of winning, given his evidence, is low.

In the preceding example the goal is pragmatic but it makes no difference if we restrict the goals to epistemic ones. For example, a scientist engaged in a difficult research program might know that believing his hypothesis is true will help him conduct successful research; it may then be a good means to his epistemic goals to have a high degree of belief that his hypothesis is true, even if the inductive probability of this, given his evidence, is low.

In another sense (called "deontological"), it is rational for a person to have a particular degree of belief if the person does not deserve blame for having it. Blameworthiness may be assessed in different ways but, to avoid multiplying conceptions of rationality even further, let us suppose that we are here concerned with epistemic blame. If "rational degree of belief" is understood in this sense then R is again false, as the following examples show.

- A person whose evidence is E has a high degree of belief in H but does so for some irrelevant reason—or no reason at all—and not because

of inductive probability is logical. Perhaps he is right about this, in which case my claim that inductive probability is logical should be qualified to say that *the cognitive component of* inductive probability is logical. However, this qualification would not affect any argument I will make, so for simplicity I will ignore it.

the person perceives any real relation between H and E. This person deserves epistemic blame and so it isn't rational (in the present sense) for the person to have a high degree of belief in H. Nevertheless, it may be that the inductive probability of H given E is high.

- A person makes a conscientious effort to determine the inductive probability of H given E but makes a subtle error and comes to the conclusion that the probability is low and as a result has a low degree of belief in H. We can suppose that this person isn't epistemically blameworthy, in which case it is rational (in the present sense) for the person to have a low degree of belief in H. Nevertheless, it may be that the inductive probability of H given E is high.

In yet another sense, a person's beliefs count as rational if they are in accord with the person's evidence. But what does it mean for a degree of belief to be in accord with a person's evidence? The natural suggestion is that X's degree of belief in H is in accord with X's evidence iff[6] it equals the inductive probability of H given X's evidence. If "rational degree of belief" is understood in this sense, then R is trivially true.

Thus, R may be true or false, depending on how one understands the term "rational degree of belief." Therefore, one cannot explain what inductive probability is merely by saying it is rational degree of belief; one would need to also identify the relevant sense of the term "rational degree of belief," which requires identifying the concept of inductive probability in some other way. Thus, calling inductive probability "rational degree of belief" is useless for explaining what inductive probability is.

In addition, the inductive probability of H given E depends only on H and E and not on whether anyone believes either H or E to any degree. Therefore, calling inductive probability "rational degree of belief" involves a gratuitous reference to the concept of belief; inductive probability is no more concerned with belief than is the relation of logical implication.

1.5 Degree of confirmation

Some authors use the term "degree of confirmation" to refer to a logical concept of probability (Hosiasson-Lindenbaum 1940; Carnap 1950; Roeper and Leblanc 1999). So let us consider:

D: The inductive probability of H given E is the degree to which E confirms H.

If "confirm" is being used in its ordinary sense, then D is false. For example, if E is irrelevant to H then we would ordinarily say that E doesn't confirm H to any degree, though the inductive probability of H given E need not be zero. Thus, that the sky is blue is irrelevant to the proposition that

[6] "Iff" is an abbreviation for "if and only if."

$2 = 2$, but the inductive probability that $2 = 2$, given that the sky is blue, is one, not zero.

Carnap (1962, xviii) claimed that "confirms" is ambiguous and he seems to have thought that D is true of one of its senses. I see no good reason to think this word is ambiguous in ordinary language but, in any case, there is no ordinary sense of "confirms" in which the sky being blue confirms that $2 = 2$.

The only way in which D could be true is if "degree of confirmation" is here being used in a non-ordinary sense. But then one cannot use D to explain what inductive probability is without identifying the special sense and doing that requires identifying the concept of inductive probability in some other way. Hence, D is useless for explaining what inductive probability is.

Furthermore, even if we could somehow specify the intended special sense of "degree of confirmation," it would still be misleading to refer to inductive probability this way, since inductive probability is not a meaning of "degree of confirmation" in ordinary language. By contrast, the term "inductive probability" suggests that the concept being referred to is a meaning of the word "probability" in ordinary language, which is correct.

Chapter 2

Physical probability

Having discussed the concept of inductive probability, I now turn to the other probability concept of ordinary language, the concept of physical probability. On page 1 I identified this concept by means of an example but I can now also characterize it as the probability concept of ordinary language that isn't logical; we could call it the *empirical* probability concept of ordinary language. This chapter will present my views on the basic properties of this concept; the views of other authors will be discussed in later chapters.

2.1 Form of statements

Let an *experiment* be an action or event such as tossing a coin, weighing an object, or two particles colliding. I will distinguish between experiment *tokens* and experiment *types*; experiment tokens have a space-time location whereas experiment types are abstract objects and so lack such a location. For example, a particular toss of a coin at a particular place and time is a token of the experiment type "tossing a coin"; the token has a space-time location but the type does not.

Experiments have *outcomes* and here again there is a distinction between tokens and types. For example, a particular event of a coin landing heads that occurs at a particular place and time is a token of the outcome type "landing heads"; only the token has a space-time location.

Now consider a typical statement of physical probability such as:

> The physical probability of heads on a toss of this coin is 1/2.

Here the physical probability appears to relate three things: tossing this coin (an experiment type), the coin landing heads (an outcome type), and 1/2 (a number). This suggests that elementary sentences of physical probability can be represented as having the form "The physical probability of X resulting in O is r," where X is an experiment type, O is an outcome type, and r is a number. I claim that this suggestion is correct.

I will use "$pp_X(O) = r$" as an abbreviation for "the physical probability of experiment type X having outcome type O is r."

2.2 Unrepeatable experiments

The types that I have mentioned so far can all have more than one token; for example, there can be many tokens of the type "tossing this coin." However, there are also types that cannot have more than one token; for example, there can be at most one token of the type "tossing this coin at noon today." What distinguishes types from tokens is not repeatability but rather abstractness, evidenced by the lack of a space-time location. Although a token of "tossing this coin at noon today" must have a space-time location, the type does not have such a location, as we can see from the fact that the type exists even if there is no token of it. It is also worth noting that in this example the type does not specify a spatial location.

This observation allows me to accommodate ordinary language statements that appear to attribute physical probabilities to token events. For example, if we know that a certain coin will be tossed at noon tomorrow, we might ordinarily say that the physical probability of getting heads on that toss is 1/2, and this may seem to attribute a physical probability to a token event; however, the statement can be represented in the form $pp_X(O) = r$ by taking X to be the unrepeatable experiment type "tossing this coin at noon tomorrow." Similarly in other cases.

2.3 Compatibility with determinism

From the way the concept of physical probability is used, it is evident that physical probabilities can take non-extreme values even when the events in question are governed by deterministic laws. For example, people attribute non-extreme physical probabilities in games of chance, while believing that the outcome of such games is causally determined by the initial conditions. Also, scientific theories in statistical mechanics, genetics, and the social sciences postulate non-extreme physical probabilities in situations that are believed to be governed by underlying deterministic laws. Some of the most important statistical scientific theories were developed in the nineteenth century by scientists who believed that *all* events are governed by deterministic laws.

The recognition that physical probabilities relate experiment and outcome *types* enables us to see how physical probabilities can have non-extreme values in deterministic contexts. Determinism implies that, if X is sufficiently specific, then $pp_X(O) = 0$ or 1; but X need not be this specific, in which case $pp_X(O)$ can have a non-extreme value even if the outcome of X is governed by deterministic laws. For example, a token coin toss belongs to both the following types:

X: Toss of this coin.

X': Toss of this coin from such and such a position, with such and such force applied at a such and such a point, etc.

Assuming that the outcome of tossing a coin is governed by deterministic laws, $pp_{X'}(\text{head}) = 0$ or 1; however, this is compatible with $pp_X(\text{head}) = 1/2$.

2.4 Differences with inductive probability

We have seen that inductive and physical probability differ in that the former is logical while the latter is empirical. But they also differ in other ways.

First, although inductive and physical probability are alike in having two arguments, they differ in what those arguments are. The arguments of inductive probability are propositions whereas those of physical probability are an experiment type and an outcome type. Propositions are true or false but experiment and outcome types aren't.

Inductive probabilities appear to exist for practically all pairs of propositions, though in many cases they lack numeric values. By contrast, there are many experiment types X and outcome types O for which $pp_X(O)$ doesn't exist. For example, if X is the experiment of placing a die on a table, in whatever way one wants, and O is that the die is placed with six facing up, then $pp_X(O)$ doesn't exist; it is not merely that this physical probability has an imprecise value but rather that there is no such thing as $pp_X(O)$.

Inductive probabilities often lack numeric values but it seems that if a physical probability exists at all then it has a numeric value; there do not appear to be any cases where a physical probability exists but has a vague value. For example, let X be the experiment of drawing a ball from an urn of unknown composition and let O be the outcome that the ball drawn is white; in such a case, although we don't know the value of $pp_X(O)$, it does have some numeric value.

Since physical probabilities may or may not exist, the question of when they do exist is of fundamental importance for the theory of physical probability. I don't have a full answer to this question but I will now state some principles that give a partial answer.

2.5 Specification

I claim that physical probabilities satisfy the following:

Specification Principle (SP). *If it is possible to perform X in a way that ensures it is also a performance of the more specific experiment type X', then $pp_X(O)$ exists only if $pp_{X'}(O)$ exists and is equal to $pp_X(O)$.*

For example, let X be tossing a normal coin, let X' be tossing a normal coin on a Monday, and let O be that the coin lands heads. It is possible to perform X in a way that ensures it is a performance of X' (just toss the coin on a Monday), and $pp_X(O)$ exists, so SP implies that $pp_{X'}(O)$ exists and equals $pp_X(O)$, which is correct.

It is easy to see that SP implies the following; nevertheless, proofs of all theorems in this chapter are given in Section 2.8.

Theorem 2.1. *If it is possible to perform X in a way that ensures it is also a performance of the more specific experiment type X_i, for $i = 1, 2$, and if $pp_{X_1}(O) \neq pp_{X_2}(O)$, then $pp_X(O)$ does not exist.*

For example, let b be an urn that contains only black balls and w an urn that contains only white balls. Let:

$$\begin{aligned} X &= \text{selecting a ball from either } b \text{ or } w \\ X_b &= \text{selecting a ball from } b \\ X_w &= \text{selecting a ball from } w \\ O &= \text{the ball selected is white.} \end{aligned}$$

It is possible to perform X in a way that ensures it is also a performance of the more specific experiment type X_b, likewise for X_w, and $pp_{X_b}(O) = 0$ while $pp_{X_w}(O) = 1$, so Theorem 2.1 implies that $pp_X(O)$ does not exist, which is correct.

Let us now return to the case where X is tossing a normal coin and O is that the coin lands heads. If this description of X was a complete specification of the experiment type, then X could be performed with apparatus that would precisely fix the initial position of the coin and the force applied to it, thus determining the outcome. It would then follow from SP that $pp_X(O)$ does not exist. I think this consequence of SP is clearly correct; if we allow this kind of apparatus, there is not a physical probability of a toss landing heads. So when we say—as I have said—that $pp_X(O)$ does exist, we are tacitly assuming that the toss is made by a normal human without special apparatus that could precisely fix the initial conditions of the toss; a fully explicit specification of X would include this requirement. The existence of $pp_X(O)$ thus depends on an empirical fact about humans, namely, the limited precision of their perception and motor control.

Physical probabilities for unrepeatable experiments can often be determined by applying SP. For example, let X be tossing a particular coin, let X' be tossing the coin at noon tomorrow, and let O be that the coin lands heads. Since X is repeatable, we may determine $pp_X(O)$ by tossing the coin repeatedly. Since X' can be performed at most once, we cannot determine $pp_{X'}(O)$ by performing it repeatedly; however, it is possible to perform X in a way that ensures it is also a performance of X' (just toss the coin at noon tomorrow), so SP implies that $pp_{X'}(O) = pp_X(O)$.

2.6 Independence

Let X^n be the experiment of performing X n times and let $O_i^{(k)}$ be the outcome of X^n which consists in getting O_i on the kth performance of X. For example, if X is tossing a particular coin and O_1 is that the coin lands heads then X^3 is tossing the coin three times and O_1^2 is that the coin lands heads on the second toss. I claim that physical probabilities satisfy the following:

Independence Principle (IN). *If $pp_X(O_i)$ exists for $i = 1, \ldots, n$ and X^n is a possible experiment then $pp_{X^n}(O_1^{(1)} \ldots O_n^{(n)})$ exists and equals $pp_X(O_1) \ldots pp_X(O_n)$.*

For example, let X be shuffling a normal deck of 52 cards and then drawing two cards without replacement; let O be the outcome of getting two aces. Here $pp_X(O) = (4/52)(3/51) = 1/221$. Applying IN with $n = 2$ and $O_1 = O_2 = O$, it follows that:

$$pp_{X^2}(O^{(1)}O^{(2)}) = [pp_X(O)]^2 = 1/221^2.$$

This implication is correct because X specifies that it starts with shuffling a normal deck of 52 cards, so to perform X a second time one must replace the cards drawn on the first performance and reshuffle the deck, hence the outcome of the first performance of X has no effect on the outcome of the second performance.

For a different example, suppose X is defined merely as drawing a card from a deck of cards, leaving it open what cards are in the deck, and let O be drawing an ace. By fixing the composition of the deck in different ways, it is possible to perform X in ways that ensure it is also a performance of more specific experiment types that have different physical probabilities; therefore, by Theorem 2.1, $pp_X(O)$ does not exist. Here the antecedent of IN is not satisfied and hence IN is not violated.

If X is tossing this coin at noon tomorrow and O is that the coin lands heads then $pp_X(O)$ exists but X^2 isn't a possible experiment and talk of pp_{X^2} seems nonsense. That is why IN includes the proviso that X^n be a possible experiment.

The following theorem elucidates IN by decomposing its consequent into two parts.

Theorem 2.2. *IN is logically equivalent to: if $pp_X(O_i)$ exists for $i = 1, \ldots, n$ and X^n is a possible experiment then both the following hold.*

(a) $pp_{X^n}(O_1^{(1)} \ldots O_n^{(n)})$ exists and equals $pp_{X^n}(O_1^{(1)}) \ldots pp_{X^n}(O_n^{(n)})$.

(b) $pp_{X^n}(O_i^{(i)})$ exists and equals $pp_X(O_i)$, for $i = 1, \ldots, n$.

Here (a) says outcomes of different performances of X are probabilistically independent in pp_{X^n} and (b) asserts a relation between pp_{X^n} and pp_X.

For further elucidation and defense of IN, see Section 8.1.4.

2.7 Direct inference

I will now discuss how physical probability is related to inductive probability. Here and in what follows I will use the notation "$ip(A|B)$" for the inductive probability of proposition A given proposition B.

Let an *R-proposition* be a consistent conjunction of propositions, each of which is either of the form "$pp_X(O) = r$" or else of the form "it is possible to perform X in a way that ensures it is also a performance of X'." Let "Xa" and "Oa" mean that a is a token of experiment type X and outcome type O, respectively. In what follows, "R" always denotes an R-proposition while "a" denotes a token event. Inductive probabilities satisfy the following:

Direct Inference Principle (DI). *If $R.Xa$ implies that $pp_X(O) = r$ then $ip(Oa|Xa.R) = r$.*

For example, let X be tossing a particular coin, let X' be tossing it from such and such a position, with such and such a force, etc., let O be that the coin lands heads, and let R be "$pp_X(O) = 1/2$ and $pp_{X'}(O) = 1$." Then DI implies $ip(Oa|Xa.R) = 1/2$ and $ip(Oa|X'a.R) = 1$. Since $Xa.X'a$ is logically equivalent to $X'a$, it follows that $ip(Oa|Xa.X'a.R) = 1$.

As it stands, DI has no realistic applications because we always have more evidence than just Xa and an R-proposition. However, in many cases our extra evidence does not affect the application of DI; I will call evidence of this sort "admissible." More formally:

Definition 2.1. *If $R.Xa$ implies that $pp_X(O) = r$ then E is* admissible *with respect to (X, O, R, a) iff $ip(Oa|Xa.R.E) = r$.*

The principles I have stated imply that certain kinds of evidence are admissible. One such implication is:

Theorem 2.3. *E is admissible with respect to (X, O, R, a) if both the following are true:*

(a) R implies it is possible to perform X in a way that ensures it is also a performance of X', where $X'a$ is logically equivalent to $Xa.E$.

(b) There exists an r such that R implies $pp_X(O) = r$.

For example, let X be tossing this coin and O that the coin lands heads. Let E be that a was performed by a person wearing a blue shirt. If R states a value for $pp_X(O)$ and that it is possible to perform X in a way that ensures the tosser is wearing a blue shirt, then E is admissible with respect to (X, O, R, a). In this example, the X' in Theorem 2.3 is tossing the coin while wearing a blue shirt.

We also have:

Theorem 2.4. *E is admissible with respect to (X, O, R, a) if both the following are true:*

(a) $E = Xb_1 \dots Xb_n.O_1b_1 \dots O_mb_m$, where $b_1, \dots, b_n$ are token events distinct from each other and from a, and $m \leq n$.

(b) For some r, and some $r_i > 0$, R implies that $pp_X(O) = r$ and $pp_X(O_i) = r_i$, $i = 1, \dots, m$.

For example, let X be tossing this coin and O that the coin lands heads. Let a be a particular toss of the coin and let E state some other occasions on which the coin has been (or will be) tossed and the outcome of some or all of those tosses. If R states a non-extreme value for $pp_X(O)$ then E is admissible with respect to (X, O, R, a). In this example, the O_i in Theorem 2.4 are all either O or $\sim O$.

Theorems 2.3 and 2.4 could be combined to give a stronger result but I will not pursue that here.

2.8 Proofs

Here "Tn" refers to Theorem n.

Proof of Theorem 2.1

Suppose it is possible to perform X in a way that ensures it is also a performance of the more specific experiment type X_i, for $i = 1, 2$. If $pp_X(O)$ exists then, by SP, both $pp_{X_1}(O)$ and $pp_{X_2}(O)$ exist and are equal to $pp_X(O)$; hence $pp_{X_1}(O) = pp_{X_2}(O)$. So, by transposition, if $pp_{X_1}(O) \neq pp_{X_2}(O)$, then $pp_X(O)$ does not exist.

Proof of Theorem 2.2

Assume IN holds, $pp_X(O_i)$ exists for $i = 1, \ldots, n$, and X^n is a possible experiment. By letting O_j be a logically necessary outcome, for $j \neq i$, it follows from IN that $pp_{X^n}(O_i^{(i)})$ exists and equals $pp_X(O_i)$; thus (b) holds. Substituting (b) in IN gives (a).

Now assume that $pp_X(O_i)$ exists for $i = 1, \ldots, n$, X^n is a possible experiment, and (a) and (b) hold. Substituting (b) in (a) gives the consequent of IN, so IN holds.

Proof of Theorem 2.3

Suppose (a) and (b) are true. Since SP is a conceptual truth about physical probability, it is analytic, so R implies:

$$pp_{X'}(O) = pp_X(O) = r.$$

Therefore,

$$\begin{aligned} ip(Oa|Xa.R.E) &= ip(Oa|X'a.R), \quad \text{by (a)} \\ &= r, \quad \text{by DI.} \end{aligned}$$

Thus E is admissible with respect to (X, O, R, a).

Proof of Theorem 2.4

Assume conditions (a) and (b) of the theorem hold. I will also assume that $m = n$; the result for $m < n$ follows by letting $O_{m+1}, \ldots, O_n$ be logically necessary outcomes.

Since IN is analytic, it follows from (b) that R implies that if X^{n+1} is a possible experiment then

$$\begin{aligned} pp_{X^{n+1}}(O_1^{(1)} \ldots O_n^{(n)}.O^{(n+1)}) &= pp_X(O_1) \ldots pp_X(O_n)\, pp_X(O) \\ &= r_1 \ldots r_n r. \end{aligned} \tag{2.1}$$

Using obvious notation, $ip(O_1b_1 \ldots O_nb_n.Oa|Xb_1 \ldots Xb_n.Xa.R)$ can be rewritten as:

$$ip(O_1^{(1)} \ldots O_n^{(n)} O^{(n+1)}(b_1 \ldots b_n a)|X^{n+1}(b_1 \ldots b_n a).R).$$

Since $R.X^{n+1}(b_1 \ldots b_n a)$ implies (2.1), it follows by DI that the above equals $r_1 \ldots r_n r$. Changing the notation back then gives:

$$ip(O_1b_1 \ldots O_nb_n.Oa|Xb_1 \ldots Xb_n.Xa.R) = r_1 \ldots r_n r. \tag{2.2}$$

Replacing O in (2.2) with a logically necessary outcome, we obtain:

$$ip(O_1b_1 \ldots O_nb_n|Xb_1 \ldots Xb_n.Xa.R) = r_1 \ldots r_n. \tag{2.3}$$

Since $r_1 \ldots r_n > 0$ we have:

$$\begin{aligned} ip(Oa|Xa.R.E) &= ip(Oa|Xa.R.Xb_1 \ldots Xb_n.O_1b_1 \ldots O_nb_n) \\ &= \frac{ip(O_1b_1 \ldots O_nb_n.Oa|Xb_1 \ldots Xb_n.Xa.R)}{ip(O_1b_1 \ldots O_nb_n|Xb_1 \ldots Xb_n.Xa.R)} \\ &= r, \quad \text{by (2.2) and (2.3).} \end{aligned}$$

Thus E is admissible with respect to (X, O, R, a).

Chapter 3

Explication

Inductive and physical probability are concepts of ordinary language and, like many such concepts, they are vague and unclear. One way to study such concepts is to reason about them directly and that is what I was doing in the preceding chapters. But there is another way to study them, called *explication*, which was given its classic formulation by Carnap (1950, 3–8). This chapter will discuss that methodology.

3.1 What explication is

Explication begins with a pre-existing vague concept; this is called the *explicandum.* That concept is *explicated* by identifying another concept, called the *explicatum*, that satisfies the following desiderata to a sufficient degree:

- It is clear and precise, not vague like the explicandum.
- It is similar enough to the explicandum that it can be used in place of the latter for some envisaged purposes.
- It permits the formulation of exceptionless generalizations; Carnap called this *fruitfulness.*
- It is as simple as is compatible with satisfying the preceding desiderata.

Before trying to explicate a concept it is important to be clear about *which* concept one is trying to explicate. This process is called *clarification of the explicandum*; it is not the same as explication because we are here only identifying which vague concept is our explicandum, not specifying a precise concept. So, for example, an explication of probability ought to begin by distinguishing the different senses of the word "probability" and identifying which of these is the explicandum. Clarification of the explicandum cannot be done by giving a precise definition of the explicandum, since the explicandum is not precise; it is rather done by giving examples of the use of the concept and perhaps by giving general characterizations of the concept, as I did for inductive and physical probability in Chapters 1 and 2.

3.2 Concepts, not terms

I have said that explicanda and explicata are *concepts*. Carnap, on the other hand, allowed that they could instead be *terms* (that is, words or phrases); for example, he wrote:

> We call the given concept (or the term used for it) the *explicandum*, and the exact concept proposed to take the place of the first (or the term proposed for it) the *explicatum*. (1950, 3)

However, terms are often ambiguous, that is, they express more than one concept; "probability" is an example. One of the main tasks in clarifying the explicandum is to distinguish these different meanings and identify the one that is the explicandum. Therefore, what is being explicated is really a concept, not a term.

