LAWS: A RANKING THEORETIC ACCOUNT

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Preliminary Remarks

This is my reading material for the 5th annual formal epistemology workshop at UW Madison, May 14-18, 2008. I shall try to condense the essentials of this material into a 45 minutes talk. Here, however, I would like to present my full material that mainly consists of chapter 12 of my book on ranking theory that I am about to write. The chapter is not self-contained and requires at least an introduction into ranking theory. For this reason I have prefixed such an introduction that I have taken from another paper of mine. All in all, that makes for a somewhat uneven reading, but the text should be complete as far as it goes. It also makes for an uneven numbering, which I have only slightly adapted. So, please keep in mind that this is a mixture of two texts, each with its own introduction. You may skip section 12.6, and you need not read all the details and proofs of section 12.5.

Content:

1. An Introduction into Ranking Theory

1.1 Introduction

Epistemology is concerned with the fundamental laws of thought, belief, or judgment. It may inquire the fundamental relations among the objects or contents of thought and belief, i.e., among propositions or sentences. Then we enter the vast realm of formal logic. Or it may inquire the activity of judging or the attitude of believing itself. Often, we talk as if this would be an affair of yes or no. From time immemorial, though, we know that judgment is firm or less than firm, that belief is a matter of degree. This insight opens another vast realm of formal epistemology.

Logic received firm foundations already in ancient philosophy. It took much longer, though, until the ideas concerning the forms of (degrees of) belief acquired more definite shape. Despite remarkable predecessors in Indian, Greek, Arabic, and medieval philosophy, the issue seemed to seriously enter the agenda of intellectual history only in 16th century with the beginning of modern philosophy. Cohen (1980) introduced the wieldy, though somewhat tendentious opposition between Baconian and Pascalian probability. This suggests that the opposition was already perceivable with the work of Francis Bacon (1561-1626) and Blaise Pascal (1623-1662). In fact, philosophers were struggling to find the right mould. In that struggle, Pascalian probability, which *is* probability *simpliciter*, was the first to take a clear and definite shape, viz. in the middle of 17th century (cf. Hacking 1975), and since then it advanced triumphantly. The extent to which it interweaves with our cognitive enterprise has become nearly total (cf. the marvelous collection of Krüger et al. 1987). There certainly were alternative ideas. However, probability theory was always far ahead; indeed, the distance ever increased. The winner takes it all!

I use 'Baconian probability' as a collective term for the alternative ideas. This is legitimate since there are strong family resemblances among the alternatives. Cohen has chosen an apt term since it gives historical depth to ideas that can be traced back at least to Bacon (1620) and his powerful description of 'the method of lawful induction'. Jacob Bernoulli and Johann Heinrich Lambert struggled with a non-additive kind of probability. When Joseph Butler and David Hume speak of probability, they often seem to have something else or more general in mind than our precise explication. In contrast to the German Fries school British 19th century's philosophers like John Herschel, William Whewell, and John Stuart Mill elaborated non-probabilistic methods of inductive inference. And so forth.[1]

Still, one might call this an underground movement. The case of alternative forms of belief became a distinct hearing only in the second half of the 20th century. On the one hand, there were scattered attempts like the 'functions of potential surprise' of Shackle (1949), heavily used and propagated in the epistemology of Isaac Levi since his (1967), Rescher's (1964) account of hypothetical reasoning, further developed in his (1976) into an account of plausible reasoning, or Cohen's (1970) account of induction which he developed in his (1977) under the label 'Non-Pascalian probability', later on called 'Baconian'. On the other hand, one should think that modern philosophy of science with its deep interest in theory confirmation and theory change produced alternatives as well. Indeed, Popper's hypothetical-deductive method proceeded non-probabilistically, and Hempel (1945) started a vigorous search for a qualitative confirmation theory. However, the former became popular rather among scientists than among philosophers, and the latter petered out after 25 years.

I perceive all this rather as prelude, preparing the grounds. The outburst came only in the mid 70's, with strong help from philosophers, but heavily driven by the needs of Artificial Intelligence. Not only deductive, but also inductive reasoning had to be implemented in the computer, probabilities appeared intractable[2], and thus a host of alternative models were invented: a plurality of default logics, non-monotonic logics and defeasible reasonings, fuzzy logic as developed by Zadeh (1975, 1978), possibility theory as initiated by Zadeh (1978) and developed by Dubois, Prade (1988), the Dempster-Shafer belief functions originating from Dempster (1967, 1968), but essentially generalized by Shafer (1976), AGM belief revision theory (cf. Gärdenfors 1988), a philosophical contribution with great success in the AI market, and so forth. The field has become rich and complex. There are attempts of unification like Halpern (2003) and huge handbooks like Gabbay et al. (1994). One hardly sees the wood for trees. It seems that what had been forgotten for centuries had to be made good for within decades.