Carnap gave the following example of clarifying an explicandum:

> I might say, for example: ... "I am looking for an explication of the term 'true', not as used in phrases like 'a true democracy', 'a true friend', etc., but as used in everyday life, in legal proceedings, in logic, and in science, in about the sense of 'correct', 'accurate', 'veridical', 'not false', 'neither error nor lie', as applied to statements, assertions, reports, stories, etc." (1950, 4–5)

Carnap here says the explicandum is a term "as used in" a particular way, but to talk of a term "as used in" a particular way is just a misleading way of talking about a concept. Thus Carnap here unwittingly acknowledged that the explicandum is a concept, not a term.

Quine (1960, 257) and Hanna (1968, 30) took explicanda to be terms and not concepts; they chose the wrong one of Carnap's two alternatives, ignoring the fact that terms are often ambiguous.

3.3 Strawson on relevance

Strawson criticized the methodology of explication, saying:

> It seems prima facie evident that to offer formal explanations of key terms of scientific theories to one who seeks philosophical illumination of essential concepts of non-scientific discourse, is to do something utterly irrelevant—is a sheer misunderstanding, like offering a text-book on physiology to someone who says (with a sigh) that he wished he understood the workings of the human heart. (Strawson 1963, 505)

Carnap replied that explication can solve philosophical problems arising in ordinary language because it gives us improved new concepts that can serve the same purposes as the ordinary concepts that created the puzzles; the problems are solved by using the new language instead of ordinary language in the problematic contexts. Carnap gave the following analogy:

> A natural language is like a crude, primitive pocketknife, very useful for a hundred different purposes. But for certain specific purposes, special tools are more efficient, e.g., chisels, cutting machines, and finally the microtome. If we find that the pocket knife is too crude for a given purpose and creates defective products, we shall try to discover the cause of the failure, and then either use the knife more skillfully, or replace it for this special purpose by a more suitable tool, or even invent a new one. [Strawson's] thesis is like saying that by using a special tool we evade the problem of the correct use of the cruder tool. But would anyone criticize the bacteriologist for using a microtome, and assert that he is evading the problem of correctly using the pocketknife? (Carnap 1963b, 938–939).

Of course, nobody would criticize the bacteriologist, but that is because the bacteriologist's problem was not about the pocketknife. However, the relevant analogy for "one who seeks philosophical illumination of essential concepts of non-scientific discourse" is someone who seeks knowledge of proper use of the pocketknife; Carnap has offered nothing to satisfy such a person.

Carnap seems to have thought that we don't need to take problems about ordinary language very seriously because, when such problems arise, we can develop a new more precise language that serves the same purposes and avoids the problems. But in many cases our purpose is to resolve a problem about a concept of ordinary language, and Carnap has not indicated how a new more precise language can serve that purpose.

For example, suppose our purpose is to determine whether some evidence E raises the inductive probability of a hypothesis H. This problem concerns a concept of probability in ordinary language. We could construct a new more precise language, with a mathematically defined function that is intended to be an explicatum for the ordinary concept of inductive probability. But in order for this new more precise language to serve our purposes, it must enable us to determine whether E raises the inductive probability of H; Carnap has not explained how the new language could do that.

Furthermore, a good explicatum needs to be sufficiently similar to the explicandum that it can be used for the same purposes, and to determine whether this is the case the explicator must understand the explicandum well. Therefore, an explicator cannot dismiss problems about ordinary language.

So Carnap's response to Strawson was insufficient. I will now propose a better response. Suppose our problem is to determine whether or not some sentence S of ordinary language is true. If we apply the method of explication to this problem, we will construct explicata for the concepts in S, formulate a corresponding sentence S' using these explicata, and determine whether or not S' is true. This does not *by itself* solve the original problem—that is

Strawson's point—but it can greatly assist in solving the problem, in three ways. (1) The attempt to formulate S' often shows that the original sentence S was ambiguous or incomplete and needs to be stated more carefully. (2) If the explicata appearing in S' are known to correspond well to their explicanda in other cases, that is a reason to think that they will correspond well in this case too, and hence to think that the truth value of S will be the same as that of S'. (3) We can translate the proof or disproof of S' into a parallel argument about the corresponding explicanda and see if this seems to be sound; if so, we obtain a direct argument for or against S. In these ways, explication can provide insights and lines of argument that we may not discover if we reason only in terms of the vague explicanda.

Here is an illustration of these points. Nicod (1923, 189) claimed that a law of the form "All F are G" is made more probable by evidence that something is both F and G. Suppose our problem is to determine whether this is correct. Following Nicod, let us use the term "confirms" to mean "raises the probability of"; thus our problem becomes whether a law of the form "All F are G" is confirmed by evidence that something is both F and G. If we attempt to explicate the concept of confirmation we soon realize that whether or not evidence E confirms hypothesis H depends not only on E and H but also on the background evidence, something that Nicod neglected to specify. If we specify that we are interested in the case where there is no background evidence, then Nicod's claim becomes:

N. A law of the form "All F are G" is confirmed by evidence that something is both F and G, given no background evidence.

Hempel (1945) argued that N is true and Good (1968) argued that it is false. Maher (2004) applied the method of explication to N; I defined an explicatum C for confirmation (71), formulated an analog of N using C—let us call this N'—and proved that N' is false (77). This does not by itself show that N is false. However, I had argued that C corresponds well with the concept of confirmation in other cases, which is a prima facie reason to think that there is correspondence here too, and hence that N is also false. Furthermore, I showed (78) that the proof that N' is false makes intuitive sense when translated back into qualitative explicandum terms. Thus the method of explication provides us with a good argument that the ordinary language hypothesis N is false.

Strawson seems to concede that explication can be useful in something like the ways I have indicated. He wrote:

> I should not wish to deny that in the discharge of this task [resolving problems in unconstructed concepts], the construction of a model object of linguistic comparison may sometimes be of great help. (Strawson 1963, 513)

But if explication can "be of great help" then it is *not* "like offering a text-book on physiology to someone who says (with a sigh) that he wished he understood the workings of the human heart."

3.4 Boniolo on definitions

Explication has received little attention in the literature since the 1960s, but two authors have recently published criticisms of it. One of these is Boniolo, who believes that an explication proceeds by giving a definition and argues that this is an inappropriate method for philosophers to use. He says:

> If a philosopher defined, he would construe the concept with all of its notes *ab initio*. But, in such a way he would bar his own chances to investigate whether the aspects upon which to dwell have been fixed at the beginning. Moreover, the philosopher who wants to ape the mathematician in using definitions instead of [discursive analyses] runs the risk of believing that his definitions are right when they may in fact be wrong. Conversely, the philosopher who [discursively analyzes] is well aware that his [analyses] may be wrong and incomplete and in such a way, during his analysis, he can suitably modify them. (Boniolo 2003, 297)

Although Boniolo makes other negative remarks about Carnap and explication, I believe the above passage contains his main substantive objection to (Carnapian) explication.

The first thing to say about this is that an explication need *not* involve giving a definition, at least not if "definition" is understood in the ordinary sense that Carnap uses. Carnap (1950, 3) said that "the explicatum must be given by explicit rules for its use, *for example*, by a definition" (emphasis mine). The alternative to defining the explicatum is to give rules for its use that do not allow it to be eliminated in sentences that contain it; in this case the explicatum is treated as a "theoretical concept" (**?**).

But let us now consider the case in which an explication does involve a definition. It is important to observe that in this case, what is defined is the explicatum, not the explicandum. So "if the explication consists in giving an explicit definition, then both the definiens and the definiendum in the definition express the explicatum, while the explicandum does not occur" (Carnap 1950, 3). For example, the explicandum in (Carnap 1950) was the ordinary language concept of inductive probability, which Carnap called "degree of confirmation" and "probability$_1$" (25); his explicatum was a function that he called c^* (562). Carnap specified c^* by giving a definition that specified its values for all possible arguments; this is a stipulative definition that specifies what is meant by "c^*." Carnap tried to *clarify* his explicandum but did not try to *define* it.

So when explication is done by giving a definition, the definition is stipulative—it specifies what the explicatum is—and consequently there is no possibility of the definition being wrong. Therefore, the philosopher who explicates by giving a definition runs no "risk of believing that his definitions are right when they may in fact be wrong," contrary to what Boniolo asserts.

Although explications cannot fail in the way Boniolo supposes, they can fail in other ways, of course. A purported explication sometimes fails because the explicator has failed to distinguish different concepts that might be intended as the explicandum. It may also fail because the explicatum differs from the explicandum in ways that prevent the former being used in place of the latter. However, nothing in the method of explication precludes critical consideration of these issues; in fact, there are many critical discussions of just these issues in Carnap's own work. Hence Boniolo is mistaken in thinking that the method of explication is inimical to the recognition of errors.

3.5 Eagle on conceptual clarification

The other recent critic of explication is Eagle, who writes:

> Carnap [1950] has a long discussion of what he calls "explication" of a pre-theoretical concept in terms of a scientifically precise concept. He gives a number of criteria: that the proposed explicatum (i) be sufficiently similar to the original concept to be recognizably an explication of it; (ii) be more exact or precise, and have clear criteria for application; (iii) play a unified and useful role in the scientific economy (so that it is not just gerrymandered and accidental); and (iv) be enmeshed in conceptual schemes simpler than any other putative explication that also meets criteria (i)–(iii). These are good constraints to keep in mind. However, this model is altogether too compressed; for it presumes that we have an independently good analysis of the scientifically precise concept (in effect, it suggests that scientific theories are not in need of conceptual clarification—that the "clear conditions of application" are sufficient for conceptual understanding). (Eagle 2004, 372)

If the term "scientific theories" is being used in its ordinary sense, then it is undeniable that scientific theories are often in need of conceptual clarification, but that is because these theories often contain concepts that are vague and lack explicit rules governing their use. For example, there are biological theories that contain the vague concept of a species. Such vague concepts are suitable targets for Carnap's methodology of explication and so it is wrong to say that Carnap's model "suggests that scientific theories are not in need of conceptual clarification." Carnap himself said that the explicandum "may belong to ... a previous stage in the development of scientific language" (1950, 3).

So I take the real issue to be this: Eagle thinks a "scientifically precise concept" which has "clear conditions of application" may nevertheless require an "analysis" or "conceptual clarification" before we can have "conceptual understanding" of it; Carnap believed that if a concept is specified by "explicit rules for its use" then it requires no further clarification. Carnap's position here accords with the widely shared idea that knowing how to use a term is a sufficient condition for knowing what the term means. How can Eagle deny this?

Just before the passage of Eagle's quoted above, Eagle gave two examples of what he has in mind. The first concerns probability; Eagle writes:

> We wish to find an analysis of probability that makes the scientific use an explication of the pre-scientific use; but this project should not be mistaken for the project of discovering a scientific concept of probability [i.e., an explicatum]. The second task had been performed exactly when we identified scientific probabilities with normed additive measures over the event spaces of scientific theories. But to make this formal structure conceptually adequate we need to give an analysis of both the explicandum and the explicatum. (Eagle 2004, 372)

To say that a function p is a "normed additive measure over the event spaces of scientific theories" (i.e., to say it satisfies the mathematical laws of probability) is not enough to give the "explicit rules for its use" that Carnap required of an explicatum. The laws of probability leave the values of p completely indeterminate except for a few special cases (e.g., the probability of a logical truth is one), whereas "explicit rules for its use" must tell us under what conditions a sentence like "$p(H|E) = r$" is true. Thus Carnap's specification of the function c^*, which was his explicatum for probability$_1$, does not say merely that c^* satisfies the mathematical laws of probability; Carnap fixed c^* uniquely by specifying all its values. And it would make no sense to try to give a "conceptual clarification" of c^*; the function is just what it is defined to be.

Eagle's second example concerns Kripke semantics for modal logic. Eagle thinks that this semantics provides an explication that requires "philosophical attention." But Kripke semantics for modal logic also fails to meet Carnap's criterion of having "explicit rules for its use;" it does not contain rules that determine which claims about possible worlds are true. And if we had such rules, there would be no room for further "conceptual clarification."

So Eagle's belief that explicata require "conceptual clarification" rests on a misunderstanding of the concept of an explicatum. When we understand the concept correctly, we can see that there is no room for further "conceptual clarification" of an explicatum.

Eagle presents himself as being more demanding than Carnap, requiring not just that an explicatum be specified but also that it be given a "conceptual clarification" or "philosophical interpretation." It is unclear to me what Eagle means by the latter phrases, but from his examples I gather that he does not require the formulation of explicit rules for the use of the concept. Carnap, on the other hand, required that an explicatum be given by stating such rules. So it is really Carnap, not Eagle, who has the higher standard of what philosophical analysis requires.

3.6 Eagle on elimination

After the passage just discussed, Eagle makes another criticism of explication. He says of this method:

> It also suggests that the explicatum replace or eliminate the explicandum; and that satisfying these constraints is enough to show that the initial concept has no further importance. But clearly the relation between the scientific and pre-scientific concepts is not so one-sided; after all, the folk are the ones who accept the scientific theories, and if the theory disagrees too much with their ordinary usage, it simply won't get accepted. I take this kind of approach to philosophical analysis to be *pragmatist* in some broad sense; it emphasizes the conceptual needs of the users of scientific theories in understanding the aims and content of those theories. (372–373)

Eagle's assertion that "the folk are the ones who accept the scientific theories" seems obviously false and the "pragmatist" approach that Eagle endorses is consistent with Carnap's views on explication. But I think that the earlier part of this passage does raise a plausible objection to the method of explication.

I would put the objection this way: Carnap (1950, 3) talked of the explicatum "replacing" the explicandum and Quine (1960, 260) said "explication is elimination."[1] This suggests that a successful explication renders the explicandum of "no further importance," as Eagle says. Yet in most cases, explications do not have this effect. For example, the ordinary concept of inductive probability continues to be important despite the various explications of it, and it is utterly unrealistic to suppose that any future explicatum will make this ordinary concept disappear. It is neither possible nor desirable to replace statements like "John will probably be late" with some precise quantitative explicatum.

But when Carnap said an explicatum "replaces" the explicandum, he did not mean that it does so in all contexts, only that it does so in particular contexts for which the explicatum is designed. This is shown by the following quotations from Carnap's reply to Strawson (Carnap 1963b, emphases mine):

> An explication replaces the imprecise explicandum by a more precise explicatum. Therefore, *whenever greater precision in communication is desired*, it will be advisable to use the explicatum instead of the explicandum. (935)

> [A scientific explicatum] will frequently be accepted later into the everyday language, such as "at 4:30 P.M.", "temperature", "speed"

[1] Carus (2007, 265) argues that Quine's conception of explication is fundamentally different to Carnap's but I can't follow his reasoning. Certainly Quine's statement that "explication is elimination" is consistent with Carnap's talk of the explicatum "replacing" the explicandum.

> as a quantitative term. *In other cases, the explicatum is chiefly used in technical, scientific contexts.* (936)
>
> The constructionist [one who explicates concepts] may ... propose to use, *in certain philosophical contexts (not in contexts of everyday life)*, certain words of everyday language according to certain rules (e.g., to use the word "or" only in the non-exclusive sense). (937)
>
> A natural language is like a crude, primitive pocketknife, very useful for a hundred different purposes. But *for certain specific purposes*, special tools are more efficient. (938)

Strawson already understood this point, writing that:

> A pre-scientific concept C is clarified in [Carnap's] sense if it is *for certain purposes* replaced (or supplanted or succeeded) by a concept C' which is unlike C in being both *exact* and *fruitful*. (Strawson 1963, 504, emphases in original)

Since an explicatum is only intended to replace the explicandum in certain contexts and for certain purposes, explication does *not* aim to make the explicandum "of no further importance."

Chapter 4

Laplace's classical theory

Pierre-Simon Laplace (1749–1827) was a French scientist who made important contributions to mathematics, astronomy, and probability theory. This chapter will not attempt to do justice to Laplace's many achievements but will merely discuss his views on the nature of probability.

4.1 Two kinds of probability

Probability theory, according to Laplace, is concerned with determining the probabilities of compound events from given probabilities of simple events. Laplace often, but not always, referred to the probabilities of the simple events as *possibilities*. He distinguished two kinds of these possibilities, which he called *absolute* and *relative* possibilities, a distinction that corresponds to my distinction between physical and inductive probabilities.

> In the analysis of chances, one aims to find the probabilities of events composed, according to a given law, of simple events with given possibilities. These possibilities may be determined in the following three ways: (1) *a priori*, when from the nature of the events one sees that they are possible in a given ratio; for example, in tossing a coin, if the coin is homogeneous and its two faces are entirely alike, we judge that heads and tails are equally possible; (2) *a posteriori*, by making many repetitions of the experiment that can produce the event in question and seeing how often the event occurs; (3) finally, by considering the reasons which may determine us to pronounce on the existence of the event; for example, if the skills of two players A and B are unknown, since we have no reason to suppose A stronger than B, we conclude that the probability of A winning a game is 1/2. The first method gives the absolute possibility of the events; the second gives approximate knowledge of it, as we will show in what follows, and the third gives only their possibility relative to our knowledge. (1781, 384–85)

Laplace accepted determinism and took it to imply that probability is relative to our knowledge. However, he maintained that this does not prevent there being absolute as well as relative possibilities.

> Every event being determined by the general laws of the universe, there is only probability relative to us and, for this reason, the distinction between absolute and relative possibility may appear imaginary. But one must observe that, among the circumstances that concur in the production of events, some change at every moment, such as the movement that the hand imparts to dice, and it is the union of these circumstances that one calls *chance*. There are others that are constant, such as the ability of the players, the tendency of the dice to fall on one face rather than the others, etc.; these form the *absolute possibility* of events and knowledge of them that is more or less incomplete forms their *relative possibility*. (1781, 385)

Laplace claimed that probabilities based on relative possibilities do not obey the same rules as those based on absolute possibilities and that previous probability theorists overlooked this.

> The work done up to now in the theory of chance assumes knowledge of the absolute possibility of events and, with the exception of some remarks that I have given in [earlier papers], I do not know that anyone has considered the case in which only their relative possibility is known. This case contains many interesting questions and is relevant to most problems concerning games. The reason mathematicians have not paid particular attention to this is presumably that they thought the same methods applied to it as to the case where the absolute possibility of the events is known. However, the essential difference between these possibilities can significantly alter the results of calculations, so that one is often exposed to considerable errors if one employs them in the same manner. (1781, 385–86)[1]

The crucial difference is that, if the absolute possibility of the events is given, then the events are independent (the probability of a conjunction of events is the product of the probabilities of each); when only their relative possibility is given, events are not in general independent. Laplace gave the following example:

> Suppose two players A and B, whose skills are unknown, play some type of game, and let us find the probability that A will win the first n games.

[1] An earlier statement of these points, without the terminology of relative and absolute possibility, is in (1774a, 61–62).

> For a single game, clearly A or B must win it and these two events are equally probable, so the probability of the first is 1/2. From this, following the ordinary rule of the analysis of chance, one concludes that the probability of A winning the first n matches is $1/2^n$. This conclusion would be correct if the probability 1/2 was based on an absolute equality between the possibilities of the two events; but there is only equality relative to our ignorance of the skill of the two players and this equality does not preclude one being a stronger player than the other. So let us suppose that $(1+\alpha)/2$ is the probability of the stronger player winning a game and $(1-\alpha)/2$ is that of the weaker. Letting P denote the probability that A will win the first n games, we have
>
> $$\mathrm{P} = \frac{1}{2^n}(1+\alpha)^n \qquad \text{or} \qquad \mathrm{P} = \frac{1}{2^n}(1-\alpha)^n,$$
>
> depending on whether A is the stronger or the weaker player. Then, since we have no reason for one supposition rather than the other, it is evident that to get the true value of P, one must take half the sum of the two preceding values, which gives
>
> $$\mathrm{P} = \frac{1}{2^{n+1}}[(1+\alpha)^n + (1-\alpha)^n].$$
>
> Expanding this expression gives
>
> $$\mathrm{P} = \frac{1}{2^n}\left[1 + \frac{n(n-1)}{1.2}\alpha^2 + \frac{n(n-1)(n-2)(n-3)}{1.2.3.4}\alpha^4 + \dots\right].$$
>
> This value of P is greater than $1/2^n$ when n is greater than one. (1781, 386–87)

Here "P" denotes physical probability in the first (disjunctive) displayed equation but it denotes inductive probability in the second and third equations. Laplace called the latter probability "the true value of P," meaning that it is the inductive probability relative to the knowledge actually available.

Critics have accused Laplace of not clearly recognizing the distinction between physical and inductive probability.[2] The true situation is the opposite of what these critics claim, since Laplace insisted on the importance of the distinction between the two kinds of probability and was, so far as he knew, the first to have done so. Nevertheless, Laplace's account of these two concepts is not completely correct, as I will now show.

[2] Hacking (1975, ch. 14) claimed that Laplace used the word "possibility" to obscure the distinction between the two kinds of probability; apparently he didn't know that Laplace distinguished two kinds of possibility. Daston (1988, 189) said Laplace "apparently saw no opposition, no choice to be made" between the two concepts; we have seen that this is the opposite of the truth. Gillies (2000, 21) said that since Laplace accepted determinism, his talk of unknown probabilities must be "a slip or inconsistency'; apparently he didn't know that Laplace argued there is no inconsistency.

Laplace said that probability is "relative to us," by which I think he meant that it is relative to what we know. This is plainly not correct for physical probability, since the value of a physical probability depends on facts that may be unknown. What is true is that physical probability is relative to an experiment type and usually this type will not specify circumstances "such as the movement that the hand imparts to the dice," but relativity to an experiment type is not relativity to us. Incidentally, Laplace's distinction between constant and variable causes implies this relativity to an experiment type, since "constant causes" can only mean causes that are present in every token of a type; the bias of a die is not a constant cause if the die can be changed from trial to trial. Similarly, when Laplace spoke of "making many repetitions of the experiment that can produce the event in question," what is being repeated is an experiment *type*.

Inductive probability is also not "relative to us." What is true is that, when we don't specify the evidence to which an inductive probability is relative, we often mean it to be the evidence we possess. However, we can also speak of inductive probabilities given "evidence" that doesn't coincide with our evidence, as Laplace did in many examples. Perhaps Laplace thought of the latter probabilities as relative to some imaginary person who has this evidence, but that person is not "us" and furthermore, nothing about this person is relevant except the proposition that we imagine to be the person's evidence. Thus inductive probability is really a function of two propositions and has nothing essentially to do with any person's knowledge. (For this reason, it is a mistake to call inductive probability "epistemic probability," as many writers do.) Even Laplace's "intelligence ... for whom nothing would be uncertain" (1820, vi–vii) can recognize that $ip(H|E)$ has non-extreme values for many H and E.

4.2 The Rule of Succession

Laplace stated the following rule for the probability of a future event:

> An event having occurred any number of times in succession, the probability that it will occur again the next time is equal to this number increased by one, divided by the same number increased by two. (1820, xvii)

In other words, given only that an event has occurred n times in succession, the probability that it will occur next time is $(n + 1)/(n + 2)$. Venn (1888) dubbed this "the Rule of Succession." Laplace gave the following example:

> If we put the oldest epoch of history at 5,000 years or at 1826213 days,[3] and the sun having risen constantly in this interval during

[3] Laplace took into account that there is a leap year every 4 years, except for centennial years that are not divisible by 400.

> each 24 hour period, the odds are 1826214 to 1 that it will rise again tomorrow. (1820, xvii)

In other words, the probability that the sun will rise tomorrow, given only that it has risen every day for 5,000 years, is $(1826213 + 1)/(1826213 + 2) = 0.9999995$.

Laplace gave a justification for this rule which began with the following assumption:

> When the probability of a simple event is unknown, one may equally suppose it to have all values from zero to one. (1820, xvii)

The probability that is "unknown" here is a physical probability. The "event" that has "occurred any number of times" is an outcome type; I will call it O. The "times" on which the event could have occurred are tokens of an experiment type; I will call this type X. In Laplace's example, X is the passage of 24 hours (from midnight to midnight, say)[4] and O is the sun rising. In this terminology, what Laplace assumes in the preceding quotation is that $pp_X(O)$ exists and that the a priori inductive probability distribution of $pp_X(O)$ is uniform on $[0, 1]$.[5] Laplace gave a proof that the rule follows from these assumptions and the laws of probability, but his proof tacitly assumes principles that he did not articulate, such as IN and DI. In Section 4.4 I give a more complete derivation which shows explicitly where these principles are used.

Although Laplace's derivation of the Rule of Succession can be made rigorous, neither of the assumptions on which it rests is true in general. First, $pp_X(O)$ does not exist for every experiment type X and outcome type O, so one cannot simply assume its existence for any arbitrary X and O, as Laplace did. Second, even if we are given that $pp_X(O)$ exists, it is not true in general that the a priori inductive probability distribution of $pp_X(O)$ is uniform on $[0, 1]$. For example, if O_1, O_2, and O_3 are three incompatible possible outcomes of X and if the a priori inductive probability distribution of $pp_X(O_i)$ were uniform on $[0, 1]$ for each i, then the a priori inductive probability of each O_i would be $1/2$, which violates the laws of probability.

These observations show that the Rule of Succession is not as widely applicable as Laplace supposed. On the other hand, Laplace's assumptions were unnecessarily restrictive in one respect. For example, suppose an urn contains m balls, each ball being either white or black. Let X be drawing a ball from this urn and let O be that the ball drawn is white. Here $pp_X(O)$ exists but the only values it can have are 0, $1/m$, …, 1; Laplace's assumption of a uniform inductive probability distribution on $[0, 1]$ doesn't hold here. Nevertheless, if

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[4] Laplace speaks of "each revolution of 24 hours," which is unclear but might be intended to mean from midnight to midnight. We can't take just any starting point for the 24 hour periods because, for half the year, there are more than 24 hours between successive sunrises.

[5] This means that the a priori inductive probability that $0 \leq pp_X(O) \leq r$ is r, for all $r \in [0, 1]$.

we suppose that the $m + 1$ possible values of the physical probability all have the same inductive probability given just the stated information, the Rule of Succession again follows, as Prevost and Lhuilier showed in 1799 (Hald 1998, 262–63).

4.3 Definition of probability

Laplace gave a definition of probability which is often cited as a paradigm of "the classical definition of probability."

4.3.1 Statements of the definition

Laplace's first statement of this definition is:[6]

> The probability of the existence of an event is ... the ratio of the number of favorable cases to that of all the possible cases, when we do not see any reason for one of these cases to occur rather than the other. It can therefore be represented by a fraction of which the numerator is the number of favorable cases and the denominator is that of all the possible cases. (1776, 146)

Many years later he restated this definition in similar words:

> The probability of an event is the ratio of the number of cases favorable to it to the number of all possible cases, provided nothing leads us to believe that one of these cases must occur rather than the others, which makes them, for us, equally possible. (1820, 181)

The last remark, that the cases are "for us, equally possible," is a side remark that is not part of the definition.

The version of Laplace's definition that is usually cited is the one in his *Philosophical Essay on Probabilities*. Here Laplace defined probability as "the ratio of the number of favorable cases to that of all possible cases" provided that "the various cases are equally possible" (1820, xi). Since he had earlier explained that "equally possible cases" are "such that we can be equally undecided about their existence" (1820, viii), this version of the definition is equivalent to the preceding ones but more convoluted.

4.3.2 Evidence that it is a definition

Laplace's definition of probability has often been criticized as patently defective; Hacking (1975, 122) called it "monstrous" and said "its inadequacy seems evident to us." So it will be worthwhile to consider whether Laplace really

[6] Hacking (1975, 131), Gillispie (1997, 12), and Hald (1998, 157) say that Laplace gave an earlier definition of probability in (1774b, 10–11). However, the "principle" that Laplace stated there was not called a definition by him and it would be circular as a definition, so I take it to be an axiom, not a definition.

meant it to be a definition (that is, a statement of the meaning of "probability"), or whether he only meant it to be a principle that probabilities satisfy. I shall argue that he did mean it as a definition.

One piece of evidence is that Laplace repeatedly called it a definition. In (1776, 145) he says he is going to "fix the meaning" of the word *probability*, then gives the definition I quoted above (1776, 146), and a couple of paragraphs later says that he has just "defined" probability. He also called each of the later formulations "the very definition of probability" (1820, ix, 181).

In the main body of his major treatise on probability, Laplace's statement of his definition is immediately followed by this rule:

> If all the cases are not equally possible, we will determine their respective possibilities and then the probability of the event will be the sum of the probabilities of each favorable case. (1820, 181)

There is a parallel statement in the *Essay* (1820, xi). If Laplace's "definition" is really a definition, these unequally possible cases must be subdividable into equally possible ones. The following facts provide strong evidence that Laplace did believe unequally possible cases can always be subdivided into equally possible ones.

1. After stating the rule for unequally possible cases, quoted above, Laplace said "this probability is relative to the subdivision of all the cases into others that are equally possible" (1820, 181). He then gave a proof of the rule which assumes a subdivision into equally possible cases.

2. In the *Essay*, Laplace illustrated the rule with an example in which the unequal possibilities are evaluated by subdividing one of them to obtain equally possible cases (1820, xi–xii).

3. Laplace gave what he called a "general demonstration" of the multiplication law of probability which assumed the events involved could be subdivided into equally possible cases (1820, 182–83).

I conclude that Laplace accepted what his definition of probability implies, namely, that an event has a probability only if it is a disjunction of elements of some partition of equally probable events.

Overall, then, the evidence firmly supports the view that Laplace seriously intended his "definition" of probability as a definition.

4.3.3 Nature of the definition

Laplace's definition is evidently intended to correspond to what "probability" ordinarily means. Furthermore, since his definition doesn't require the cases to have the same absolute possibility, but only to be such that we "do not see any reason for one of these cases to occur rather than the other," the concept that it is intended to correspond to is inductive probability. Since Laplace thought that all probability is "relative to us," he may have thought that a definition of inductive probability would include physical probability as a special case, but since this is incorrect, and Laplace is not clearly committed to it, I will focus just on inductive probability in what follows.

We must now decide whether to regard Laplace's definition as a descriptive definition of inductive probability or as an explication of this concept. It does not appear to me that Laplace distinguished these things but what we regard as the strengths or weaknesses of his definition will differ depending on which of these interpretations we adopt. For example, the fact that Laplace's definition implies probabilities always have precise numeric values would be a weakness if we regard it as a descriptive definition but not if we regard it as an explication.

I find it more interesting to evaluate Laplace's definition as an explication, rather than as a descriptive definition. I will therefore interpret Laplace as giving a stipulative definition of a concept, which he calls "probability," that is meant to be an explicatum for an ordinary language concept that is also called "probability." To avoid this equivocation I will introduce the symbol "p_L" to denote the concept that is defined by Laplace's definition. The following is then a more explicit statement of Laplace's definition. (Here A, B_i, and C are any propositions.)

Definition 4.1. *$p_L(A|C) = r$ iff (1) there exists a partition $\{B_1, \ldots, B_n\}$ of C such that someone whose total evidence is C has no reason to expect one B_i rather than another, (2) A is logically equivalent, given C, to a disjunction of m of the B_i, and (3) $r = m/n$.*

In this terminology, I am taking Laplace to have proposed p_L as an explicatum for inductive probability.

4.3.4 Evaluation of the definition

Laplace's definition of probability has often been criticized as circular. However, since Definition 4.1 is a stipulative definition of the new term p_L, it is plainly not circular.[7]

Another criticism of Laplace's definition is that it is inconsistent, in the sense that it assigns different probabilities to the same propositions on the same evidence. For example, Hájek (2007a), adapting an example of van Fraassen (1989), argues essentially as follows: Let C be that a particular body is a cube whose sides have a length between 0 and 1 foot. Let A be that the length of the sides is between 0 and 1/2 a foot. Since C gives us no reason to expect A rather than not-A, Definition 4.1 gives $p_L(A|C) = 1/2$. However, C implies that each face of the cube has an area between 0 and 1 square feet,

[7] Even if we regarded Laplace's definition as a descriptive definition of the ordinary word "probability," it would still not be circular, since "probability" does not appear in the definiens. The arguments that it is circular (Hájek 2007a gives the most sophisticated one I have seen) only show that Laplace's definition doesn't reduce probability to non-probabilistic concepts, but a definition can be non-reductive without being circular.

and C gives us no reason to expect this area to be in one rather than another of the intervals $[0, 1/4]$, $[1/4, 1/2]$, $[1/2, 3/4]$, and $[3/4, 1]$. Since A is true iff the area is in the interval $[0, 1/4]$, Definition 4.1 now gives $p_L(A|C) = 1/4$. By considering the volume instead of the area we can similarly get $p_L(A|C) = 1/8$.

Laplace didn't discuss this argument but he did say that it has been a common error to take alternatives as equally possible when they aren't and he gave examples where this has happened (1776, 149); he also said that the correct determination of which cases are equally possible is "one of the most delicate points in the analysis of chances" (1820, 181). So Laplace might say that the conflicting judgments of equal possibility in the preceding paragraph are not all correct. He could say that only one of the partitions (e.g., the one based on length) really gives cases that are equally possible. Alternatively, he could say that the existence of the different partitions means that none of them gives equally possible cases, in which case $p_L(A|C)$ would be undefined rather than multi-valued. However, if Laplace's definition is defended along these lines then we have to admit that it is unclear when we have "no reason to expect one case rather than another," whereas a good explicatum should be clear and precise. We would then have to agree with Cramér (1966, 16) that "the classical definition of probability cannot be considered satisfactory, as it does not provide any criterion for deciding when ... the various possible cases may be regarded as symmetric or equally possible." I conclude that Laplace's definition is either contradictory or else unacceptably vague.

Another common criticism of Laplace's definition is that it is too narrow because there are numeric probabilities that aren't reducible to equally possible cases. The examples of this that have been offered in the literature are physical probabilities, such as the physical probability of a biased coin landing heads or of a newborn child being a boy (von Mises 1957, 69; Salmon 1967, 66). Since we are here considering whether p_L is a good explicatum for *inductive* probability, examples of that sort aren't immediately relevant to the question we are considering. Nevertheless, there is a corresponding problem with inductive probability, as I will now show. Let C be the proposition that a coin is about to be tossed and that the physical probability of this coin landing heads is 0.493. Letting A be that the toss in question will land heads, DI implies $ip(A|C) = 0.493$. However, there does not appear to be any partition of C of the kind required by Definition 4.1, so it appears that $p_L(A|C)$ is undefined. Thus p_L is undefined in some cases in which inductive probability has a numeric value. Furthermore, these cases are important in applications since they serve as the link between inductive and physical probability.

I turn now to a criticism of Laplace's definition that I haven't seen made before. It seems clear from Definition 4.1 that if $p_L(A|C)$ exists and has a unique value then $ip(A|C)$ also exists and has the same numeric value. Therefore, whenever $ip(A|C)$ lacks a numeric value, $p_L(A|C)$ either doesn't exist or isn't unique. But inductive probabilities often do lack numeric values and these non-numeric inductive probabilities are often important in practical and theoretical contexts. For example, it seems obvious that the inductive probability of a future event, given just data about the past, does not in general have a numeric value. (Laplace's Rule of Succession attempts to show otherwise but, as we've seen, it rests on assumptions that are not true in general.) The cases in which inductive probabilities lack numeric values are precisely the ones for which an explication of inductive probability would be most useful, so the fact that p_L is undefined in all such cases seriously undermines its usefulness.

At this point we have seen that p_L is either multiply valued or else excessively vague, it is undefined in important cases in which inductive probabilities have precise numeric values, and it lacks a unique value in all the cases in which inductive probabilities lack numeric values. This is more than enough to show that p_L is a poor explicatum for inductive probability and so I will not pursue this evaluation of Laplace's definition of probability any further. I will present a better method of explicating inductive probability in Chapter 6.

4.4 Derivation of the Rule of Succession

This section gives a derivation of the Rule of Succession that follows the general approach sketched by Laplace (1820, xvii) but fills in details that he omitted.

Let E be $Oa_1 \dots Oa_n$, let K be $Xa_1 \dots Xa_{n+1}$, and let R_r be the proposition that $pp_X(O) = r$. Then we have:

$$\begin{aligned}
ip(E|R_r.K) &= ip(Oa_1 \dots Oa_n|R_r.K) \\
&= ip(Oa_1|R_r.K).ip(Oa_2|R_r.K.Oa_1) \dots ip(Oa_n|R_r.K.Oa_1 \dots Oa_{n-1}) \\
&= r^n, \quad \text{by Theorem 2.4 and Definition 2.1.}
\end{aligned} \tag{4.1}$$

For any proposition A, let

$$ip'(R_r|A) = \lim_{\delta \to 0^+} \frac{ip(r \leq pp_X(O) \leq r + \delta|A)}{\delta}.$$

Laplace's assumption that the a priori inductive probability distribution of $pp_X(O)$ is uniform on $[0, 1]$ implies:

$$ip'(R_r|K) = 1 \text{ for all } r \in [0, 1). \tag{4.2}$$

Applying a generalized form of Bayes's theorem we have, for all $r \in [0, 1)$:

$$\begin{aligned}
ip'(R_r|E.K) &= \frac{ip(E|R_r.K)\, ip'(R_r|K)}{\int_0^1 ip(E|R_s.K)\, ip'(R_s|K)\, ds} \\
&= \frac{ip(E|R_r.K)}{\int_0^1 ip(E|R_s.K)\, ds}, \quad \text{by (4.2)} \\
&= \frac{r^n}{\int_0^1 s^n\, ds}, \quad \text{by (4.1)} \\
&= (n+1)r^n.
\end{aligned} \tag{4.3}$$

Applying a generalized form of the law of total probability, we now have:

$$\begin{aligned}
ip(Oa_{n+1}|E.K) &= \int_0^1 ip(Oa_{n+1}|E.R_r.K)\, ip'(R_r|E.K)\, dr, \\
&= \int_0^1 ip(Oa_{n+1}|E.R_r.K)(n+1)r^n\, dr, \quad \text{by (4.3)} \\
&= \int_0^1 (n+1)r^{n+1}\, dr, \quad \text{by Theorem 2.4 and Definition 2.1} \\
&= \frac{n+1}{n+2}.
\end{aligned}$$

Chapter 5

Keynes's logical theory

John Maynard Keynes (1883–1946) is best known as an economist but he also made an important contribution to the philosophy of probability in his book *A Treatise on Probability*, published in 1921. This chapter will discuss the conception of probability defended by Keynes in that book. Besides being of historical interest, this discussion will further develop the account of inductive probability that I gave in Chapter 1. References are to the pages of Keynes (1921) unless otherwise indicated.

5.1 The meaning of probability

According to Keynes, probability is a relation between two propositions, not a property of one.

> While it is often convenient to speak of propositions as certain or probable, this expresses strictly a relationship in which they stand to a *corpus* of knowledge, actual or hypothetical, and not a characteristic of the propositions in themselves. A proposition is capable at the same time of varying degrees of this relationship, depending on the knowledge to which it is related, so that it is without significance to call a proposition probable unless we specify the knowledge to which we relating it. (3–4).

He asserted that this relation is objective.

> A proposition is not probable because we think it so. When once the facts are given which determine our knowledge, what is probable or improbable in these circumstances has been fixed objectively, and is independent of our opinion. (4)

Keynes regarded logic as concerned with valid or rational thought (3) and on this basis he said that probability is logical.

> The Theory of Probability is logical ... because it is concerned with the degree of belief which it is *rational* to entertain in given conditions. (4)

Keynes said he was using "probability" in the ordinary sense of that word and that probabilities in this ordinary sense don't always have numeric values.

> It is not straining the use of words to speak of this as the relation of probability. It is true that mathematicians have employed the term in a narrower sense; for they have often confined it to the limited class of instances in which the relation is adapted to an algebraic treatment. But in common usage the word has never received this limitation. (6)

In all these respects, Keynes's conception of probability agrees with inductive probability, so I think it is safe to conclude that what Keynes meant by "probability" is inductive probability.

I endorse most of Keynes's characterization of this concept but I disagree on two points. One is that Keynes thought the word "probability" has only one meaning in ordinary language. That is implicit in the preceding quotation and Keynes stated it more explicitly a few pages later:

> In the great majority of cases the term "probable" seems to be used by different persons to describe the same concept. Differences of opinion have not been due, I think, to a radical ambiguity of language. (8)

Later still, in discussing Venn's frequency theory of probability, Keynes asserted that "probability" is often used in a logical sense and it is "this [logical] sense *alone* which has importance" (96). By contrast, I believe that "probability" has two important senses, only one of which was recognized by Keynes.[1]

The other aspect of Keynes's discussion that I find unsatisfactory is his characterization of logic and the sense in which probability is logical. Keynes's view, that logic is concerned with rational thought, is incorrect on a variety of senses of "rational"; it is only correct if we understand "rational thought" as thought that corresponds to logical relations, in which case the characterization is empty. Likewise, as argued in Section 1.4, the meaning of "inductive probability" cannot be explained by identifying it with "rational degree of belief." I would say rather that logic is concerned with relations between propositions that hold in virtue of meanings and that inductive probability is logical because it is such a relation; the concept of belief plays no role in this.

5.2 Unmeasurable probabilities

Keynes called probabilities that have a numeric value "measurable." He agreed that some probabilities are measurable but, as we have seen, he denied that all are. The following is one of many examples given by Keynes:

[1] Keynes not only rejected physical probability himself, he also failed to recognize when others were using this concept, and supposed instead that they were always talking about inductive probability. As a result, he misunderstood many things Laplace said; for example, he thought Laplace's derivation of the Rule of Succession was inconsistent (373).

> We are out for a walk—what is the probability that we shall reach home alive? Has this always a numerical measure? If a thunderstorm bursts upon us, the probability is less than it was before; but is it changed by some definite numerical amount? (29)

Someone might claim that these probabilities could be determined by obtaining statistical data; Keynes gave two responses to this. First, if such data isn't included in the evidence to which the probabilities are related then the response is irrelevant—it at best shows that some *other* probabilities are measurable, namely, probabilities relative to different evidence. Second, even when statistical data is included in the evidence, we may also have non-statistical information that is relevant to the case at hand, in which case the probability may still not be measurable.

In the preceding example the probabilities, though not measurable, are nevertheless *comparable*; that is, one is larger than the other. Keynes further maintained that some probabilities are not even comparable; there is no saying which of them is larger. He gave the following example:

> Consider [two] sets of experiments, each directed towards establishing a generalisation. The first set is more numerous; in the second set the irrelevant conditions have been more carefully varied . . . Which of these generalisations is on such evidence the most probable? There is, surely, no answer; there is neither equality nor inequality between them . . . If we have more grounds than before, comparison is possible; but, if the grounds in the two cases are quite different, even a comparison of more and less, let alone numerical measurement, may be impossible.

Keynes is correct on all these points. However, his position has often been regarded as deplorable. For example, de Finetti, speaking of Keynes's view that there exist "probabilities which cannot be expressed as numbers," said:

> I myself regard as unacceptable, as a matter of principle, Keynes's position (the more so since the reservations which he had disappear when one adopts a subjective point of view). (de Finetti 1985, 359)

But if we are talking about a concept of ordinary language, as Keynes was, then its properties are what they are and cannot be changed by deploring them. Furthermore, a subjective point of view doesn't make the problem disappear, since degrees of belief are also often vague and unquantifiable.

5.3 Ramsey's criticisms

Keynes's book was sharply criticized by Ramsey. In a passage that continues to be quoted approvingly,[2] Ramsey wrote:

[2] For example, by Gillies (2000, 52), Hacking (2001, 144), Williamson (2005, 189), and Suppes (2006, 35).

> But let us now return to a more fundamental criticism of Mr. Keynes' views, which is the obvious one that there really do not seem to be any such things as the probability relations he describes. He supposes that, at any rate in certain cases, they can be perceived; but speaking for myself I feel confident that this is not true. I do not perceive them, and if I am to be persuaded that they exist it must be by argument; moreover, I shrewdly suspect that others do not perceive them either, because they are able to come to so very little agreement as to which of them relates any two given propositions. (Ramsey 1926, 161)

Unlike Keynes, I do not say that inductive probabilities can be "perceived"; my view is that we know their values, insofar as we do, in virtue of our grasp of the semantics of our language. Nevertheless, I agree with Keynes that inductive probabilities exist and we sometimes know their values. The passage I have just quoted from Ramsey suggests the following argument against the existence of inductive probabilities. (Here P is a premise and C is the conclusion.)

P: People are able to come to very little agreement about inductive probabilities.
C: Inductive probabilities do not exist.

P is vague (what counts as "very little agreement"?) but its truth is still questionable. Ramsey himself acknowledged that "about some particular cases there is agreement" (28). He asserted that "these paradoxically are always immensely complicated" but my coin example on page 1 is a counterexample to that. In any case, whether complicated or not, there is more agreement about inductive probabilities than P suggests.

Ramsey continued:

> If ... we take the simplest possible pairs of propositions such as "This is red" and "That is blue" or "This is red" and "That is red," whose logical relations should surely be easiest to see, no one, I think, pretends to be sure what is the probability relation which connects them. (162)

I agree that nobody would pretend to be sure of a numeric value for these probabilities, but there are inequalities that most people on reflection would agree with. For example, the probability of "This is red" given "That is red" is greater than the probability of "This is red" given "That is blue." This illustrates the point that inductive probabilities often lack numeric values. It doesn't show disagreement; it rather shows *agreement*, since *nobody* pretends to know numeric values here and practically everyone will agree on the inequalities.

Ramsey continued:

> Or, perhaps, they may claim to see the relation but they will not be able to say anything about it with certainty, to state if it is

> more or less than 1/3, or so on. They may, of course, say that it is incomparable with any numerical relation, but a relation about which so little can be truly said will be of little scientific use and it will be hard to convince a sceptic of its existence. (162)

Although the probabilities that Ramsey is discussing lack numeric values, they are not "incomparable with any numerical relation." Since there are more than three different colors, the a priori probability of "This is red" must be less than 1/3 and so its probability given "This is blue" must likewise be less than 1/3. In any case, the "scientific use" of something is not relevant to whether it exists. And the question is not whether it is "hard to convince a sceptic of its existence" but whether the sceptic has any good argument to support his position; Ramsey is perhaps suggesting that vagueness provides such an argument but I have already shown that it does not.