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[1] This is not the place for a historical account. See, e.g., Cohen (1980) and Shafer (1978) for some details.

[2] Only Pearl (1988) showed how to systematically deal with probabilities without exponential computational explosion.

Ranking theory, first presented in Spohn (1983, 1988)[3], belongs to this field as well. Since its development, by me and others, is scattered in a number of papers, one goal of the present paper is to present an accessible survey of the present state of ranking theory. This survey will emphasize the philosophical applications, thus reflecting my bias towards philosophy. My other goal is justificatory. Of course, I am not so blinded to claim that ranking theory would be *the* adequate account of Baconian probability. As I said, 'Baconian probability' stands for a collection of ideas united by family resemblances; and I shall note some of the central resemblances in the course of the paper. However, there is a multitude of epistemological purposes to serve, and it is entirely implausible that there is one account to serve all. Hence, postulating a reign of probability is silly, and postulating a duumvirate of probability and something else is so, too. Still, I am not disposed to see ranking theory as just one offer among many. On many scores, ranking theory seems to me to be superior to rival accounts, the central score being the notion of *conditional* ranks. I shall explain what these scores are, thus trying to establish ranking theory as one particularly useful account of the laws of thought.

The plan of the paper is simple. In the five sections of part 2, I shall outline the main aspects of ranking theory. This central part will take some time. I expect the reader to get impatient meanwhile; you will get the compelling impression that I am not presenting an alternative to (Pascalian) probability, as the label 'Baconian' suggests, but simply probability itself in a different disguise. This is indeed one way to view ranking theory, and a way, I think, to understand its virtues. However, the complex relation between probability and ranking theory, though suggested at many earlier points, will be systematically discussed only in the two sections of part 3. The two sections of part 4 will finally compare ranking theory to some other accounts of Baconian probability. (Only three sections of part 2 are reproduced here.)

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[3] There I called its objects ordinal conditional functions. Goldszmidt, Pearl (1996) started calling them ranking functions, a usage I happily adapted.

1.2 Basics

We have to start with fixing the objects of the cognitive attitudes we are going to describe. This is a philosophically highly contested issue, but here we shall stay conventional without discussion. These objects are pure contents, i.e., propositions. To be a bit more explicit: We assume a non-empty set W of mutually exclusive and jointly exhaustive possible worlds or *possibilities*, as I prefer to say, for avoiding the grand associations of the term 'world' and for allowing to deal with *de se* attitudes and related phenomena (where doxastic alternatives are considered to be centered worlds rather than worlds). And we assume an algebra $\mathcal{A}$ of subsets of W, which we call *propositions*. All the functions we shall consider for representing doxastic attitudes will be functions defined on that algebra $\mathcal{A}$.

Thereby, we have made the philosophically consequential decision of treating doxastic attitudes as intensional. That is, when we consider sentences such as "*a* believes (with degree *r*) that *p*", then the clause *p* is substitutable salva veritate by any clause *q* expressing the same proposition and in particular by any logically equivalent clause *q*. This is so because by taking propositions as objects of belief we have decided that the truth value of such a belief sentence depends only on the proposition expressed by *p* and not on the particular way of expressing that proposition. The worries raised by this decision are not our issue.

The basic notion of ranking theory is very simple:

Definition 1: Let $\mathcal{A}$ be an algebra over W. Then κ is a *negative ranking function*[4] for $\mathcal{A}$ iff κ is a function from $\mathcal{A}$ into $\mathbf{R}^* = \mathbf{R}^+ \cup \{\infty\}$ (i.e., into the set of non-negative reals plus infinity) such that for all $A, B \in \mathcal{A}$:

(1) $\kappa(W) = 0$ and $\kappa(\emptyset) = \infty$,

(2) $\kappa(A \cup B) = \min\{\kappa(A), \kappa(B)\}$ [*the law of disjunction* (*for negative ranks*)].

$\kappa(A)$ is called the (*negative*) *rank* of A.

It immediately follows for each $A \in \mathcal{A}$:

(3) either $\kappa(A) = 0$ or $\kappa(\bar{A}) = 0$ or both [*the law of negation*].

[4] For systematic reasons I am slightly rearranging my terminology from earlier papers. I would be happy if the present terminology became the official one.

A negative ranking function κ, this is the standard interpretation, expresses a *grading of disbelief* (and thus something negative, hence the qualification). If $\kappa(A) = 0$, A is not disbelieved at all; if $\kappa(A) > 0$, A is disbelieved to some positive degree. Belief in A is the same as disbelief in $\overline{A}$; hence, A is *believed* in κ iff $\kappa(\overline{A}) > 0$. This entails (via the law of negation), but is not equivalent to $\kappa(A) = 0$. The latter is compatible also with $\kappa(\overline{A}) = 0$, in which case κ is neutral or unopinionated concerning A. We shall soon see the advantage of explaining belief in this indirect way via disbelief.

A little example may be instructive. Let us look at Tweetie of which default logic is very fond. Tweetie has, or fails to have, each of the three properties: being a bird (B), being a penguin (P), and being able to fly (F). This makes for eight possibilities. Suppose you have no idea what Tweetie is, for all you know it might even be a car. Then your ranking function may be the following one, for instance:[5]

κ	$B \& \overline{P}$	$B \& P$	$\overline{B} \& \overline{P}$	$\overline{B} \& P$
F	0	5	0	25
$\overline{F}$	2	1	0	21

In this case, the strongest proposition you believe is that Tweetie is *either* no penguin and no bird ($\overline{B} \& \overline{P}$) *or* a flying bird and no penguin ($F \& B \& \overline{P}$). Hence, you neither believe that Tweetie is a bird nor that it is not a bird. You are also neutral concerning its ability to fly. But you believe, for instance: if Tweetie is a bird, it is not a penguin and can fly ($B \rightarrow \overline{P} \& F$); and if Tweetie is not a bird, it is not a penguin ($\overline{B} \rightarrow \overline{P}$) – each if-then taken as material implication. In this sense you also believe: if Tweetie is a penguin, it can fly ($P \rightarrow F$); and if Tweetie is a penguin, it cannot fly ($P \rightarrow \overline{F}$) – but only because you believe that it is not a penguin in the first place; you simply do not reckon with its being a penguin. If we understand the if-then differently, as we shall do later on, the picture changes. The large ranks in the last column indicate that you strongly disbelieve that penguins are not birds. And so we may discover even more features of this example.

What I have explained so far makes clear that we have already reached the first fundamental aim ranking functions are designed for: the *representation of belief*.

[5] I am choosing the ranks in an arbitrary, though intuitively plausible way (just as I would have to arbitrarily choose plausible subjective probabilities, if the example were a probabilistic one). The question how ranks may be measured will be taken up in section 2.3.

Indeed, we may define $\mathcal{B}_\kappa = \{A \mid \kappa(\overline{A}) > 0\}$ to be the *belief set* associated with the ranking function κ. This belief set is finitely *consistent* in the sense that whenever $A_1,\ldots,A_n \in \mathcal{B}_\kappa$, then $A_1 \cap \ldots \cap A_n \neq \emptyset$; this is an immediate consequence of the law of negation. And it is finitely *deductively closed* in the sense that whenever $A_1,\ldots,A_n \in \mathcal{B}_\kappa$ and $A_1 \cap \ldots \cap A_n \subseteq B \in \mathcal{A}$, then $B \in \mathcal{B}_\kappa$; this is an immediate consequence of the law of disjunction. Thus, belief sets just have the properties they are normally assumed to have. (The finiteness qualification is a little cause for worry that will be addressed soon.)

There is a big argument about the rationality postulates of consistency and deductive closure; we should not enter it here. Let me only say that I am disappointed by all the attempts I have seen to weaken these postulates. And let me point out that the issue was essentially decided at the outset when we assumed belief to operate on propositions or truth-conditions or sets of possibilities. With these assumptions we ignore the relation between propositions and their sentential expressions or modes of presentation; and it is this relation where all the problems hide.

When saying that ranking functions represent belief I do not want to further qualify this. One finds various notions in the literature, full beliefs, strong beliefs, weak beliefs, one finds a distinction of acceptance and belief, etc. In my view, these notions and distinctions do not respond to any settled intuitions; they are rather induced by various theoretical accounts. Intuitively, there is only one perhaps not very clear, but certainly not clearly subdivisible phenomenon which I exchangeably call believing, accepting, taking to be true, etc.