Ramsey concluded the paragraph I have been quoting as follows:

> Besides this view is really rather paradoxical; for any believer in induction must admit that between "This is red" as conclusion and "This is round" together with a billion propositions of the form "*a* is round and red" as evidence, there is a finite probability relation; and it is hard to suppose that as we accumulate instances there is suddenly a point, say after 233 instances, at which the probability relation becomes finite and so comparable with some numerical relations. (162)

Ramsey is here attacking the view that the probability of "This is red" given "This is round" cannot be compared with any number, but Keynes didn't say that and it isn't my view either. The probability of "This is red" given only "This is round" is the same as the a priori probability of "This is red" and hence less than 1/3. Given the additional billion propositions that Ramsey mentions, the probability of "This is red" is high (greater than 1/2, for example) but it still lacks a precise numeric value. Thus the probability is always both comparable with some numbers and lacking a precise numeric value; there is no paradox here.

I have been evaluating Ramsey's apparent argument from P to C. So far I have been arguing that P is false and responding to Ramsey's objections to unmeasurable probabilities.[3] Now I want to note that the argument is also invalid. Even if P were true, it could be that inductive probabilities exist in the (few) cases that people generally agree about. It could also be that the disagreement is due to some people misapplying the concept of inductive probability in cases where inductive probabilities do exist. Hence it is possible for P to be true and C false.

It may be suggested that the argument is not meant to be valid but rather is an inductive argument, the claim being merely that C is probable given P.

[3] Franklin (2001, 289) also argues that P is false and furthermore inconsistent with other things that Ramsey says.

But if this is a claim about inductive probability, as it seems to be, then it is inconsistent with the conclusion C of the argument that it is attempting to defend.

I conclude that Ramsey gave no good reason to doubt that inductive probabilities exist.

5.4 Measurement of probabilities

I will now consider how we can ascertain the numeric values of those probabilities that have such values. According to Keynes, "in order that numerical measurement may be possible, we must be given a number of *equally* probable alternatives (41)." More fully, I take Keynes's view to be that, in order to determine a numeric value for the probability of H given E, we must identify a partition $E_1, \ldots, E_n$ of E such that (1) each E_i has the same probability given E, and (2) H is logically equivalent, given E, to a disjunction of some number m of the E_i. The probability of H given E is then m/n. This is similar to Laplace's definition of probability except that Keynes attempting to give a rule for when probabilities are equal.

Keynes claimed that everyone has always agreed that this is the only way of measuring probabilities.

> It has always been agreed that a numerical measure can actually be obtained in those cases only in which a reduction to a set of exclusive and exhaustive *equiprobable* alternatives is practicable. (65)

This historical claim is false. From the earliest days of probability it has been widely believed that numeric probability values can sometimes be determined from statistical data. For example, Jacob Bernoulli conceded that the division into equally possible alternatives "can hardly ever be done ... except in games of chance" but he added that "what cannot be ascertained a priori, may at least be found out a posteriori from the results many times observed in similar situations" (Bernoulli 1713, 326–27). In the previous chapter we saw Laplace saying the same thing. Keynes himself, elsewhere in his book, acknowledged that:

> In statistical inquiries it is generally the case that [the] initial probability is based, not upon the Principle of Indifference, but upon the statistical frequencies of similar events which have been observed previously. (367)

However, the probabilities that are determined a posteriori are physical probabilities, whereas what Keynes means by "probability" is inductive probability. So I will now show that it is sometimes possible to determine a numeric value for an inductive probability without identifying any partition into equally probable alternatives.

One way this can happen is if the evidence specifies the value of a physical probability. If R states that $pp_X(O) = r$, then by DI we have $ip(Oa|Xa.R) = r$, without any need to find equally probable cases. Keynes wouldn't have accepted this because he didn't accept physical probability, but it is correct nevertheless.

Another way it can happen is if the evidence contains suitable statistical information. For example, the inductive probability that a particular newborn will be a boy, given just the statistical evidence of human sex ratios, is close to 0.51. Inductive probabilities of this sort are not perfectly precise but in many cases the indeterminacy in their values is negligible.

Finally, if E is consistent and logically implies H then it is uncontroversial that the inductive probability of H given E is one and that of not-H given E is zero; here again we can determine numeric values for inductive probabilities without having to divide the evidence into equally probable alternatives.

So Keynes was wrong to say that numeric values of probabilities can only be known when we are given a number of equally probable alternatives; there are at least three other ways in which they may be known. On the other hand, it is true that numeric inductive probabilities often are determined by using a partition into equally probable alternatives. The example with which I began this book is a case in point; here we don't have relevant statistics or the associated physical probability and the evidence doesn't imply or contradict the hypothesis; instead we judge that the coin must either land heads or tails and these alternatives have the same inductive probability given the evidence, hence the inductive probability of each is 1/2. Thus it is important to consider how we can determine when alternatives are equally probable; that is the topic of the next section.

5.5 The Principle of Indifference

Keynes seems to say that the only way to know that alternatives are equally probable is to derive this from a general rule. For example, immediately after saying that numerical measurement of probabilities requires equally probable alternatives he said that "the discovery of a rule, by which equiprobability could be established, was, therefore, essential" (41). Also, he later spoke of "the rules, in virtue of which we can assert equiprobability" (65).

Keynes noted that there was a traditional rule for this purpose, which he called "the Principle of Indifference."

> The Principle of Indifference asserts that if there is no *known* reason for predicating of our subject one rather than another of several alternatives, then relatively to such knowledge the assertions of each of these alternatives have an *equal* probability. (42)

In the previous chapter we saw Laplace using this principle and we also saw that the principle appears to lead to contradictions. Keynes agreed that the principle, formulated in this way, is contradictory, but he then proposed a more careful formulation that was designed to avoid the contradictions. However, subsequent writers have pointed out that Keynes's reformulation of the principle doesn't prevent all contradictions (Ramsey 1922; Howson and Urbach 1993; Gillies 2000).

The literature examining different formulations of the Principle of Indifference is large and technical. Fortunately, there is no need to discuss that literature here. Instead, I will argue that we we do not need a general rule, such as the Principle of Indifference purports to be, in order to know that some inductive probabilities are equal.

I begin by noting that Keynes himself acknowledged that probabilities can sometimes be known directly, without deducing them from a general rule.

> Inasmuch as it is always assumed that we can sometimes judge directly that a conclusion *follows from* a premiss, it is no great extension of this assumption to suppose that we can sometimes recognize that a conclusion *partially follows from*, or stands in a relation of probability to, a premiss. Moreover, the failure to explain or define "probability" in terms of other logical notions, creates a presumption that particular relations of probability must be, in the first instance, directly recognised as such, and cannot be evolved by rule out of *data* which themselves contain no statements of probability. (52–53)

The purpose of the Principle of Indifference, according to Keynes, is merely to enable us derive some probabilities from others that are recognized directly. Specifically, Keynes's reformulation of the Principle of Indifference requires that "there must be no *relevant* evidence relating to one alternative, unless there is *corresponding* evidence relating to the other" (55), and since relevant evidence is evidence that makes a difference to the probability, we can only apply this rule by first judging whether the probability of an alternative given our evidence is changed if some of that evidence is removed. Thus Keynes wrote:

> We have stated the Principle of Indifference in a more accurate form, by displaying its necessary dependence upon judgments of relevance and so bringing out the hidden element of direct judgment or intuition, which it has always involved. It has been shown that the Principle lays down a rule by which direct judgments of relevance and irrelevance can lead on to judgments of preference and indifference. (63–64)

But if we can make direct judgments of relevance and irrelevance we can surely also make direct judgments of indifference, i.e., judgments that two alternatives have the same probability on certain evidence, so it is inconsistent of Keynes to say that a general rule is needed to make such judgments. I think Keynes's remarks on this point were not fully considered and that a more considered statement of his view would be that general rules are helpful for determining when probabilities are equal, not that they are essential.

So far I have merely argued that Keynes was not being consistent with himself when he said that a general rule is essential for judgments of equiprobability. Now I will argue the substantive point, that we really can judge directly that alternatives are equally probable, at least in some cases. I begin my argument by recalling that inductive probability is logical, which means that true elementary statements of inductive probability are analytic. Thus the statement that two alternatives are equally probable given certain evidence is, if true, analytic. Furthermore, we obviously can recognize certain analytic truths directly, in virtue of our grasp of the concepts involved, and not by deriving them from a general rule. For example, we easily recognize that the following is true:

W_1: Given that an object is white, it follows logically that it is not black.

As far as I know, there is no general rule that determines whether one statement follows from another, so our recognition of the truth of W_1 doesn't derive from knowledge of such a rule. Similarly, almost everyone agrees that the following is true:

W_2: Given only that an object is either white or black, the inductive probability that it is white is 1/2.

The fact that almost everyone agrees that this is true is good evidence that it is true, since an error on this matter would involve a misapplication of the concepts of ordinary language and the people who endorse W_2 are competent users of ordinary language.[4] And since there are few if any people who know a general rule that determines when alternatives are equally probable, it follows that knowledge of W_2 doesn't require knowledge of such a general rule but rather can be derived from our grasp of the relevant concepts.

The lack of success in formulating a correct Principle of Indifference has been taken by some philosophers to show that logical probability "does not exist" (van Fraassen 1989, 292) or that "the logical interpretation ... does not allow numerical probabilities" (Gillies 2000, 48). We can now see that the latter inference is unsound for two reasons. First, numerical inductive probabilities can be determined in other ways than via judgments of equiprobability, as we saw in the preceding section. Second, knowledge that alternatives are equally probable needn't be derived from a general principle and hence doesn't require the Principle of Indifference.

5.6 Conclusion

Keynes gave a fundamentally correct account of the concept of inductive probability. He was right that these probabilities exist, that they relate pairs of

[4] This reverses Ramsey's argument for the non-existence of logical probabilities, considered in Section 5.3.

propositions, that they are not empirical or subjective but logical, and that they often lack numeric values. His main errors were his denial of physical probability and his defense of the Principle of Indifference.

Chapter 6

Explication of inductive probability

We have seen that inductive probabilities often lack numeric values and can even fail to be comparable. These are facts on which Keynes rightly insisted but they make it difficult to reason rigorously about inductive probabilities, especially in complex situations. Fortunately, there is a methodology for mitigating this difficulty, not recognized by Keynes: we can explicate the concept of inductive probability. This chapter will discuss how to do that. I begin with general considerations, then present a particular explication due to Rudolf Carnap, then discuss common criticisms of Carnap.

6.1 General considerations

6.1.1 Domain of the explicatum

The propositions A and B for which $ip(A|B)$ is meaningful are enormously diverse and it isn't feasible to construct an explicatum with such a large and diverse domain. Fortunately, this is also not necessary, since the explicatum only needs to be usable for specific purposes. Therefore, the first step in explicating inductive probability is to specify a limited domain of pairs of propositions A and B for which we will explicate $ip(A|B)$.

In many contexts there is a proposition K such that we are only interested in explicating inductive probabilities for which the evidence includes K; I will call such a K "background evidence," though it can be any proposition and need not be someone's evidence. To take a very simple example, K might be that a coin will be tossed twice and that each toss will land either heads or tails, while the inductive probabilities we want to explicate may be of the form $ip(A|K)$ or $ip(A|B.K)$, where A and B concern the outcome of one or both tosses. In more complex situations, K may be a large body of information possessed by some person. It would be difficult to explicate the a priori inductive probability of any such K and that isn't necessary for our purposes. Therefore, in what follows I will assume that the evidence propositions in the domain of the explicatum are all of the form $B.K$, for some B. The proposition B may be analytic, so the case where the evidence consists of K alone is automatically included in this. Situations in which there is no background evidence can be handled by taking K to be an analytic proposition. If there is background evidence that we cannot express in a proposition, it can still be denoted "K" and treated like a proposition (Pearl 1990).

For any propositions A and B, I will denote the proposition that A is false by "$\sim A$," the proposition that A and B are both true by "$A.B$," and the proposition that at least one of A and B is true by "$A \vee B$." An *algebra* of propositions is a nonempty set of propositions with the property that, for every A and B that it contains, it also contains $\sim A$, $A.B$, and $A \vee B$.

I will assume that we choose an algebra $\mathcal{A}$ of propositions with the intention that we will explicate all inductive probabilities of the form $ip(A|B.K)$, where A and B are in $\mathcal{A}$. For example, in the case where K says that a coin will be tossed twice and land either heads or tails on each toss, if H_i is the proposition that the coin lands heads on the ith toss then we could take $\mathcal{A}$ to be the algebra generated by H_1 and H_2, that is, the smallest algebra that contains H_1 and H_2.

6.1.2 Form of the explicatum

In the method I am proposing, the explicatum for inductive probability will be a function that takes two elements of $\mathcal{A}$ as arguments and has real numbers as its values; I will call this function "p" and I will denote the value of p for arguments A and B by "$p(A|B)$." This function is to be defined in such a way that $p(A|B)$ is a good explicatum for $ip(A|B.K)$, for all A and B in $\mathcal{A}$. I don't include K in the second argument of p because it is fixed in any context.

The definition of p will consist of axioms that together specify the value of $p(A|B)$ for all A and B in $\mathcal{A}$. These values must be specified in a way that doesn't depend on contingent facts; for example, an axiom may state that $p(A|B)$ equals 1/2 but not that it equals the proportion of times that a coin lands heads, even though the latter proportion may in fact be 1/2. By defining p in this way we ensure that it is logical and hence is, in this respect, like inductive probability.

The axioms that define p will include axioms that ensure p obeys the mathematical laws of probability. There are two reasons for this requirement. First, when inductive probabilities have numeric values they satisfy these laws, and we want $p(A|B)$ to equal $ip(A|B.K)$ when the latter has a numeric value, so we need p to satisfy the laws of probability when the corresponding inductive probabilities have numeric values. Second, a good explicatum is fruitful and simple, so it is desirable to have p satisfy the same laws even when the corresponding inductive probabilities lack numeric values.

In what follows it will be helpful to have some notation for logical relations. I will use "$A \Rightarrow B$" to mean that A logically implies B, that is, $\sim A \vee B$

is analytic. I will also use "$A \Leftrightarrow B$" to mean that A and B are logically equivalent, that is, $A \Rightarrow B$ and $B \Rightarrow A$.

The following axioms ensure that p satisfies the laws of probability; these are asserted for all A, B, C, and D in $\mathcal{A}$.[1]

Axiom 1. $p(A|B) \geq 0$.

Axiom 2. $p(A|A) = 1$.

Axiom 3. $p(A|B) + p(\sim A|B) = 1$, *provided* $B.K$ *is consistent.*

Axiom 4. $p(A.B|C) = p(A|C)\, p(B|A.C)$.

Axiom 5. *If* $A.K \Leftrightarrow C.K$ *and* $B.K \Leftrightarrow D.K$ *then* $p(A|B) = p(C|D)$.

One consequence of these axioms is the following:[2]

Theorem 6.1. *If* $B.K \Rightarrow A$ *then* $p(A|B) = 1$.

A corollary of this is:

Theorem 6.2. *If* $K \Rightarrow \sim B$ *then* $p(A|B) = 1$.

Hence if $B.K$ is inconsistent we have $p(A|B) + p(\sim A|B) = 1 + 1 = 2$; that is the reason for the proviso in Axiom 3.

Axioms 1–5 also entail the following additivity law

Theorem 6.3. *If* $K \Rightarrow \sim(A.B)$ *then* $p(A \vee B|C) = p(A|C) + p(B|C)$, *provided* $C.K$ *is consistent.*

6.1.3 Alternative formulations

The approach described in the two preceding subsections incorporates a number of choices that could be done differently. I will now indicate the main alternatives and my reasons for making the choices that I did.

I took the arguments of p to be propositions but one could instead take them to be sentences of a formal language that can express the relevant propositions. I decided not to use the latter method because it requires attention to linguistic details that are a distraction from the main issues involved in explicating inductive probability. Also, the apparently greater concreteness and rigor involved in using sentences is mostly illusory, since we need to specify the semantics of the formal language and this is done ultimately by stating,

[1] Similar axiomatizations of probability have been given by von Wright (1957, 93), Carnap (1971, 38), and Roeper and Leblanc (1999, 11), though my formulation differs from all of them in some respects. Von Wright imposed the restriction to consistent evidence on Axiom 2 rather than Axiom 3, which has the result that Theorems 6.1 and 6.2 don't hold. Carnap took $p(A|C)$ to be undefined for inconsistent C. Roeper and Leblanc redundantly added Theorem 6.2 as an additional axiom. And none of these authors allowed for background evidence.

[2] All theorems in this chapter are proved in Section 6.5 unless I refer to a proof elsewhere.

in ordinary language, the propositions that are the meanings of the sentences of the formal language.

I treated the concept of a proposition as primitive but propositions could instead be identified with sets of possible states of affairs; the latter approach derives from Kolmogorov (1933) and is standard among mathematicians. I have not done this because it would require me to give an exposition of set theory and explain how propositions can be correlated with sets. Explanations are all the more necessary because this is not a natural way of representing propositions. Freudenthal (1974) makes further criticisms of the set representation.

My way of accommodating background evidence is new, I believe, though it is merely an attempt to explicitly allow for a common Bayesian practice. An alternative approach would be to include K in $\mathcal{A}$ and have $p(A|B)$ defined only for those B in $\mathcal{A}$ that entail K; however, that is messier and isn't the way Bayesian probability models are normally formulated.

Most presentations of probability theory follow Kolmogorov (1933) in beginning with an unconditional function $p(\cdot)$. Kolmogorov's elementary axioms for this function, stated in my notation, are:

K1. $p(A) \geq 0$.

K2. If A is analytic then $p(A) = 1$.

K3. If $A.B$ is inconsistent then $p(A \vee B) = p(A) + p(B)$.

Conditional probability is then introduced by adopting as a definition:

K4. $p(A|B) = p(A.B)/p(B)$, provided $p(B) > 0$.

These axioms follow from mine, in the following sense:

Theorem 6.4. *If $p(A)$ is defined to be $p(A|T)$, where T is analytic, then K1–K4 all hold.*

Since all the usually-recognized elementary laws of probability follow from K1–K4, this theorem shows that those laws also follow from Axioms 1–5.

My main reason for not starting with unconditional probability is that K4 leaves $p(A|B)$ undefined when $p(B) = 0$, although $ip(A|B.K)$ can exist even when $ip(B|K) = 0$. For example, if X is tossing a coin, O is that the coin lands heads, and R_r is that $pp_X(O) = r$, then $ip(Oa|R_r.Xa) = r$ even though $ip(R_r|Xa) = 0$ for most, if not all, r. See Hájek (2003) for further discussion of the drawbacks of taking unconditional probability as primitive, including attempts to evade the problem by using infinitesimals.

Even writers who take conditional probability as primitive often say it is undefined when the second argument is inconsistent, whereas I have taken $p(A|B)$ to be defined for all B in $\mathcal{A}$, including inconsistent B. This has no practical significance, since our evidence is always consistent, but it has some advantages in simplicity and uniformity. Also, if we think of conditional probability as a generalization of logical implication then, since $B \Rightarrow A$ for inconsistent B, we should likewise have $p(A|B) = 1$ for inconsistent B.

6.2 Carnap's Basic System

Axioms 1–5 imply that $p(A|B) = 1$ if $B.K \Rightarrow A$ and $p(A|B) = 0$ if $B.K \Rightarrow {\sim}A$ and $K \not\Rightarrow {\sim}B$. However, these axioms don't fix the value of $p(A|B)$ in any other case and so additional axioms must be added to complete the definition of p. Unlike Axioms 1–5, these additional axioms must depend on the content of K and the propositions in $\mathcal{A}$. I will now present an example of such additional axioms, due to Carnap.

Carnap called the explication of inductive probability "inductive logic" and he worked on it from the 1940s until his death in 1970. Most discussions of Carnap's inductive logic only talk about his early explications published between 1945 and 1952, though his later explications were much better. Here I will present one special case from Carnap's posthumous "Basic System of Inductive Logic" (1971; 1980). I won't always state things exactly the way Carnap did; in particular, I will restate Carnap's proposals in the notation I have been using.

6.2.1 Domain of the explicatum

Carnap (1971, 43) assumed there is a denumerable set of *individuals*, denoted a_1, a_2, …; they could be balls in an urn, outcomes of tossing a die, birds, people, or almost anything else. It is assumed that the names "a_i" are chosen in such a way that, for $i \neq j$, it is analytic that a_i and a_j are different individuals.

Carnap (1971, 43) called a *type* of property a *modality*. Some examples of modalities are color (red, blue, …), shape (square, cubical, …), substance (iron, stone, wood, …), and age in years (0, 1, 2, …). The first three of these are qualitative and the last is quantitative. Other quantitative modalities include weight and height.

A *family of properties* is a set of properties that belong to one modality, are mutually exclusive, and jointly exhaustive. A *primitive property* is a property that isn't defined in terms of other properties in our analysis. In the explication I am presenting, Carnap (1971, 121) took the primitive properties to be the elements of a finite family of properties. These primitive properties will here be denoted F_1, F_2, …, F_k.

Goodman (1979, 74) defined the predicate "grue" as follows: It applies to things examined before time t iff they are green, and to things not examined before t iff they are blue. For example, if t is the year 2000, then a green emerald that was examined in 1960 is grue and a green emerald that was first examined in 2001 isn't grue. Since grue is a combination of two modalities (color and time), and Carnap required the primitive properties to belong to one modality, grue cannot be one of Carnap's primitive properties.[3]

An *atomic proposition* is a proposition that ascribes one of the primitive properties to one of the individuals. I will use "$F_i a_j$" to denote the atomic

[3] Carnap (1971, 74) also had another objection to grue, which I am omitting here.

proposition that individual a_j has primitive property F_i. A *sample* is a finite set of individuals. A *sample proposition* is a conjunction of atomic propositions, one for each individual in some sample. For example, $F_2a_1.F_5a_2$ is a sample proposition for the sample $\{a_1, a_2\}$. As a matter of formal convenience, we count the empty set as a sample and we deem an analytic proposition to be a sample proposition for the empty set.

We now fix the domain of the explicatum by taking $\mathcal{A}$ to be the algebra generated by the atomic propositions and taking K to be an analytic proposition. Note that $\mathcal{A}$ contains every sample proposition. Also, since K is analytic, no background evidence is assumed.

6.2.2 Definition of p

We have already partially defined p by Axioms 1–5. We will now complete the definition of p, for the domain of explication just described, by adding further axioms that were proposed by Carnap. As in Theorem 6.4, $p(A)$ here means $p(A|T)$, where T is analytic. Also, E is here any sample proposition, i is any integer between 1 and k, and m and n are any positive integers.

Carnap assumed that none of the F_i is infinitely precise (for example, specifying the exact wavelength of light reflected by an object). In that case, $ip(E) > 0$, for every sample proposition E. Hence Carnap (1971, 101) adopted:

Axiom 6 (Regularity)**.** $p(E) > 0$.

The individuals are supposed to be identified in a way that carries no information about which primitive property any individual has. Therefore, permuting the individuals will not change the inductive probability of any sample proposition; for example, $ip(F_1a_3.F_2a_5) = ip(F_1a_5.F_2a_3)$. Hence Carnap (1971, 118) adopted:

Axiom 7 (Symmetry[4])**.** $p(E)$ *isn't changed by permuting individuals.*

A characteristic property of inductive probability is that evidence that one individual has a property raises the probability that other individuals have the same property. For example, evidence that one bird is white raises the probability that another bird is white. Hence Carnap (1971, 161) adopted:

Axiom 8 (Instantial relevance)**.** $p(F_ia_n|E.F_ia_m) > p(F_ia_n|E)$ *provided* E *does not involve* a_m *or* a_n.

When the evidence is a sample proposition and the hypothesis is that some unobserved individual has a property, we typically take the relative frequency of that property in the sample to be the relevant fact provided by the evidence. For example, if someone is given the outcome of past tosses of a die and asked to state the probability that the die will come up six on the next toss, usually the person will look at the relative frequency of six in the past tosses and

[4] Following de Finetti (1937), this is also called *exchangeability*.

ignore the specific results of the tosses that didn't come up six. This suggests that it would be appropriate to adopt:

Axiom 9 (λ-condition)**.** *If a is any individual not involved in E then $p(F_i a|E)$ depends only on the number of individuals mentioned in E and the number that E says have F_i.*

But if, for example, F_1 is more similar to F_2 than to F_3, then reasoning by analogy suggests $ip(F_1a_1|F_2a_2) > ip(F_1a_1|F_3a_2)$, whereas Axiom 9 implies $p(F_1a_1|F_2a_2) = p(F_1a_1|F_3a_2)$. Carnap (1980, 84) was aware of this but considered the λ-condition to be appropriate when such differential similarity relations are insigificant.

Carnap (1980, §19) proved that Axioms 1–9 imply:

Theorem 6.5 ($\lambda\gamma$ theorem)**.** *If $k > 2$ then there exist $\lambda > 0$ and $\gamma_1, \ldots, \gamma_k \in (0, 1)$ such that the following holds: If E is a sample proposition for a sample of s individuals, s_i is the number of individuals to which E ascribes F_i, and a is any individual not involved in E, then*

$$p(F_i a|E) = \frac{s_i + \lambda\gamma_i}{s + \lambda}.$$

For example, if $\gamma_1 = 1/4$ and $\lambda = 2$ then

$$p(F_1a_4|F_1a_1.F_2a_2.F_3a_3) = \frac{1 + 2/4}{3 + 2} = \frac{3}{10}.$$

Extension of Theorem 6.5 to the case where $k = 2$ requires a further assumption (Carnap 1980, 98).

To get numeric values from Theorem 6.5 we must fix the values of λ and the γ_i. I'll now discuss how to to do that, starting with the γ_i.

By setting $s = 0$ in Theorem 6.5, we see that $\gamma_i = p(F_i a)$; thus γ_i needs to be a good explicatum for the a priori inductive probability that something has F_i. Let the *attribute space* for the F_i be the logical space whose points are the most specific properties of the relevant modality. Carnap (1971, 43–45) noted that each F_i corresponds to a region of the attribute space and he proposed (1980, 33–34) that γ_i be set equal to the proportion of the attribute space that corresponds to F_i.

For example, suppose the F_i are colors; then the attribute space could be taken to be the unit cube whose axes represent the degree of saturation (from 0 to 1) of red, green, and blue. If F_1 is the color red, it occupies a region around the point (1,0,0); if that region occupies 1/20 of the volume of the cube then we would set $\gamma_1 = 1/20$ (assuming that the object is monochromatic).

I now turn to λ. The formula in Theorem 6.5 can be rewritten as:

$$p(F_i a|E) = \left(\frac{s}{s + \lambda}\right)\frac{s_i}{s} + \left(\frac{\lambda}{s + \lambda}\right)\gamma_i.$$

This shows that $p(F_i a|E)$ is a weighted average of two factors, s_i/s and γ_i. The factor s_i/s is the relative frequency of F_i in the sample and hence is empirical, whereas γ_i is our explicatum for the a priori probability of F_ia, which is logical. The larger λ is, the more weight is put on the logical factor and the slower someone using p will learn from experience. In the limit as $\lambda \to \infty$, $p(F_ia|E) \to \gamma_i$ and there is no learning from experience; at the other extreme, as $\lambda \to 0$, $p(F_ia|E) \to s_i/s$. Carnap (1980, 107–19) considered the effect of different values of λ in a variety of examples and concluded that, in order for p to agree with inductive probability (to put it in my terms), λ should not be much less than 1 or much greater than 2. Since integer values are simplest, he further concluded that λ should be set equal to either 1 or 2. I think this is correct as far as it goes but we can go further, as follows.

A theorem of de Finetti shows that we can think of the individuals a_i as tokens of some experiment type X and the F_i as outcome types for which $pp_X(F_i)$ exists but is unknown.[5] If $\gamma_i = 1/2$ then the expected value of $pp_X(F_i)$ must be 1/2 and it is then natural to explicate the a priori inductive probability distribution for $pp_X(F_i)$ as uniform from 0 to 1. The assumptions used to derive the Rule of Succession (Section 4.2) are now satisfied and so, if E says that in a sample of s individuals all have F_i, we have:

$$p(F_ia|E) = \frac{s+1}{s+2}.$$

But by Theorem 6.5 and the assumption that $\gamma_i = 1/2$, we also have:

$$p(F_ia|E) = \frac{s+\lambda/2}{s+\lambda}.$$

These two identities imply that $\lambda = 2$.

Having thus fixed the values of λ and the γ_i, we have fixed the value of $p(A|B)$ for all A and B in $\mathcal{A}$, and hence the explication is complete.

6.3 Spurious criticisms of Carnap

There are many criticisms of Carnap's inductive logic that are frequently repeated by philosophers. Most of these criticisms are spurious, at least with respect to Carnap's Basic System. In this section I will point out the errors in the spurious criticisms presented by Hájek (2007a, sec. 3.2) and then, in the following section, I will discuss some legitimate criticisms.

6.3.1 Arbitrariness

Hájek writes:

> Is there a correct setting of λ, or said another way, how "inductive" should the confirmation function be? The concern here is that any particular setting of λ is arbitrary in a way that compromises Carnap's claim to be offering a *logical* notion of probability.

[5] The theorem is called de Finetti's representation theorem; Jeffrey (1971, 217–21) gives an exposition of it.

But the choice of λ isn't arbitrary; it is designed to ensure that p is a good explicatum for inductive probability and I have argued that setting $\lambda = 2$ is best for this purpose. Furthermore, even if the choice of λ were arbitrary, p would still be logical in the sense that Carnap (1950, 30) claimed, because its values are specified by its definition in a way that doesn't depend on contingent facts.

A little later Hájek expands on the objection this way:

> The whole point of the theory of logical probability is to explicate ampliative inference, although given the apparent arbitrariness in the choice of language and in the setting of λ—thus, in the choice of confirmation function—one may wonder how well it achieves this.

Here Hájek suggests that, in addition to the alleged arbitrariness in the choice of λ, there is also "arbitrariness in the choice of language." My presentation has used propositions rather than sentences of a language but, abstracting from this detail, the objection is that the choice of the domain of p is arbitrary. However, if $\mathcal{A}$ is chosen to contain the propositions whose inductive probabilities we want to explicate, as I proposed in Section 6.1.1, then the choice isn't arbitrary. Furthermore, even if the choice were arbitrary, that wouldn't prevent p being a good explicatum within its domain.

Hájek believes that in Carnap's inductive logic, the value of $p(H|E)$, for fixed H and E, changes when new predicates are added to the language.[6] Since the new predicates do not appear in H or E, our decision to include or exclude them from the language is irrelevant to $ip(H|E)$. Thus I think the objection Hájek intended to make is not what he said (that the choice of language is arbitrary) but rather that the value of $p(H|E)$ depends on irrelevant features of the language (or of the algebra $\mathcal{A}$). The answer to this objection is that there is no such dependence in Carnap's Basic System. In the special case that I presented, the primitive properties were required to belong to one family, so new ones can only be added by replacing existing ones. For example, we might subdivide an existing property into several more specific properties. Doing that will not change λ or the γ_i for the F_i that have not been replaced, hence it will not change $p(H|E)$ for any H and E that don't involve the new properties. We can also enrich $\mathcal{A}$ by allowing more than one family of properties; I haven't discussed how to do that but Carnap did and the proposals he made ensure that the value of $p(H|E)$, for given H and E, isn't altered by adding new families of properties (Carnap 1971, 46).

6.3.2 Axioms of symmetry

Hájek writes:

[6] Hájek states later that "by Carnap's lights, the degree of confirmation of a hypothesis depends on the language in which the hypothesis is stated" and he gives as examples "the addition of new predicates and the deletion of old ones."

> Significantly, Carnap's various axioms of symmetry are hardly logical truths.

In the explication I have described there is just one "axiom of symmetry," namely Axiom 7. That axiom, like all the other axioms, is part of the definition of p, hence analytic, and in that sense a logical truth. Furthermore, if there were additional symmetry axioms, they would also be part of the definition of p and hence also logical truths.

Hájek continues:

> Moreover, Fine (1973, 202) argues that we cannot impose further symmetry constraints that are seemingly just as plausible as Carnap's, on pain of inconsistency.

There are two things wrong with this. First:

Theorem 6.6. *There are uncountably many probability functions that satisfy all the constraints that Fine (1973, 193) claimed are not jointly satisfiable.*

Second, one of Fine's constraints (his L6) is not something that an explicatum for inductive probability should satisfy. It implies that all γ_i have the same value, which is not desirable in general. It also implies that, when there are multiple families of properties, the explicatum is insensitive to analogies between individuals that the evidence says differ in any respect, which is never desirable.[7]

6.3.3 Syntactic approach

Hájek writes:

> Another Goodmanian lesson is that inductive logic must be sensitive to the meanings of predicates, strongly suggesting that a purely syntactic approach such as Carnap's is doomed.

This criticism assumes that Carnap's inductive logic uses "a purely syntactic approach," that is, it assigns p values to pairs of expressions based on the form of the expression, without regard to what the expression means. However, Carnap's Basic System assigns p values to pairs of propositions, not expressions; hence it isn't a "syntactic approach."

Hájek's criticism seems to be that, because of its allegedly syntactic approach, Carnap's inductive logic is unable to distinguish between predicates like Goodman's "grue" and normal predicates like "green" and "blue." Stated non-linguistically, the objection would be that Carnap has no way of distinguishing properties like grue from normal properties like green and blue. But Carnap did distinguish between these properties, as we saw in Section 6.2.1.

[7] The problem here is essentially the one discussed in Maher (2001, sec. 3).

6.3.4 No canonical language

Hájek writes:

> Finding a canonical language seems to many to be a pipe dream, at least if we want to analyze the "logical probability" of any argument of real interest—either in science, or in everyday life.

This objection appears to assume that Carnap's inductive logic requires a "canonical language," though Hájek does not explain what this is or why he thinks Carnap is committed to it. In fact, one of Carnap's central philosophical principles was that there is no uniquely correct or right language.

> Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. (Carnap 1937, 52)
>
> Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them. (Carnap 1956, 221)

Perhaps what Hájek meant to say is that Carnapian inductive logic can only deal with propositions expressed in a formalized language and that no such language can express all the propositions involved in "any argument of real interest." Neither part of this is true. First, Carnap (1971) takes propositions to be sets of models of a language and observes that most of these propositions cannot be expressed in the language. Second, many arguments of real interest have been expressed in a formalized language.

6.3.5 Total evidence isn't well defined

Hájek writes:

> If one's credences are to be based on logical probabilities, they must be relativized to an evidence statement, e. But which is it to be? Carnap's recommendation is that e should be one's *total evidence* ... However, when we go beyond toy examples, it is not clear that this is well-defined. Suppose I have just watched a coin toss, and thus learned that the coin landed heads. Perhaps "the coin landed heads" is my total evidence? But I also learned a host of other things: as it might be, that the coin landed at a certain time, bouncing in a certain way, making a certain noise as it did so ... Call this long conjunction of facts X. I also learned a potentially infinite set of *de se* propositions: "I learned that X," "I learned that I learned that X" and so on. Perhaps, then, my total evidence is the infinite intersection of all these propositions, although this is still not obvious—and it is not something that can be represented by a sentence in one of Carnap's languages, which is finite in length.

It is true that the concept of a person's total evidence is vague, but most concepts of ordinary language are vague and that doesn't prevent them being useful. So I will take the objection to be that the concept of a person's evidence is too vague or complex to be represented in Carnap's inductive logic.

One answer to this objection is that a person's total evidence in a given context may be *explicated* by a relatively precise proposition. Such an explication is satisfactory if it captures sufficiently well the part of the person's total evidence that is relevant to the hypotheses under consideration in that context. Hájek's sequence of *de se* propositions would normally be irrelevant and could be omitted. A second answer was mentioned in Section 6.1.1: we can simply denote a person's total evidence as "K", without attempting to articulate all that it contains, and explicate inductive probabilities conditional on K.

6.3.6 Foundationalism

Hájek continues:

> The total evidence criterion goes hand in hand with positivism and a foundationalist epistemology according to which there are such determinate, ultimate deliverances of experience. But perhaps learning does not come in the form of such "bedrock" propositions, as Jeffrey (1992) has argued—maybe it rather involves a shift in one's subjective probabilities across a partition, without any cell of the partition becoming certain.

Carnap (1936, 425; 1963a, 57) denied that there are "bedrock" propositions. On the other hand, the inductive probability of any proposition given itself is 1, so if I use inductive probabilities given my evidence to guide my actions, I will act as if I am certain that my evidence is true. Carnap never explained how to reconcile these things.

The apparent contradiction can be resolved by recognizing that what we count as evidence isn't completely certain but only sufficiently certain that it can be treated as certain in the context at hand. Thus what counts as my evidence can change when the context changes. So if K is my total evidence in a particular context, then the principle of total evidence implies that I should treat K as certain in that context but it doesn't imply that K is a "bedrock" proposition; on the contrary, there may be other contexts in which I need to consider the possibility that K is false, and K won't be evidence for me in those contexts. See Maher (1996, 158–162) for further discussion of this account of evidence.

Before moving on it may be worth noting that the requirement of total evidence has been, and continues to be, widely endorsed. Carnap (1950, 212) cites endorsements by Jacob Bernoulli, Peirce, and Keynes; more recent endorsements include the following:

> Your assignment of 1/2 to the coin landing heads superficially seems unconditional; but really it is conditional on tacit assumptions about the coin, the toss, the immediate environment, and so on. In fact, it is conditional on your total evidence. (Hájek 2003, 315)

> The point of view I maintain is based on the thesis that *it is senseless to speak of the probability of an event unless we do so in relation to the body of knowledge possessed by a given person.* (de Finetti 2008, 3)

So if there were a problem with the requirement of total evidence, it would not be a problem peculiar to Carnap.

6.3.7 Circularity

Hájek writes:

> By Carnap's lights, the degree of confirmation of a hypothesis depends on the language in which the hypothesis is stated and over which the confirmation function is defined. But scientific progress often brings with it a change in scientific language (for example, the addition of new predicates and the deletion of old ones), and such a change will bring with it a change in the corresponding c-values. Thus, the growth of science may overthrow any particular confirmation theory. There is something of the snake eating its own tail here, since logical probability was supposed to explicate the confirmation of scientific theories.

This objection contains at least three errors. First, if the language of science changes, it doesn't follow that the domain of p changes, contrary to what Hájek assumes. Second, Carnap's Basic System isn't language sensitive in the way Hájek here supposes, that is, with respect to addition or deletion of predicates (as I pointed out in Section 6.3.1). Third, even if the growth of science did cause p to be substantively changed, that wouldn't make Carnap's inductive logic circular. I will elaborate on this last point.

The only things that can be logically circular are arguments and definitions. An explication isn't an argument, so the only way Carnap's inductive logic could be circular is if it contains a circular definition. But what is defined in Carnap's inductive logic is only the explicatum (p in my notation) and that definition is obviously not circular. In the particular explication I have presented, the definition is the conjunction of Axioms 1–9 and there is no circularity there. Hence, Carnap's inductive logic is demonstrably not circular.

Perhaps Hájek was thinking that, if we use the value of $ip(H|E)$ to fix the value of $p(H|E)$, and then use the latter to draw a conclusion about $ip(H|E)$, then we are arguing in a circle. However, nothing in Carnapian inductive logic entails that it should be used in this way. I discussed the use of explication for making inferences about the explicandum in Section 3.3; applied to the present case, my point was that if we find that p agrees with inductive probability in a variety of cases, we may infer that p also agrees with inductive probability in other cases in which we were previously unsure of the inductive probability; this isn't circular because we are reasoning from agreement in some cases to agreement in *other* cases.

6.4 Legitimate criticisms of Carnap

Although most of the common criticisms of Carnap's inductive logic are spurious, there are some legitimate criticisms of it. I will discuss them in this section.

The explication that I presented in Section 6.2 has the property that, for any sample proposition E, $p(F_i a_1 \ldots F_i a_n|E) \rightarrow 0$ as $n \rightarrow \infty$. So, if we were to add to $\mathcal{A}$ a proposition A_i that all individuals have F_i, we would have $p(A_i|E) = 0$, for every sample proposition E. This is true of all Carnap's explications of inductive probability and many authors regard it as unsatisfactory. However, there are a variety of ways of modifying Carnap's explications to avoid this result (Zabell 1997) and Carnap (1980, 145) himself had this issue on his agenda, so this defect (if it is one) is correctable, not fundamental.

Another legitimate criticism is that the explicata developed by Carnap have very simple domains and, as a result, aren't applicable to most situations of real interest. For example, Carnap never developed explications for domains involving relations or continuous magnitudes, or for situations with rich background evidence, though these are all common in science and everyday life. While this is true, it is merely a fact about the explications that Carnap actually developed; it doesn't show that the methodology of explicating inductive probability is similarly limited. On the contrary, Bayesian statisticians have developed probability models, which I would interpret as explications of inductive probability, for a wide variety of realistic domains; there are many examples in Gelman et al. (2003), Congdon (2007), and elsewhere. Furthermore, an explication of inductive probability for an artificially simple domain isn't necessarily useless, since it may help to clarify fundamental questions about the properties of confirmation and resolve philosophical paradoxes, as I have shown elsewhere.[8]

I conclude that explication of inductive probability is a valuable methodology for reasoning about inductive probability and that the particular explication of Carnap's that I have presented is a creditable simple example of such an explication.

[8] See (Maher 2004). Today I would replace the term "justified degree of belief" used in that paper with "inductive probability."

6.5 Proofs

6.5.1 Proof of Theorem 6.1

$$\begin{aligned}
p(A|B) &= p(A|B)p(B|B) && \text{by Axiom 2} \\
&= p(A|B)p(B|A.B) && \text{by Axiom 5 and } B.K \Rightarrow A \\
&= p(A.B|B) && \text{by Axiom 4} \\
&= p(B|B) && \text{by Axiom 5 and } B.K \Rightarrow A \\
&= 1 && \text{by Axiom 2.}
\end{aligned}$$

6.5.2 Proof of Theorem 6.2

If $K \Rightarrow \sim B$ then $B.K$ is inconsistent, so trivially $B.K \Rightarrow A$, and hence $p(A|B) = 1$ by Theorem 6.1.

6.5.3 Lemmas used in the proof of Theorem 6.3

Lemma 1. *If $C.K \Rightarrow A$ then $p(\sim A|C) = 0$, provided $C.K$ is consistent.*

Proof:

$$\begin{aligned}
p(\sim A|C) &= 1 - p(A|C) && \text{by Axiom 3} \\
&= 1 - 1 && \text{by Theorem 6.1 and } C.K \Rightarrow A \\
&= 0.
\end{aligned}$$

Lemma 2. *$p(A|C) = p(A.B|C) + p(A.\sim B|C)$, provided $C.K$ is consistent.*

Proof: If $A.C.K$ is consistent then

$$\begin{aligned}
p(A|C) &= p(A|C)[p(B|A.C) + p(\sim B|A.C)] && \text{by Axiom 3} \\
&= p(A.B|C) + p(A.\sim B|C) && \text{by Axiom 4.}
\end{aligned}$$

If $A.C.K$ is inconsistent then $C.K \Rightarrow \sim A$ so, by Lemma 1, all quantities in Lemma 2 are zero.

6.5.4 Proof of Theorem 6.3

$$\begin{aligned}
p(A \vee B|C) &= p[(A \vee B).A|C] + p[(A \vee B).\sim A|C] && \text{by Lemma 2} \\
&= p(A|C) + p(B.\sim A|C) && \text{by Axiom 5} \\
&= p(A|C) + p(B|C) && \text{by Axiom 5 and } K \Rightarrow \sim(A.B).
\end{aligned}$$

6.5.5 Proof of Theorem 6.4

K1 follows from Axiom 1, K2 from Axiom 2, K3 from Theorem 6.3, and K4 from Axiom 4.

6.5.6 Proof of Theorem 6.6

Lemma 5 of Fine (1973, 193) asserts that five constraints, called L1, L2, L3, L6, and L7 by Fine, are not jointly satisfiable. I will show that, on the contrary, there are uncountably many functions that satisfy those constraints. To facilitate comparison with Fine's text, I will mostly use Fine's notation in this proof.

Let a *family of properties* be a finite set of properties that are pairwise exclusive and jointly exhaustive.[9] Let $\mathcal{P}^1, \ldots, \mathcal{P}^n$ be logically independent families of properties with $\mathcal{P}^i = \{P^i_1, \ldots, P^i_{k_i}\}$. Let $P^{1\ldots n}_{l_1\ldots l_n}$ be the property of having all of $P^1_{l_1}, \ldots, P^n_{l_n}$ and let

$$\mathcal{P}^{1\ldots n} = \{P^{1\ldots n}_{l_1\ldots l_n} :\ 1 \le l_j \le k_j,\ j = 1, \ldots, n\}.$$

Thus $\mathcal{P}^{1\ldots n}$ is a family of properties formed by combining $\mathcal{P}^1, \ldots, \mathcal{P}^n$.

Let a finite set of individuals be given and, for any property ϕ, let ϕa be the proposition that individual a has ϕ. A proposition of the form $P^i_l a$ will be called an *atomic proposition.* Let a *sample proposition* with respect of family of properties $\mathcal{P}$ be a proposition that ascribes a property from $\mathcal{P}$ to each member of some sample. Logically true propositions will be regarded as sample propositions with respect to any family of properties, the sample in this case being the empty set.

Let $\mathcal{A}$ be the algebra of propositions generated by the atomic propositions and let $C(H|E)$ be defined for all $H \in \mathcal{A}$ and consistent $E \in \mathcal{A}$ by the following axioms. Here H and H' are any propositions in $\mathcal{A}$, E and E' are any consistent propositions in $\mathcal{A}$, and λ is any positive real constant.

A1. If E is a sample proposition with respect to $\mathcal{P}^{1\ldots n}$ for a sample of s individuals, $s^{1\ldots n}_{l_1\ldots l_n}$ is the number of individuals to which E ascribes $P^{1\ldots n}_{l_1\ldots l_n}$, and a is any individual not involved in E, then

$$C(P^{1\ldots n}_{l_1\ldots l_n} a|E) = \frac{s^{1\ldots n}_{l_1\ldots l_n} + \lambda/k_1 \ldots k_n}{s + \lambda}.$$

A2. If $H \Leftrightarrow H'$ and $E \Leftrightarrow E'$ then $C(H|E) = C(H'|E')$.

A3. If $E \Rightarrow {\sim}(H.H')$ then $C(H \vee H'|E) = C(H|E) + C(H'|E)$.

A4. $C(H.E'|E) = C(H|E.E')\, C(E'|E)$.

Let a *state* be a proposition of the form $\phi_1 a_1 \ldots \phi_\nu a_\nu$, where each ϕ_i is a property in $\mathcal{P}^{1\ldots n}$ and $a_1, \ldots, a_\nu$ are all the individuals. Letting $C(H)$ be an abbreviation for $C(H|T)$, where T is any logically true proposition, repeated application of A4 gives:

$$C(\phi_1 a_1 \ldots \phi_\nu a_\nu) = C(\phi_1 a_1) C(\phi_2 a_2|\phi_1 a_1) \ldots C(\phi_\nu a_\nu|\phi_1 a_1 \ldots \phi_{\nu-1} a_{\nu-1}).$$

[9] This is a looser definition than the one in Section 6.1.1, since it doesn't require the properties to belong to one modality. It agrees with Fine's definition but not with that of Carnap (1971, 43) and will only be used in the present proof.

The value of each term on the right hand side is given by A1, hence the axioms fix the value of $C(S)$ for every state S. Every consistent $H \in \mathcal{A}$ is equivalent to a disjunction of states so, letting "S" be a variable ranging over states, we have by A2 and A3:

$$C(H) = \sum_{S \Rightarrow H} C(S), \quad \text{for all consistent } H \in \mathcal{A}. \tag{6.1}$$

If H is inconsistent then

$$\begin{aligned} C(H) &= C(H \vee {\sim}H) - C({\sim}H), \quad \text{by A3} \\ &= C({\sim}H) - C({\sim}H), \quad \text{by A2} \\ &= 0. \end{aligned}$$

Combining this with (6.1), we have:

$$C(H) = \sum_{S \Rightarrow H} C(S), \quad \text{for all } H \in \mathcal{A}. \tag{6.2}$$

By A1, $C(S) > 0$ for all states S and hence $C(E) > 0$ for all consistent $E \in \mathcal{A}$. Therefore, by A4, we have

$$C(H|E) = C(H.E)/C(E), \quad \text{for all consistent } E \in \mathcal{A}. \tag{6.3}$$

Since the values of $C(H.E)$ and $C(E)$ are given by (6.2), this shows that A1–A4 fix the value of $C(H|E)$ for all $H \in \mathcal{A}$ and consistent $E \in \mathcal{A}$. I will now show that C satisfies the constraints that Fine claimed could not be jointly satisfied.

L1

Theorem 2 of Fine (1973, 189) states that L1 is equivalent to a conjunction of five conditions. Three of these are identical to A2, A3, and A4; the other two are the following (asserted for $H, H' \in \mathcal{A}$ and consistent $E, E', E'' \in \mathcal{A}$):

(i) $0 \leq C(H|E) < \infty$.

(ii) If $E \Rightarrow H$ and $E' \not\Rightarrow H'$ then $C(H|E) > C(H'|E')$.

I will now show that both these conditions are satisfied.

Proof of (i):

$$\begin{aligned} C(H|E) &= \frac{C(H.E)}{C(E)}, \quad \text{by (6.3)} \\ &= \frac{\sum_{S \Rightarrow H.E} C(S)}{\sum_{S \Rightarrow E} C(S)}, \quad \text{by (6.2).} \end{aligned}$$

There are at least as many terms in the denominator as in the numerator and, by A1, each term is positive. Hence $0 \leq C(H|E) \leq 1$, which entails (i).

Proof of (ii): Assume $E \Rightarrow H$ and $E' \not\Rightarrow H'$. Then:

$$\begin{aligned} C(H|E) &= \frac{C(H.E)}{C(E)}, \quad \text{by (6.3)} \\ &= \frac{C(E)}{C(E)}, \quad \text{by A2 and } E \Rightarrow H \\ &= 1. \end{aligned}$$

$$\begin{aligned} C(H'|E') &= \frac{C(H'.E')}{C(E')}, \quad \text{by (6.3)} \\ &= \frac{\sum_{S \Rightarrow H'.E'} C(S)}{\sum_{S \Rightarrow E'} C(S)}, \quad \text{by (6.2)}. \end{aligned}$$

Since $E' \not\Rightarrow H'$, the terms in the numerator are a proper subset of those in the denominator and so, since all terms are positive, $C(H'|E') < 1$. Hence $C(H|E) > C(H'|E')$.

L2

L2 says $C(H|E)$ is invariant under any permutation of the individuals. It is satisfied because A1–A4 treat all individuals alike.

L3

L3 says that $C(H|E)$ is invariant under augmentation of the set of individuals.[10] This is satisfied because none of A1–A4 refers to the total number of individuals.

L6

L6 says that $C(H|E)$ is invariant under any permutation of properties in $\mathcal{P}^{1\ldots n}$. We have seen that

$$C(H|E) = \frac{\sum_{S \Rightarrow H.E} C(S)}{\sum_{S \Rightarrow E} C(S)}.$$

Permuting the properties in $\mathcal{P}^{1\ldots n}$ will not change the number of states S that entail $H.E$ or E and, by A1 and A4, it will not change the value of $C(S)$ for any S. Therefore, $C(H|E)$ will not change and L6 is satisfied.

L7

L7 says that $C(H|E)$ is invariant under augmentation of the set of families of properties. So let $\mathcal{P}^{n+1} = \{P_1^{n+1}, \ldots, P_{k_{n+1}}^{n+1}\}$ be a family of properties that is logically independent of $\mathcal{P}^{1\ldots n}$. Let $\mathcal{A}'$ be the algebra generated by propositions of the form $P_{l_i}^i a$, where $1 \leq i \leq n+1$ and $1 \leq l_i \leq k_i$. Let $C'(H|E)$ be defined for all $H \in \mathcal{A}'$ and consistent $E \in \mathcal{A}'$ by the following axioms.

[10] I omit Fine's qualification that H and E not contain universal or existential quantifiers because $\mathcal{A}$ contains only truth-functional combinations of atomic propositions.

A1′. If E is a sample proposition with respect to $\mathcal{P}^{1\ldots n+1}$ for a sample of s individuals, $s^{1\ldots n+1}_{l_1\ldots l_{n+1}}$ is the number of individuals to which E ascribes $P^{1\ldots n+1}_{l_1\ldots l_{n+1}}$, and a is any individual not involved in E, then

$$C'(P^{1\ldots n+1}_{l_1\ldots l_{n+1}}a|E) = \frac{s^{1\ldots n+1}_{l_1\ldots l_{n+1}} + \lambda/k_1\ldots k_{n+1}}{s+\lambda}.$$

A2′. If $H \Leftrightarrow H'$ and $E \Leftrightarrow E'$ then $C'(H|E) = C'(H'|E')$.

A3′. If $E \Rightarrow \sim(H.H')$ then $C'(H \vee H'|E) = C'(H|E) + C'(H'|E)$.

A4′. $C'(H.E'|E) = C'(H|E.E')\,C'(E'|E)$.

These axioms fix the value of $C'(H|E)$ for all $H \in \mathcal{A}'$ and consistent $E \in \mathcal{A}'$; the proof is exactly analogous to the proof that A1–A4 fix the value of $C(H|E)$ for all $H \in \mathcal{A}$ and consistent $E \in \mathcal{A}$. I will now show that C' agrees with C on $\mathcal{A}$.

Let E be a sample proposition with respect to $\mathcal{P}^{1\ldots n}$ for a sample of s individuals and let E' be any sample proposition with respect to $\mathcal{P}^{1\ldots n+1}$ that involves the same individuals as E and is such that $E' \Rightarrow E$. Then for any individual a not involved in E:

$$\begin{aligned}
C'(P^{1\ldots n}_{l_1\ldots l_n}a|E) &= \sum_{E'} C'(P^{1\ldots n}_{l_1\ldots l_n}a|E')C'(E'|E), \quad \text{by A2′–A4′} \\
&= \sum_{E'}\sum_{l_{n+1}} C'(P^{1\ldots n+1}_{l_1\ldots l_{n+1}}a|E')C'(E'|E), \quad \text{by A2′ and A3′} \\
&= \sum_{E'}\sum_{l_{n+1}} \frac{s^{1\ldots n+1}_{l_1\ldots l_{n+1}} + \lambda/k_1\ldots k_{n+1}}{s+\lambda}\, C'(E'|E), \quad \text{by A1′} \\
&= \frac{s^{1\ldots n}_{l_1\ldots l_n} + \lambda/k_1\ldots k_n}{s+\lambda} \sum_{E'} C'(E'|E) \\
&= \frac{s^{1\ldots n}_{l_1\ldots l_n} + \lambda/k_1\ldots k_n}{s+\lambda}, \quad \text{by A2′–A4′.}
\end{aligned}$$

Hence C' satisfies the proposition that results from substituting "C'" for "C" in A1. The same is obviously true for A2–A4. Hence $C'(H|E) = C(H|E)$ for all $H \in \mathcal{A}$ and consistent $E \in \mathcal{A}$, so L7 is satisfied.

This completes the proof that C satisfies all the constraints in Fine's Lemma 5. Since λ can be any positive real number, and each choice of λ gives a different C, it follows that there are uncountably many functions that satisfy Fine's constraints.

Chapter 7

Von Mises' frequency theory

Richard von Mises (1883–1953) was an applied mathematician who worked primarily on mechanics and probability. In 1919 he proposed a theory of the nature of probability, which he developed further in later publications, and which became one of the most influential theories of probability in the 20th century. This chapter will describe and evaluate von Mises' theory.

7.1 Mises' theory

According to von Mises,

> the subject matter of probability theory is long sequences of experiments or observations repeated very often and under a set of invariable conditions. We observe, for example, the outcome of the repeated tossing of a coin or of a pair of dice; we record the sex of newborn children in a population; we determine the successive coordinates of the points at which bullets strike a target in a series of shots aimed at the bull's-eye; or, to give a more general example, we note the varying outcomes which result from measuring "the same quantity" when "the same measuring procedure" is repeated many times. (1964, 2)

Although von Mises here said that the experiment is to be repeated "under a set of invariable conditions," he obviously didn't mean that *every* circumstance must be exactly the same in all details for each repetition; otherwise, repeated tosses of a coin or a pair of dice would always give the same result. I suppose he meant that each repetition must be a token of the same experiment type.

Von Mises acknowledged that in ordinary language we often speak of probabilities in situations that don't involve repetition of an experiment type but he said his theory wasn't concerned with that kind of probability.

> We all know very well that in colloquial language the term probability or probable is very often used in cases which have nothing to

> do with mass phenomena or repetitive events. But I decline positively to apply the mathematical theory to questions like this: What is the probability that Napoleon was a historical person rather than a solar myth? This question deals with an isolated fact which in no way can be considered an element in a sequence of uniform repeated observations. (1941, 191)

Although von Mises was concerned with long sequences of experiments, he wasn't concerned with all such sequences; he was only concerned with those that also satisfy two further conditions. One of these conditions is that the relative frequency[1] of an attribute in the first n elements of a sequence becomes increasingly stable with increasing n.

> It is essential for the theory of probability that experience has shown that in the game of dice, as in all the other mass phenomena which we have mentioned, the relative frequencies of certain attributes become more and more stable as the number of observations is increased. (1957, 12).

The other is that events occur randomly, not in any regular pattern.

> Examples can easily be found where the relative frequencies converge towards definite limiting values, and where it is nevertheless not appropriate to speak of probability. Imagine, for instance, a road along which milestones are placed, large ones for whole miles and smaller ones for tenths of a mile. If we walk long enough along this road, calculating the relative frequencies of large stones, the value found in this way will lie around 1/10 ... The sequence of observations of large or small stones differs essentially from the sequence of observations, for instance, of the results of a game of chance, in that the first obeys an easily recognizable law ... We shall, in future, consider only such sequences of events or observations, which satisfy the requirements of complete lawlessness or "randomness." (1957, 23–24)

Having thus identified the kind of phenomena that he was concerned with, von Mises defined concepts that he intended to represent the relevant features of these phenomena in an idealized way.

> I aim at the construction of a rational theory, based on the simplest possible exact concepts, one which, although admittedly inadequate to represent the complexity of the real processes, is able to reproduce satisfactorily some of their essential properties. (1957, 8)

[1] The *relative frequency* of an attribute in a class is the proportion of the members of the class that have the attribute. For example, if a coin is tossed 100 times and lands heads on 53 of those tosses, the relative frequency of heads in those tosses is 0.53.

In von Mises' theory, a long sequence of outcomes of an experiment is represented by a hypothetical infinite sequence of events. For example, to deal with tosses of a coin von Mises would consider a hypothetical infinite sequence of tosses of that coin, even though it is impossible to toss a coin infinitely often.

Let a *place selection* be a procedure[2] for selecting elements from a sequence in which the decision whether to select a given element doesn't depend on the attribute of that or any subsequent element of the sequence. For example, in an infinite sequence of coin tosses, choosing every second toss is a place selection, and so is choosing every toss that follows a head, but choosing every toss that lands heads isn't a place selection. Von Mises (1957, 24–25) called an infinite sequence S of events a *collective* if it has both of the following properties:

(1) For each attribute A, the relative frequency of A in the first n elements of S approaches a limit as n goes to infinity.

(2) For each infinite sequence S' obtained from S by a place selection, and each attribute A, the relative frequency of A approaches the same limit in S' that it does in S.

Property (1) is intended to reflect the requirement that relative frequencies become more stable as the number of observations is increased. Property (2) is intended to reflect the requirement that the attributes occur randomly.

We come now to von Mises' definition of probability. Although "probability" is a word in ordinary language, von Mises' definition of probability was not intended to give the ordinary meaning of this word; instead he intended it to specify a more restricted and precise concept that is suitable for scientific purposes. Von Mises (1957, 4–5) acknowledged that using a pre-existing word for a newly defined concept might cause confusion but he justified it by saying this was a common procedure in science; for example, "work" is defined in mechanics in a way that differs from its ordinary meaning.

Probability, as defined by von Mises, is relative to a collective; this is something that he emphasized repeatedly.

> The principle which underlies our whole treatment of the probability problem is that a collective must exist before we begin to speak of probability. The definition of probability which we shall give is only concerned with "the probability of encountering a certain attribute in a given collective." (1957, 12)

Von Mises (1957, 29) defined the probability of an attribute A in a collective C as the limit of the relative frequency of A in C. The existence of this limit is, of course, guaranteed by the definition of a collective.

[2] See Church (1940) for discussion of the concept of a "procedure."

7.2 Evaluation

Von Mises stated said he wasn't concerned with all ordinary uses of the word "probability." Furthermore, his examples of uses of "probability" that he wasn't concerned with were always inductive probabilities, such as the probability that Napoleon was a historical person (1941, 191), that Germany will be involved in a war with Liberia (1957, 9), and that the same person wrote the *Iliad* and the *Odyssey* (1964, 1). Conversely, von Mises' examples of the kind of probability he *was* concerned with were physical probabilities, as in the following passage.

> To reach the essence of the problems of probability which do form the subject-matter of this book, we must consider, for example, the probability of winning in a carefully defined game of chance. Is it sensible to bet that a "double 6" will appear at least once if two dice are thrown twenty-four times? Is this result "probable"? More exactly, how great is its probability? Such are the questions we feel able to answer. (1957, 9)

Von Mises also said that his aim wasn't to give an accurate description but rather to define an idealized concept of probability. These are all things that someone might say if they were aiming to explicate the concept of physical probability. It will therefore not be entirely inappropriate to evaluate von Mises' theory as a proposed explication of physical probability; in any case, that is how I'm going to evaluate it.

The next question is what we are to take as von Mises' explicatum for physical probability; I will consider a variety of possibilities.

7.2.1 Von Mises' definition of probability

For any collective C and attribute A, let $md_C(A)$ be the limiting relative frequency of A in C. This function md is what von Mises' defined as "probability." It may then seem natural that we should regard md as von Mises' explicatum for physical probability.

As argued in Section 2.1, physical probability is a function of an experiment type and an outcome type. Von Mises own writings confirm that this is correct; for example, in the preceding quotation he identified the probability he was concerned with by stating the experiment type (throwing two dice twenty four times) and an outcome type (double six). On the other hand, md is a function of a collective and an attribute. An outcome type is an attribute but an experiment type isn't a sequence and hence isn't a collective. Therefore, md differs from physical probability in being a function of a collective rather than an experiment type.

This fundamental difference between md and physical probability makes the former a poor explicatum for the latter. To see this, recall that a good explicatum must be able to be used for the same purposes as its explicandum.

Now, in any particular use of the concept of physical probability, we identify which physical probability we are talking about by specifying the relevant experiment and outcome types. Since no collective is specified, it is not possible to substitute md in place of physical probability, hence md cannot be used for the same purposes.

7.2.2 Idealized actual sequence

As stated in Section 7.1, von Mises took himself to be dealing with cases in which there is a long actual sequence of experiment tokens and he thought of the collective as an idealization of that long actual sequence. So, for any experiment type X and outcome type O, let us define $mi_X(O)$ to be the limiting relative frequency of O in the collective that is the idealization of the sequence of actual outcomes of X. Perhaps we should regard mi as von Mises' proposed explicatum for physical probability.

However, it is easy to see that mi is also a poor explicatum for physical probability. In many cases, the experiment types whose physical probability we want to discuss aren't performed often; they may not even be performed at all. For example, we can discuss the physical probability that a particular coin will land heads when tossed, even if that coin is never actually tossed. Furthermore, regardless of how many times an experiment is performed, there are many infinite sequences that might equally well be regarded as idealizations of the actual sequence of outcomes, provided X has more than one possible outcome. Therefore, in all interesting cases, there is no such thing as *the* idealization of the sequence of actual outcomes of X and so $mi_X(O)$ is undefined. A concept that is undefined in all interesting cases obviously can't be used for the same purposes as the concept of physical probability.

7.2.3 Jeffrey's interpretation

According to Jeffrey (1977), von Mises assumed that there is a unique collective that *would* result if an experiment were repeated forever. So, for any experiment type X and outcome type O, let $mj_X(O)$ be the limiting relative frequency of O in the collective that would result if X were repeated infinitely. Jeffrey's interpretation of von Mises suggests taking mj to be von Mises' explicatum for physical probability.

I doubt that this is a correct interpretation of von Mises. Jeffrey gave no textual evidence to support his attribution and von Mises never, to my knowledge, stated or implied the assumption that Jeffrey attributed to him. It is true that von Mises talked as though there was a one-to-one correlation between experiment types and collectives but the explanation for that could be the one that von Mises himself gave, namely, that he took the collective associated with an experiment to be an idealization of the actual sequence of outcomes of the experiment.

In any case, mj isn't an adequate explicatum for physical probability. As

Jeffrey pointed out, there is in general no such thing as *the* collective that would result if an experiment were repeated infinitely. For example, in tossing a coin,

> unless the coin has two heads or two tails (or the process is otherwise rigged), there is no telling whether the coin would have landed head up on a toss that never takes place. That's what probability is all about. (Jeffrey 1977, 193)

Therefore, if X is tossing a fair coin and O is that the coin lands heads then $mj_X(O)$ doesn't exist although $pp_X(O)$ exists and equals 1/2. Similarly in all other cases in which X has more than one possible outcome. Therefore, mj can't be used for the same purposes as physical probability.

7.2.4 Howson and Urbach's interpretation

According to Howson and Urbach (1993, 325, notation changed),

> the apparent dependence of the probabilities in von Mises' theory on the particular collective C is misleading, though such a dependence has often been cited in criticism of that theory. It is clear that von Mises intended the probabilities to characterize the experiment X rather than C itself ... He took it as a fact about the world, as everybody does who accepts a frequency interpretation of probability, that all those sequences which might have been generated by the same collective-generating experiment X would possess the same long-run characteristics.

So, for any experiment type X and outcome type O, let $mh_X(O)$ be the limiting relative frequency of O in any one of the collectives that might result if X were repeated infinitely. Howson and Urbach's interpretation of von Mises suggests taking mh to be von Mises' explicatum for physical probability.

I doubt that this is a correct interpretation of von Mises. Howson and Urbach gave no textual evidence to support their interpretation and, so far as I know, von Mises never stated the assumption they attribute to him. Furthermore, Howson and Urbach's interpretation is inconsistent with von Mises' (1957, 12) insistence that "a collective must exist before we begin to speak of probability."

In any case, mh isn't an adequate explicatum for physical probability. To see this, consider the case in which X is tossing a fair coin. Every infinite sequence of heads and/or tails is a logically possible outcome of repeating X infinitely often and each of these sequences has the same probability of occurring, namely zero. Furthermore, for each of these sequences the probability of getting the first n elements of the sequence is the same, namely $1/2^n$. So, there is no basis for saying that some of these sequences might result and others might not; they are all on a par with each other and the experiment of repeating X infinitely often might have any one of them as its outcome. But the long run properties of these sequences are *not* all the same; for example, the limiting relative frequency of heads is zero in some, one in others, has an intermediate value in others, and doesn't exist in others. Therefore, if O is that the coin lands heads then $mh_X(O)$ doesn't exist although $pp_X(O)$ exists and equals 1/2. I have here discussed a particular example but the same result will hold in many other cases, so mh isn't a concept that can be used for the same purposes as physical probability.

7.2.5 A modified theory

For any experiment type X and outcome type O, let $mm_X(O) = r$ iff repeating X infinitely would produce a collective in which O has a limiting relative frequency of r. Note that $mm_X(O) = r$ can be true even if there is no collective that would result of X were repeated infinitely; what is required is only that, if X were repeated infinitely, then the resulting sequence would be one or other of the (in general infinitely many) collectives in which O has a limiting relative frequency of r. The concept mm thus violates von Mises' precept that we should begin by identifying a collective. Nevertheless, mm is defined using von Mises' concept of limiting relative frequency in a collective, so the proposal of mm as an explicatum for physical probability may be considered as a modified version of von Mises' theory. I will now evaluate this modified theory.

Consider again the case where X is tossing a fair coin and O is that the coin lands heads. I have argued that every infinite sequence of heads and/or tails *might* be the result of tossing the coin infinitely. But, according to Lewis (1973, 2), to say that something *might* happen is equivalent to saying that *it is not the case that it would not* happen; following DeRose (1999, 387), I will call this *the duality thesis*. If we accept the duality thesis, and that every infinite sequence might result from tossing the coin infinitely often, then there is no limiting relative frequency that *would* result from tossing the coin infinitely often and hence $mm_X(O)$ doesn't exist. This argument has been given by Lewis (1986, 90),Levi (1980, 258–59), and Hájek (2009, 220). If these authors are right then mm has essentially the same flaw as mh.

Another objection which has been raised against concepts like mm is that a world in which an experiment is repeated infinitely must be so very different to the actual world that there is no saying what would happen in it. For example, Hájek (2009, 216) has argued that in such a world the laws of nature would be different to those in the actual world and, as a result, the physical probabilities might be different to what they are in the actual world. If this is right then, even when the value of $pp_X(O)$ is uncontroversial, $mm_X(O)$ may be unknowable or not exist.

It isn't clear to me that either of these objections is correct. Perhaps the duality thesis is false, as DeRose (1999) has argued. And perhaps it can be argued that, when we speak of what would happen if X were performed infinitely, it is to be understood that the characteristics relevant to pp_X don't change. On the other hand, it also isn't just obvious that these objections are wrong, as is shown by the fact that these objections have been made by experts in the field. Therefore, a safe criticism of mm is that it is unclear. In fact, mm is less clear than physical probability, since even when people agree on the value of $pp_X(O)$ they disagree about the existence or value of $mm_X(O)$. This isn't surprising, since mm is defined using a counterfactual conditional and such conditionals are notoriously unclear. A good explicatum must at least be clearer than the explicandum, so mm is a poor explicatum for physical probability.

I conclude that von Mises' theory of probability, and various modifications one might make in an attempt to improve on it, all fail to adequately explicate physical probability.

7.3 The reference class problem

Frequency theories of probability have been subjected to many criticisms in the literature, other than the one I have just made; Hájek (2009) gives a compendium of such criticisms. I will here discuss just one of those criticisms; it is called "the reference class problem."

We have seen that in von Mises' theory probabilities are relative to a collective and in the modified theory they are relative to an experiment type. In some other frequency theories, probabilities are said to be relative to a class, called "the reference class." However, when we need to make a decision whose outcome depends on whether a certain proposition is true, we need a single number that we can use as the probability of this proposition. So, if we assume that the probabilities in the frequency theory are to be used for guiding decisions, we need to somehow fix the other term of the probability (the collective, or experiment type, or reference class) to obtain a single number. The question of how this should be done is the so-called "reference class problem." As applied to the modified theory, a more accurate name would be "experiment type problem" but I will here follow the terminology that has become familiar.

Let us assume that the aim of a frequency theory of probability is to explicate the concept of physical probability. The concept of physical probability is relative to an experiment type. Therefore, any adequate explicatum for it must also be relative to an experiment type. Consequently, the fact that the explicata in frequency theories are all relative to something isn't a relevant criticism of those theories; this feature of them is just what is should be. Of course, if the explicatum is relative to something other than an experiment type, such as a class or a sequence, then the theory can be criticized for being relative to the wrong kind of thing (as I did in Section 7.2.1), but that isn't what people mean when they speak of "the reference class problem."

The probabilities that should be used for guiding decisions are inductive probabilities given our evidence, or explications of those (Maher 2009). These need not be identifiable with any physical probability. In the example on page 1, the inductive probability of the coin landing heads, given your evidence, is 1/2, although there is no experiment type relative to which the physical probability of the coin landing heads is 1/2. Thus, the alleged "reference class problem" rests on a failure to distinguish between physical and inductive probability.[3]

7.4 Population probabilities

So far, the examples of physical probabilities that I have discussed have all concerned games of chance. However, von Mises and other frequency theorists also speak of what I will call "population probabilities"; a typical example is "the probability of a 40-year-old American male surviving to age 41." The characteristic feature of these so-called probabilities is that no experiment is mentioned, instead we are given a population, also called a "reference class"; in my example the population is American males aged 40.

I claim that all physical probabilities are relative to an experiment and hence that, if there is to be a physical probability of a 40-year-old American male surviving to age 41, some experiment that hasn't been mentioned must be understood. I will now consider what this experiment could be.

Von Mises (1941, 192) suggests that the experiment in this case would be recording, for individual American males aged 40, whether or not they survive to age 41. However, if that is a complete description of the experiment then it can be performed in different ways that can be expected to have different outcomes. For example, one could choose only individuals with a parent who died young or one could exclude such individuals. These different more specific ways of carrying out the experiment don't have the same physical probability of resulting in death before age 41 so, by SP, there is not a physical probability of the less specific experiment resulting in death before age 41.[4]

Even if the individuals are chosen randomly, the results could be influenced by interfering with the individuals and their environment. For example, if the recording of outcomes were combined with measures to reduce crime, encourage safe driving, and improve public health, the death rate can be expected to be lower than without those measures. Thus, if the experiment type leaves it open whether there is any such interference, SP again implies that there is not a physical probability of surviving to age 41.

Let us then take the experiment X to be *randomly choosing an American*

[3] Hájek (2007b, 572–74) argued that logical probability has its own "reference class problem" but his arguments are ones that I showed to be spurious in Section 6.3.

[4] Von Mises claimed that "the death of an insured person during his forty-first year does not give the slightest indication of what will be the fate of another who is registered next to him in the books of the insurance company, *regardless of how the company's list was prepared*" (1957, 23, emphasis added). But if the list is prepared with siblings together, for instance, then the death of one person is an indication that the next person on the list will also die; hence this claim of von Mises is wrong.

male aged 40 and not interfering in the person's activities or environment in a way that might affect survival; also let O be that the person lives to age 41. It may be that $pp_X(O)$ exists and this is perhaps what people mean, or should mean, when they speak of "the probability of a 40-year-old American male surviving to age 41."

A probability of the sort just described need not equal the proportion of individuals in the population that have the outcome in question. To see this, imagine that an urn contains a large number of dice and that once each year the urn is emptied, each die is tossed once, those that come up six are removed, and the remainder are put back in the urn. If all the dice are fair then the physical probability that a randomly chosen die in the urn will still be in the urn a year later is 5/6 but the proportion of dice in the urn with this property is not likely to be exactly 5/6. Similarly, the probability of a 40-year-old American male surviving to age 41, understood as in the previous paragraph, need not equal the proportion of 40-year-old American males who survive to age 41. Population statistics are a good guide to the associated physical probabilities but they aren't infallible.

What has been said here about probabilities in human populations applies equally to other kinds of populations. To take a very different example, also mentioned by von Mises (1957, 20), the probability that a molecule in a particular volume of gas will have a certain velocity would be more accurately expressed as the physical probability that randomly choosing one of these molecules, without doing anything that could alter its velocity, will give a molecule with the specified velocity.

Chapter 8

Theories of chance

Several philosophers of probability have presented theories of what they call "chance." There are some reasons to think that these are meant to be theories of chance that are not frequency theories. In this chapter I will discuss several theories of this kind.

8.1 Levi's theory

I begin with the theory of chance presented by Isaac Levi (1980, 1990).

8.1.1 Identification of the concept

Levi does not give an explicit account of what he means by "chance" but there are some reasons to think he means physical probability. For example, he says:

> The nineteenth century witnessed the increased use of notions of objective statistical probability or chance in explanation and prediction in statistical mechanics, genetics, medicine, and the social sciences. (1990, 120)

This shows that Levi regards "chance" as another word for "objective statistical probability," which suggests its meaning is a sense of the word "probability." Also, the nineteenth century scientific work that Levi here refers to used the word "probability" in a pre-existing empirical sense and thus was using the concept of physical probability.

However, there are also reasons to think that what Levi means by "chance" is *not* physical probability. For example:

- Levi (1990, 117, 120) speaks of plural "conceptions" or "notions" of chance, whereas there is only one concept of physical probability.
- Levi (1990, 142) criticizes theories that say chance is incompatible with determinism by saying "the cost is substantial and the benefit at best negligible." This criticism, in terms of costs and benefits, would be appropriate if "chance" meant a newly proposed concept but it is irrelevant

> if "chance" means the pre-existing ordinary language concept of physical probability. If "chance" means physical probability then the appropriate criticism is simply that linguistic usage shows that physical probability is compatible with determinism—as I argued in Section 2.3.

So, it is not clear that what Levi means by "chance" is physical probability. Nevertheless, I think it worthwhile to compare my account of physical probability with the account that is obtained by interpreting Levi's "chance" as if it meant physical probability. I will do that in the remainder of this section.

8.1.2 Form of statements

Levi (1990, 120) says:

> Authors like Venn (1866) and Cournot (1851) insisted that their construals of chance were indeed consistent with respect to underlying determinism ... The key idea lurking behind Venn's approach is that the chance of an event occurring to some object or system—a "chance set up," according to Hacking (1965), and an "object," according to Venn (1866, ch. 3)—is relative to the kind of trial or experiment (or "agency," according to Venn) conducted on the system.

Levi endorses this "key idea." The position I defended in Section 2.1 is similar in making physical probability relative to a type of experiment, but there is a difference. I represented statements of physical probability as relating three things: An experiment type (e.g., a human tossing a certain coin), an outcome type (e.g., the coin landing heads), and a number (e.g., 1/2). On Levi's account, chance relates four things: A chance set up (e.g., a particular coin), a type of trial or experiment (e.g., tossing by a human), an outcome type, and a number. Thus what I call an "experiment" combines Levi's "chance set up" and his "trial or experiment."

An experiment (in my sense) can often be decomposed into a trial on a chance set up in more than one way. For example, if the experiment is weighing a particular object on a particular scale, we may say:

- The set up is the scale and the trial is putting the object on it.
- The set up is the object and the trial is putting it on the scale.
- The set up is the object and scale together and the trial is putting the former on the latter.

These different analyses make no difference to the physical probability. Therefore, Levi's representation of physical probability statements, while perhaps adequate for representing all such statements, is more complex than it needs to be.

8.1.3 Specification

Since SP is a new principle, Levi was not aware of it. I will now point out two ways in which his theory suffers from this.

A mistaken example

To illustrate how chance is relative to the type of experiment, Levi (1990, 120) made the following assertion:

> The chance of coin a landing heads on a toss may be 0.5, but the chance of the coin landing heads on a toss by Morgenbesser may, at the same time, be 0.9.

But let X be tossing a (by a human), let X' be tossing a by Morgenbesser, and let O be that a lands heads. It is possible to perform X in a way that ensures it is also a performance of X' (just have Morgenbesser toss the coin), so SP implies that if $pp_X(O) = 0.5$ then $pp_{X'}(O)$ must have the same value. Levi, on the other hand, asserts that it could be that $pp_X(O) = 0.5$ and $pp_{X'}(O) = 0.9$.

Intuition supports SP here. If the physical probability of heads on a toss of a coin were different depending on who tosses the coin (as Levi supposes) then, intuitively, there would not be a physical probability for getting heads on a toss by an unspecified human, just as there is not a physical probability for getting a black ball on drawing a ball from an urn of unspecified composition. Thus, Levi's example is mistaken.

An inadequate explanation

Levi (1980, 264) wrote:

> Suppose box a has two compartments. The left compartment contains 40 black balls and 60 white balls and the right compartment contains 40 red balls and 60 blue balls. A trial of kind S is selecting a ball at random from the left compartment and a trial of kind S' is selecting a ball at random from the right compartment ... Chances are defined for both kinds of trials over their respective sample spaces [i.e., outcome types].
>
> Consider trials of kind $S \vee S'$. There is indeed a sample space consisting of drawing a red ball, a blue ball, a black ball, and a white ball. However, there is no chance distribution over the sample space.
>
> To see why no chance distribution is defined, consider that the sample space for trials of kind $S \vee S'$ is such that a result consisting of obtaining a [black] or a [white] ball is equivalent to obtaining a result of conducting a trial of kind S ... Thus, conducting a trial of kind $S \vee S'$ would be conducting a trial of kind S with some definite chance or statistical probability.

> There is no a priori consideration precluding such chances; but there is no guarantee that such chances are defined either. In the example under consideration, we would normally deny that they are.

Let O be that the drawn ball is either black or white. I agree with Levi that $pp_{S\vee S'}(O)$ doesn't exist. However, Levi's explanation of this is very shallow; it rests on the assertion that $pp_{S\vee S'}(S)$ doesn't exist, for which Levi has no explanation. It also depends on there not being balls of the same color in both compartments, though the phenomenon is not restricted to that special case; if we replaced the red balls by black ones, Levi's explanation would fail but $pp_{S\vee S'}(O)$ would still not exist.

SP provides the deeper explanation that Levi lacks. The explanation is that it is possible to perform $S \vee S'$ in a way that ensures S is performed, likewise for S', and $pp_S(O) \neq pp_{S'}(O)$, so by Theorem 2.1, $pp_{S\vee S'}(O)$ does not exist. In Levi's example, $pp_S(O) = 1$ and $pp_{S'}(O) = 0$; if the example is varied by replacing the red balls with black ones then $pp_{S'}(O) = 0.4$; the explanation of the non-existence of $pp_{S\vee S'}(O)$ is the same in both cases.

8.1.4 Independence

Levi considers a postulate equivalent to IN and argues that it doesn't hold in general. Here is his argument:

> [A person] might believe that coin a is not very durable so that each toss alters the chance of heads on the next toss and that how it alters the chance is a function of the result of the previous tosses. [The person] might believe that coin a, which has never been tossed, has a .5 chance of landing heads on a toss as long as it remains untossed. Yet, he might not believe that the chance of r heads on n tosses is $\binom{n}{r}(.5)^n$. (1980, 272)

The latter formula follows from IN and $pp_X(\text{heads}) = 0.5$.

Levi here seems to be saying that the chance of experiment type X giving outcome type O can be different for different tokens of X. He explicitly asserts that elsewhere:

> Sometimes kinds of trials are not repeatable on the same object or system ... And even when a trial of some kind can be repeated, the chances of response may change from trial to trial. (1990, 128)

But that is inconsistent with Levi's own view, according to which chance is a function of the experiment and outcome types.

In fact, IN is not violated by Levi's example of the non-durable coin, as the following analysis shows.

- We may take X to be starting with the coin symmetric and tossing it n times. Here repetition of X requires starting with the coin again symmetric, so different performances of X are independent, as IN requires.

This is similar to the example of drawing cards without replacement that I gave in Section 2.6.

- We may take X to be tossing the coin once when it is in such-and-such a state. Here repetition of X requires first restoring the coin to the specified state, so again different performances of X are independent.

- Levi seems to be taking X to be tossing the coin once, without specifying the state that the coin is in. In that case, pp_X(heads) does not exist, so again there is no violation of IN.

I conclude that Levi's objection to IN is fallacious.

8.1.5 Direct inference

Levi endorses a version of the direct inference principle; the following is an example of its application:

> If Jones knows that coin a is fair (i.e., has a chance of 0.5 of landing heads and also of landing tails) and that a is tossed at time t, what degree of belief or credal probability ought he to assign to the hypothesis that the coin lands heads at that time? Everything else being equal, the answer seems to be 0.5. (Levi 1990, 118).

As this indicates, Levi's direct inference principle concerns the degree of belief that a person ought to have. By contrast, the principle DI in Section 2.7 concerns inductive probability.

To understand Levi's version of the principle we need to know what it means to say that a person "ought" to have a certain degree of belief. Levi doesn't give any adequate account of this, so I am forced to make conjectures about what it means.

One might think that a person "ought" to have a particular degree of belief iff the person would be well advised to adopt that degree of belief. But if that is what it means, then Levi's direct inference principle is false. For example, Jones might know that coin a is to be tossed 100 times, and that the tosses are independent, in which case Levi's direct inference principle says that for each r from 0 to 100, Jones's degree of belief that the coin will land heads exactly r times ought to be $\binom{100}{r}(0.5)^{100}$. However, it would be difficult (if not impossible) to get one's degrees of belief in these 101 propositions to have precisely these values and, unless something very important depends on it, there are better things to do with one's time. Therefore, it is not always advisable to have the degrees of belief that, according to Levi's direct inference principle, one "ought" to have.

Alternatively, one might suggest that a person "ought" to have a particular degree of belief iff it is the only one that is justified by the person's evidence. But what does it mean for a person's degree of belief to be justified by the person's evidence? According to the deontological conception of justification, which Alston (1985, 60) said is used by most epistemologists, it means that the person is not blameworthy in having this degree of belief. On that account, the suggestion would be that a person "ought" to have a particular degree of belief iff the person would deserve blame for not having it. However, there need not be anything blameworthy about failing to have all the precise degrees of beliefs in the example in the preceding paragraph; so on this interpretation, Levi's direct inference principle is again false.

For a third alternative, we might say that a person "ought" to have a particular degree of belief in a particular proposition iff this degree of belief equals the inductive probability of the proposition given the person's evidence. On this interpretation, Levi's direct inference principle really states a relation between inductive probability and physical probability, just as DI does; the reference to a person's degree of belief is a misleading distraction that does no work and would be better eliminated.

So, my criticism of Levi's version of the direct inference principle is that it is stated in terms of the unclear concept of what a person's degree of belief "ought" to be, that on some natural interpretations the principle is false, and the interpretation that makes it true is one in which the reference to degree of belief is unnecessary and misleading. These defects are all avoided by DI.

8.1.6 Admissible evidence

As I noted in Section 2.7, DI by itself has no practical applications because we always have more evidence than just the experiment type and an R-proposition. For example, Jones, who is concerned with the outcome of a particular toss of coin a, would know not only that coin a is fair but also a great variety of other facts. It is therefore important to have an account of when additional evidence is admissible.

Levi's (1980, 252) response is that evidence is admissible if it is known to be "stochastically irrelevant," i.e., it is known that the truth or falsity of the evidence does not alter the physical probability. That is right, but to provide any substantive information it needs to be supplemented by some principles about what sorts of evidence are stochastically irrelevant; Levi provides no such principles.

By contrast, Theorems 2.3 and 2.4 provide substantive information about when evidence is admissible. Those theorems were derived from SP and IN, neither of which is accepted by Levi, so it is not surprising that he has nothing substantive to say about when evidence is admissible.

8.2 Lewis's theory

I will now discuss the theory of chance proposed by Lewis (1980, 1986). A related theory was proposed earlier by Mellor (1971), and other writers have subsequently expressed essentially the same views (Loewer 2004; Schaffer 2007), but I will focus on Lewis's version. The interested reader will be able to apply what I say here to those other theories.

8.2.1 Identification of the theory

According to Lewis (1986, 96–97), chance is a function of three arguments: a proposition, a time, and a (possible) world. He writes $P_{tw}(A)$ for the chance at time t and world w of A being true.

Lewis (1986, 95–97) says that the *complete theory of chance* for world w is the set of all conditionals that hold at w and are such that (1) the antecedent is a proposition about history up to a certain time, (2) the consequent is a proposition about chance at that time, and (3) the conditional is a "strong conditional" of some sort, such as the counterfactual conditional of Lewis (1973). He uses the notation T_w for the complete theory of chance for w. He also uses H_{tw} for the complete history of w up to time t. Lewis (1986, 97) argues that the conjunction $H_{tw}T_w$ implies all truths about chances at t and w.

Lewis's version of the direct inference principle, which he calls the *Principal Principle*, is:

> Let C be any reasonable initial credence function. Then for any time t, world w, and proposition A in the domain of P_{tw}, $P_{tw}(A) = C(A|H_{tw}T_w)$. (1986, 97)

Lewis (1986, 127) argues that if H_{tw} and the laws of w together imply A, then $H_{tw}T_w$ implies $P_{tw}(A) = 1$. It follows that if w is deterministic then P_{tw} cannot have any values other than 0 or 1. For example, in a deterministic world, the chance of any particular coin toss landing heads must be 0 or 1. Lewis accepts this consequence.

> If a determinist says that a tossed coin is fair, and has an equal chance of falling heads or tails, he does not mean what I mean when he speaks of chance. (1986, 120)

Nevertheless, prodded by Levi (1983), Lewis proposed an account of what a determinist does mean when he says this; he called it "counterfeit" chance. I will now explain this concept.

For any time t, the propositions $H_{tw}T_w$, for all worlds w, form a partition that Lewis (1986, 99) calls the *history-theory partition* for time t. Another way of expressing the Principal Principle is to say that the chance distribution at any time t and world w is obtained by conditioning any reasonable initial credence function on the element of the history-theory partition for t that holds at w. Lewis (1986, 120–121) claimed that the history-theory partition has the following qualities:

> (1) It seems to be a natural partition, not gerrymandered. It is what we get by dividing possibilities as finely as possible in certain straightforward respects.

(2) It is to some extent feasible to investigate (before the time in question) which cell of this partition is the true cell; but

(3) it is unfeasible (before the time in question, and without peculiarities of time whereby we could get news from the future) to investigate the truth of propositions that divide the cells.

With this background, Lewis states his account of counterfeit chance:

> Any coarser partition, if it satisfies conditions (1)–(3) according to some appropriate standards of feasible investigation and of natural partitioning, gives us a kind of counterfeit chance suitable for use by determinists: namely, reasonable credence conditional on the true cell of that partition. Counterfeit chances will be relative to partitions; and relative, therefore, to standards of feasibility and naturalness; and therefore indeterminate unless the standards are somehow settled, or at least settled well enough that all the remaining candidates for the partition will yield the same answers. (1986, 121)

So we can say that for Lewis, physical probability (the empirical concept of probability in ordinary language) is reasonable initial credence conditioned on the appropriate element of a suitable partition. It may be chance or counterfeit chance, depending on whether the partition is the history-theory partition or something coarser. I will now criticize this theory of physical probability.

8.2.2 Form of statements

Lewis says that chance is a function of three arguments: a proposition, a time, and a world. He does not explicitly say what the arguments of counterfeit chance are but, since he thinks this differs from chance only in the partition used, he must think that counterfeit chance is a function of the same three arguments, and hence (to put it in my terms) that physical probability is a function of these three arguments.

Let us test this on an example. Consider again the following typical statement of physical probability:

H: The physical probability of heads on a toss of this coin is 1/2.

Lewis (1986, 84) himself uses an example like this. However, H doesn't attribute physical probability to a proposition or refer to either a time or a possible world. So, this typical statement of physical probability does not mention any of the things that Lewis says are the arguments of physical probability.

Of course, it may nevertheless be that the statement could be analyzed in Lewis's terms. Lewis did not indicate how to do that, although he did say that when a time is not mentioned, the intended time is likely to be the time when the event in question begins (1986, 91). So we might try representing H as:

H': For all s and t, if s is a token toss of this coin and t is a time just prior to s then the physical probability at t in the actual world of the proposition that s lands heads is $1/2$.

But there are many things wrong with this. First, "s lands heads" is not a proposition, since s is here a variable. Second, H' is trivially true if the coin is never tossed, though H would still be false if the coin is biased, so they are not equivalent. Third, the physical probability of a coin landing heads is different depending on whether we are talking about tossing by a human, with no further specification (in which case H is probably true), or about tossing with such and such a force from such and such a position, etc. (in which case H is false), but H' doesn't take account of this. And even if these and other problems could be fixed somehow (which has not been done), the resulting analysis must be complex and its correctness doubtful. By contrast, my account is simple and follows closely the grammar of the original statement; I represent H as saying that the physical probability of the experiment type "tossing this coin" having the outcome type "heads" is $1/2$.

I will add that, regardless of what we take the other arguments of physical probability to be, there is no good reason to add a possible world as a further argument. Of course, the value of a physical probability depends on empirical facts that are different in different possible worlds, but this does not imply that physical probability has a possible world as an argument. The simpler and more natural interpretation is that physical probability is an empirical concept, not a logical one; that is, even when all the arguments of physical probability have been specified, the value is in general a contingent matter.

Lewis himself sometimes talks of physical probability in the way I am here advocating. For instance, he said that counterfeit chance is "reasonable credence conditional on the *true* cell of [a] partition" (emphasis added); to be consistent with his official view, he should have said that counterfeit chance *at w* is reasonable credence conditional on the cell of the partition *that holds at w*. My point is that the former is the simpler and more natural way to represent physical probability.

So, Lewis made a poor start when he took the arguments of physical probability to be a proposition, a time, and a world. That representation has not been shown to be adequate for paradigmatic examples, including Lewis's own, and even if it could be made to handle those examples it would still be needlessly complex and unnatural. The completely different representation that I proposed in Section 2.1 avoids these defects.

8.2.3 Reasonable credence

In Lewis's presentation of his theory, the concept of a "reasonable initial credence function" plays a central role. Lewis says this is "a non-negative, normalized, finitely additive measure defined on all propositions" that is

> reasonable in the sense that if you started out with it as your ini-

> tial credence function, and if you always learned from experience by conditionalizing on your total evidence, then no matter what course of experience you might undergo your beliefs would be reasonable for one who had undergone that course of experience. I do not say what distinguishes a reasonable from an unreasonable credence function to arrive at after a given course of experience. We do make the distinction, even if we cannot analyze it; and therefore I may appeal to it in saying what it means to require that C be a reasonable initial credence function. (1986, 88)

However, there are different senses in which beliefs are said to be reasonable and Lewis has not identified the one he means. A reasonable degree of belief could be understood as one that a person would be well advised to adopt, or that a person would be not be blameworthy for adopting, but on those interpretations Lewis's theory would give the wrong results, for the reasons indicated in Section 8.1.5. Alternatively, we might say that a reasonable degree of belief is one that agrees with inductive probability given the person's evidence, but then reasonable degrees of belief would often lack precise numeric values (Maher 2006) whereas Lewis requires a reasonable initial credence function to always have precise numeric values.

I think the best interpretation of Lewis here is that his "reasonable initial credence function" is a probability function that is a precisification of inductive probability given no evidence. This is compatible with the sort of criteria that Lewis (1986, 110) states and also with his view (1986, 113) that there are multiple reasonable initial credence functions.

Although Lewis allows for multiple reasonable initial credence functions, his Principal Principle requires them to all agree when conditioned on an element of the history-theory partition. So, if a reasonable initial credence function is a precisification of inductive probability, Lewis's theory of chance can be stated more simply and clearly using the concept of inductive probability, rather than the concept of a reasonable initial credence function, as follows:

> The chance of a proposition is its inductive probability conditioned on the appropriate element of the history-theory partition.

This shows that the concept of credence does no essential work in Lewis's theory of chance; hence Lewis's theory isn't subjectivist and (Lewis 1980) is mistitled.

What goes for chance also goes for counterfeit chance, and hence for physical probability in general. Thus Lewis's theory of physical probability may be stated as:

> The physical probability of a proposition is its inductive probability conditioned on the appropriate element of a suitable partition.

Again, the concept of credence is doing no essential work in Lewis's theory and clarity is served by eliminating it.

8.2.4 Partitions

We have seen that according to Lewis, physical probability is inductive probability conditioned on the appropriate element of a suitable partition. Also, suitable partitions are natural partitions such that it is "to some extent feasible to investigate (before the time in question) which cell of this partition is the true cell" but "unfeasible" to investigate the truth of propositions that divide the cells. Lewis says the history-theory partition is such a partition and using it gives genuine chance. Coarser partitions, using different standards of naturalness and feasibility, give what Lewis regards as counterfeit chance. I will now argue that Lewis is wrong about what counts as a suitable partition, both for chance and counterfeit chance.

I begin with chance. Let t be the time at which the first tritium atom formed and let A be the proposition that this atom still existed 24 hours after t. The elements of the history-theory partition specify the chance at t of A. But let us suppose, as might well be the case, that the only way to investigate this chance is to observe many tritium atoms and determine the proportion that decay in a 24 hour period. Then, even if sentient creatures could exist prior to t (which is not the case), it would not be feasible for them to investigate the chance at t of A, since there were no tritium atoms prior to t. Therefore, the history-theory partition does not fit Lewis's characterization of a suitable partition.

Now consider a case of what Lewis calls counterfeit chance. Suppose that at time t I bend a coin slightly by hammering it and then immediately toss it; let A be that the coin lands heads on this toss. If I assert that coin tossing is deterministic but the physical probability of this coin landing heads is not 0 or 1 then, according to Lewis, the physical probability I am talking about is inductive probability conditioned on the true element of a suitable partition that is coarser than the history-theory partition. Lewis has not indicated what that partition might be but this part of his theory is adapted from Jeffrey, who indicates (1983, 206) that the partition is one whose elements specify the limiting relative frequency of heads in an infinite sequence of tosses of the coin. However, there cannot be such an infinite sequence of tosses and, even if it existed, it is not feasible to investigate its limiting relative frequency prior to t. On the other hand, it is perfectly feasible to investigate many things that divide the cells of this partition, such as what I had for breakfast. Lewis says different partitions are associated with different standards of feasibility, but there is no standard of feasibility according to which it is feasible prior to t to investigate the limiting relative frequency of heads in an infinite sequence of non-existent future tosses, yet unfeasible to investigate what I had for breakfast. Hence this partition is utterly unlike Lewis's characterization of a suitable partition.

So, Lewis's characterization of chance and counterfeit chance in terms of partitions is wrong. This doesn't undermine his theory of chance, which is based on the Principal Principle rather than the characterization in terms of partitions, but it does undermine his theory of counterfeit chance. I will now diagnose the source of Lewis's error.

Lewis's original idea, expressed in his Principal Principle, was that inductive probability conditioned on the relevant chance equals that chance. That idea is basically correct, reflecting as it does the principle of direct inference. Thus what makes the history-theory partition a suitable one is not the characteristics that Lewis cited, concerning naturalness and feasibility of investigation; it is rather that each element of the history-theory partition specifies the value of the relevant chance. We could not expect the Principal Principle to hold if the conditioning proposition specified only the history of the world to date and not also the relevant chance values for a world with that history. Yet, that is essentially what Lewis tries to do in his theory of counterfeit chance. No wonder it doesn't work.

So if counterfeit chance is to be inductive probability conditioned on the appropriate element of a suitable partition, the elements of that partition must specify the (true!) value of the counterfeit chance. But then it would be circular to explain what counterfeit chance is by saying that it is inductive probability conditioned on the appropriate element of a suitable partition. Therefore, counterfeit chance cannot be explained in this way—just as chance cannot be explained by saying it is inductive probability conditioned on the appropriate element of the history-theory partition. Thus the account of counterfeit chance, which Lewis adopted from Jeffrey, is misguided.

The right approach is to treat what Lewis regards as genuine and counterfeit chance in a parallel fashion. My account of physical probability does that. On my account, Lewis's chances are physical probabilities in which the experiment type specifies the whole history of the world up to the relevant moment, and his counterfeit chances are physical probabilities in which the experiment type is less specific than that. Both are theoretical entities, the same principle of direct inference applies to both, and we learn about both in the same ways.

Chapter 9

Subjective theories

In recent decades, many authors have endorsed some kind of subjective theory of probability, though it is often unclear exactly what theory a given author intends to endorse and different authors endorse different theories. Therefore, this chapter will discuss a variety of possible subjective theories of probability, rather than focusing on any particular author's theory.

This book is concerned with describing and explicating the concepts of probability in ordinary language. Therefore, I am here only interested in theories of probability that purport to do one or other of these things. I call theories of the first type *descriptive theories* and theories of the second type *explications*. I think it is a legitimate question whether a theory that doesn't claim to either describe or explicate an ordinary concept of probability even deserves to be called a theory *of probability*; however, since that is just a semantic question, I won't pursue it here.

Let a *subjective concept* be a concept that is relative to a subject (in the sense of a person or mind); an example is the concept of degree of belief. I will call a descriptive theory of probability subjective if it says that some ordinary concept of probability is subjective; for example, the claim that inductive probability is degree of belief is a subjective descriptive theory of probability. I will call an explication of probability subjective if it proposes a subjective concept as an explicatum for an ordinary concept of probability; for example, the proposal of degree of belief as an explicatum for inductive probability is a subjective explication of probability.

The question to be discussed in this chapter, then, is whether there is any correct subjective descriptive theory of probability or any satisfactory subjective explication of probability. I will argue that there is not.

9.1 Descriptive theories

According to many accounts, "the subjective theory of probability" asserts that probability is degree of belief.[1] It would be natural to interpret this as saying that "probability" in ordinary English means degree of belief. However, that seems to assume that "probability" in ordinary English has only one meaning, which isn't the case. To avoid that objection, we might take the view to be that degree of belief is *one* of the meanings that "probability" has in ordinary English. But we saw in Section 1.1 that inductive probability isn't the same thing as degree of belief and the considerations adduced there can easily be adapted to show that "probability" in ordinary language *never* means degree of belief. Therefore, this theory is false.

Subjectivists who begin by saying that probability is degree of belief often segue into saying that it is *rational* degree of belief, or arguing as if that is what they meant to say. Sometimes other terms are used in place of "rational," such as "coherent" or "consistent," but it usually seems to be assumed that a rational person's degrees of belief would be coherent, or consistent. In any case, I will here use "rational" merely as a placeholder for whatever condition subjectivists intend to impose. Let us also suppose, for definiteness, that what is meant by "probability" here is inductive probability. Then the theory we are considering is:

Theory 9.1. *Inductive probability is rational degree of belief.*

According to this theory,

$$\text{The inductive probability that the coin will land heads is } 1/2 \tag{9.1}$$

means the same as

$$\text{The rational degree of belief that the coin will land heads is } 1/2. \tag{9.2}$$

However, if Theory 9.1 is to be a *subjective* theory, the term "rational" must be understood in such a way that there is in general no uniquely rational degree of belief in a proposition for a person with given evidence; different people with the same evidence can have different degrees of belief without being irrational. Therefore, (9.2) is incomplete since it doesn't specify *whose* rational degree of belief we are talking about. Since (9.1) isn't incomplete in that way, these statements don't mean the same and Theory 9.1 is false.

What subjectivists seem to have in mind is that the person whose degree of belief is relevant is the one who made the statement of probability. And since we are requiring the degrees of belief to be rational, what we seem to get from this is:

Theory 9.2. *Statements of inductive probability refer to the speaker's degree of belief, provided the speaker is rational.*

[1] See, for example, Weatherford (1982, 219), Gillies (2000, 1), Jeffrey (2004, xi), Howson and Urbach (2006, 8), and Hájek (2007a, sec. 3.5.1).

Now subjectivists always say that a rational person's degrees of belief satisfy the laws of probability. On the other hand, real people's degrees of belief often violate those laws and yet real people can make meaningful statements of inductive probability. Therefore, Theory 9.2 is at best incomplete in failing to account for statements of inductive probability by irrational people. In addition, if "rational" is understood in a subjectivist sense, then Theory 9.2 isn't even a correct account of the statements of inductive probability by rational people, for the sorts of reasons given in Section 1.1.

Generalizing from this discussion, we may say that the considerations advanced in Section 1.1 show that the concept of inductive probability isn't a subjective concept. Parallel considerations show the same for physical probability. Therefore, *all* subjectivist descriptive theories of probability are false.

9.2 Satisfaction theories

Perhaps the slogan "probability is degree of belief" isn't intended to give a meaning of "probability" in ordinary English. It might instead be just a misleading way of asserting:

Theory 9.3. *Degrees of belief satisfy the laws of probability.*

But this is false (Kahneman et al. 1982) and subjectivists concede that it is false. What subjectivists argue for is:

Theory 9.4. *The degrees of belief of a rational person satisfy the laws of probability.*

This may or may not be true, depending on what is meant by "rational."

I call these *satisfaction theories* because they merely claim that some concept satisfies the laws of probability. They don't claim to *describe* any ordinary concept of probability, nor do they propose an *explicatum* for an ordinary concept of probability.

It sometimes seems to be assumed that if a function satisfies the laws of probability then it is a kind of probability; if that were so, the truth of Theory 9.4 would imply that the degrees of belief of a rational person are a kind of probability. However, not every function that satisfies the laws of probability is a probability concept of ordinary language. As Kolmogorov (1933, 1) remarked:

> Every axiomatic (abstract) theory admits, as is well known, of an unlimited number of concrete interpretations besides those from which it was derived. Thus we find applications [of the calculus of probability] in fields of science which have no relation to the concepts of random event and of probability in the precise meaning of those words.

For example, Kolmogorov's axioms for probability are satisfied if we take his elementary events to be disjoint regions of space and take the probability function to measure the normalized volume of a set of these points, though "probability" in ordinary language never means normalized volume. Also, truth values (1 for true and 0 for false) satisfy the laws of probability, though "probability" in ordinary language never means truth value.

Thus, satisfaction theories neither state nor imply either a description or an explication of any ordinary concept of probability. Hence, they aren't theories of probability of the kind I am here concerned with.

9.3 Decision theory

The remainder of this chapter will consider subjective *explications* of an ordinary concept of probability. Now many subjectivists grant that there is an objective concept of probability, similar to what I call physical probability, in addition to their subjective concept;[2] therefore, if we interpret them as proposing a subjective explication, it is most natural to take their explicandum to be inductive probability. So, to avoid making chapter excessively long, I will here consider only subjective explications in which the explicandum is inductive probability.

In order for an explication to be satisfactory, the explicatum it proposes must satisfy the desiderata for an explicatum, one of which is that it be capable of serving the same purposes as the explicandum. Therefore, evaluation of an explication of inductive probability requires consideration of the purposes that inductive probability serves. We are often interested in the values of inductive probabilities out of curiosity, with no application in mind, but this is not a purpose served by inductive probability and doesn't explain why we are so interested in inductive probabilities. I take it that the main purpose served by inductive probability is its use in guiding decision making, so I will now give an account of how inductive probability is used for this purpose.

There are at least two different senses in which a choice is said to be rational. I will call a choice *absolutely rational* if it could be made by an ideal agent who has the same empirical evidence that the real agent has. On the other hand, I will say that a choice is *deontologically rational* if the agent would not deserve blame for making that choice. The following example illustrates the difference between these concepts.[3]

> Andrew must decide whether to bet at even odds that a double six will be obtained in 24 tosses of a pair of fair dice. He is unable

[2] Examples include Ramsey (1926), Lewis (1980), Levi (1980), Gillies (2000), and Howson and Urbach (2006).

[3] This example is loosely based on a problem mentioned by Pascal in one of his letters to Fermat; see Ore (1960) for the historical details. In this example, and also in others that I will give later, I assume that the value of the outcomes is a linear function of the amount of money received.

> to calculate the probability of this in the time available but he recollects that the probability of getting a six in four tosses of a single die is greater than 1/2 and he notices that 24 is to 36 (the number of possible outcomes of tossing two dice) as 4 is to 6 (the number of possible outcomes of tossing one die). He knows that according to an old gambling rule, it follows that the probability of winning the bet is greater than 1/2 and so he accepts the bet.

Andrew's choice isn't absolutely rational because an ideal agent with Andrew's evidence would have determined that the probability of getting the double six is actually a little less than 1/2. On the other hand, Andrew's choice is deontologically rational, since he did the best he could under the circumstances.

Inductive probability can be used to determine which choices are absolutely rational in a decision problem. The first step is to represent the problem in a suitable manner. We do this by identifying the following:

- A set of *acts* such that the agent must choose one and only one of them.
- A set of *consequences* which could result from choosing one or other of these acts; each consequence must include every aspect of the outcome that is relevant to its value.
- A set of propositions, called *states of the world* or simply *states*, which satisfy the following conditions: (i) the agent's evidence implies that one and only one state is true, (ii) the true state together with the act chosen determines the consequence obtained, and (iii) the agent's choice has no influence on which state is true.

Let the agent's evidence be E and suppose that, for each state S, $ip(S|E)$ has a numeric value. Suppose further that for each consequence there is a numeric measure of the value of that consequence for the agent; this measure is called *utility* and I will use $u(a, S)$ to denote the utility of the consequence that would be obtained if a is chosen and the true state is S. Then the *expected utility* of an act a (for this agent) is defined to be

$$EU(a) = \sum_{S} ip(S|E)u(a, S).$$

Here the sum is over all the states S. An act is said to *maximize expected utility* if its expected utility is at least as great as that of any other available act. Finally, an act is absolutely rational iff it maximizes expected utility.

I will now illustrate these concepts by applying them to Andrew's decision problem. We may take the acts available to Andrew to be "accept the bet" and "reject the bet"; I will denote these as a and $\sim a$, respectively. Assuming that the monetary gain or loss is the only thing of value to Andrew that could be affected by the decision, and supposing the amount bet to be \$100, we may take the consequences to be "win \$100," "lose \$100," and "status quo"; let us suppose that these have utilities of 100, -100, and 0, respectively. The states can be identified as "at least one double six" and "no double six"; I will denote them as S_1 and S_0, respectively. Thus we have $u(a, S_1) = 100$, $u(a, S_2) = -100$, and $u(\sim a, S_1) = u(\sim a, S_2) = 0$.

Let X be the experiment of tossing two fair dice 36 times and let O be the outcome of getting at least one double six. Let R be the proposition that the dice are fair, that is to say, the physical probability of getting six on a toss of either die is 1/6. By IN (Section 2.6), R implies that $pp_X(O) = 1-(35/36)^{24} = 0.49$ (to two decimal places). So, by DI (Section 2.7), $ip(Oa|Xa.R) = 0.49$, where a is the token event on which Andrew is offered the bet. I assume that Andrew's evidence (call it E) includes Xa and R, and he has no evidence that is inadmissible with respect to (X, O, R, a). Since Oa is S_1, it follows that $ip(S_1|E) = 0.49$ and hence $ip(S_2|E) = 0.51$. So we have:

$$\begin{aligned} EU(a) &= (0.49)(100) + (0.51)(-100) = -2. \\ EU(\sim a) &= (0.49)(0) + (0.51)(0) = 0. \end{aligned}$$

Thus rejecting the bet maximizes expected utility and hence is the absolutely rational choice.

In this example there is only one act that maximizes expected utility but in general there may be more than one, in which case the choice of any of those acts is absolutely rational.

So far I have been assuming that $ip(S|E)$ has a numeric value for each state S. When this is false, it may still be that the inductive probabilities satisfy inequalities which suffice to determine which acts maximize expected utility.[4] For example, there is no numeric inductive probability that the sun will rise tomorrow, given my evidence, but it is clear that this probability is much greater than 1/2 and hence that betting at even odds that the sun will rise tomorrow has higher expected utility than not betting. In such cases, inductive probability can be used to determine the absolutely rational choices, even though the inductive probabilities lack numeric values.

9.4 Explications

A naive subjective explication of inductive probability would be the following.

Theory 9.5. *Degree of belief is proposed as an explicatum for inductive probability.*

But, since degrees of belief in a given proposition vary from person to person, and Theory 9.5 doesn't indicate whose degrees of belief are to be used, this theory doesn't even succeed in identifying a definite explicatum.

When calculating expected utility in decision theory, we used inductive probabilities conditioned on the agent's evidence at the time of the decision.

[4] This is the case if, for every numeric function p satisfying these inequalities, substituting it for inductive probability in the definition of expected utility gives the same set of acts that maximize expected utility.

The obvious subjective analog of this is the agent's degrees of belief at the time of the decision. This suggests:

Theory 9.6. *The agent's degree of belief in proposition A at time t is proposed as an explicatum for the inductive probability of A given the agent's evidence at t.*

This does succeed in identifying a definite explicatum for a restricted class of inductive probabilities; I will now examine whether it is a satisfactory explicatum.

Let the *subjective expected utility* (SEU) of an act be its expected utility calculated using the agent's degrees of belief in the states, rather than the inductive probabilities of the states given the agent's evidence. If Theory 9.6 is satisfactory then SEU could be used in place of EU to determine the absolutely rational choices in a decision problem. But this is not the case, for at least two reasons.

First, real agents' degrees of belief are often so vague that there is no fact as to which acts maximize SEU, even when there is a fact about which acts maximize EU. For example, in Andrew's decision problem, many people would not have degrees of belief in the states sufficiently precise to determine which act maximizes SEU, though the inductive probabilities given the stated evidence do have numerically precise values. In such cases, the explicatum in Theory 9.6 cannot be used to determine which choices are absolutely rational, though the explicandum can.

Second, even if the agent's degrees of belief are precise, they may differ from the inductive probabilities given the agent's evidence, in which case the acts that maximize SEU may be absolutely irrational. Andrew was an example of this; here is another example:

> Belinda has a high degree of belief that a die will land six on the next toss, for no reason except that she has a hunch this will happen, and despite the fact that most of her hunches have been incorrect in the past. On this basis, Belinda bets at even odds that the die will land six on the next toss.

Belinda's decision isn't absolutely rational and, unlike Andrew's, it isn't even deontologically rational, since she can rightly be blamed for putting so much weight on her hunch, especially since she knows her poor track record with hunches. Nevertheless, Belinda's decision does maximize SEU.

It may be thought that these objections to Theory 9.6 can be avoided by adding a qualification to the effect that the agent's degrees of belief are rational. So let us consider:

Theory 9.7. *The agent's degree of belief in proposition A at time t is proposed as an explicatum for the inductive probability of A given the agent's evidence at t, provided the agent is rational.*

For this proposal to be satisfactory, the term "rational" must be understood in such a way that, whenever the act that maximizes expected utility using Theory 9.6 is irrational, the agent is irrational. For example, both Andrew and Belinda must be deemed irrational. But then, in all such cases Theory 9.7 has the result that the explicatum for inductive probability is undefined and hence we cannot use it to determine which acts are rational. On the other hand, inductive probabilities exist in these cases and can be used to determine which acts are rational. Hence, Theory 9.7 doesn't provide an explicatum that can play the same role in decision theory that inductive probability plays.

Another objection to Theory 9.7 is that, even when the agent is rational in the relevant sense, the agent's degrees of belief may still be too vague to be useful for calculating expected utility, even when the inductive probabilities are sufficiently precise for that purpose. So again, the explicatum isn't an adequate substitute for its explicandum.

An alternative approach is:

Theory 9.8. *The degree of belief that is rational for the agent to have in A at time t is proposed as an explicatum for the inductive probability of A given the agent's evidence at t.*

This avoids the first objection to Theory 9.7 because with Theory 9.8 the explicatum can exist even if the agent isn't rational. However, if "rational" is understood in such a way that there is a unique degree of belief in A that is rational for the agent at time t then Theory 9.8 isn't a subjective theory. And if "rational" isn't understood in such a way, then the explicatum proposed by Theory 9.8 doesn't exist. Either way, Theory 9.8 isn't a satisfactory subjective explication of inductive probability.

I have now considered all the subjective explications of inductive probability that are suggested in the literature or that I could think of and found them all to be unsatisfactory. Furthermore, since inductive probability isn't a subjective concept, it is implausible that any subjective concept could serve the same functions as inductive probability does, and hence implausible that there could be a satisfactory subjective explication of inductive probability.

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