However, if the representation of belief were our only aim, belief sets or their logical counterparts as developed in doxastic logic (see already Hintikka 1962) would have been good enough. What then is the purpose of the ranks or degrees? Just to give another account of the intuitively felt fact that belief is graded? But what guides such accounts? Why should the degrees of belief behave like ranks as defined? Intuitions by themselves are not clear enough to provide this guidance. Worse still, intuitions are usually tainted by theory; they do not constitute a neutral arbiter. Indeed, problems already start with the intuitive conflict between representing belief and representing degrees of belief. By talking of belief *simpliciter*, as I have just insisted, I seem to talk of *ungraded* belief.

The only principled guidance we can get is a theoretical one. The degrees must serve a clear theoretical purpose and this purpose must be shown to entail their behavior. For me, the theoretical purpose of ranks is unambiguous; this is why I invented them. It is the *representation of the dynamics of belief*; that is the second fundamental aim we pursue. How this aim is reached and why it can be reached in no other way will unfold in the course of this part of the paper. This point is essential; as we shall see, it distinguishes ranking theory from all similarly looking accounts, and it grounds its superiority.

For the moment, though, let us look at a number of variants of definition 1. Above I mentioned the finiteness restriction of consistency and deductive closure. I have always rejected this restriction. An inconsistency is irrational and to be avoided, be it finitely or infinitely generated. Or, equivalently, if I take to be true a number of propositions, I take their conjunction to be true as well, even if the number is infinite. If we accept this, we arrive at a somewhat stronger notion:

Definition 2: Let $\mathcal{A}$ be a complete algebra over W (closed also under infinite Boolean operations). Then κ is a *complete negative ranking function* for $\mathcal{A}$ iff κ is a function from W into $\mathbf{N}^+ = \mathbf{N} \cup \{\infty\}$ (i.e., into the set of non-negative integers plus infinity) such that $\kappa^{-1}(0) \neq \emptyset$ and and $\kappa^{-1}(n) \in \mathcal{A}$ for each $n \in \mathbf{N}^+$. κ is extended to propositions by defining $\kappa(\emptyset) = \infty$ and $\kappa(A) = \min\{\kappa(w) \mid w \in A\}$ for each non-empty $A \in \mathcal{A}$.

Obviously, the propositional function satisfies the laws of negation and disjunction. Moreover, we have for any $\mathcal{B} \subseteq \mathcal{A}$:

(4) $\kappa(\bigcup \mathcal{B}) = \min \{\kappa(B) \mid B \in \mathcal{B}\}$ [*the law of infinite disjunction*].

Due to completeness, we could start in definition 2 with the point function and then define the set function as specified. Equivalently, we could have defined the set functions by the conditions (1) and (4) and then reduce the set function to a point function. Henceforth I shall not distinguish between the point and the set function. Note, though, that without completeness the existence of an underlying point function is not guaranteed.

Why are complete ranking functions confined to integers? The reason is condition (4). It entails that any set of ranks has a minimum and hence that the range of a complete ranking function is well-ordered. Hence, the natural numbers are a natural choice. In my first publications (1983) and (1988) I allowed for more generality and assumed an arbitrary set of ordinal numbers as the range of a ranking function. However, since we want to calculate with ranks, this meant to engage into ordinal arithmetic, which is awkward. Therefore I later confined myself to complete ranking functions as defined above.

The issue about (4) was first raised by Lewis (1973, sect. 1.4) where he introduced the so-called Limit Assumption in relation to his semantics of counterfactuals. Endorsing (4), as I do, is tantamount to endorsing the Limit Assumption. Lewis finds reason against it, though it does not affect the *logic* of counterfactuals. From a semantic point of view, I do not understand his reason. He requests us to counterfactually suppose that a certain line is longer than an inch and asks how long it would or might be. He argues in effect that for each $\varepsilon > 0$ we should accept as true: "If the line would be longer than 1 inch, it would not be longer than $1 + \varepsilon$ inches." This strikes me as blatantly inconsistent, even if we cannot derive a contradiction in counterfactual logic. Therefore, I am accepting the Limit Assumption and, correspondingly, the law of infinite disjunction. This means in particular that in that law the minimum must not be weakened to the infimum.

Though I prefer complete ranking functions for the reasons given, the issue will have no further relevance here. In particular, if we assume the algebra of propositions to be finite, each ranking function is complete, and the issue does not arise. In the sequel, you can add or delete completeness as you wish.

Let me add another observation apparently of a technical nature. It is that we can mix ranking functions in order to form a new ranking function. This is the content of

Definition 3: Let Λ be a non-empty set of negative ranking functions for an algebra $\mathcal{A}$ of propositions, and let ρ be a complete negative ranking function over Λ. Then κ defined by

(5) $\kappa(A) = \min \{\lambda(A) + \mathrm{r}(\lambda) \mid \lambda \in \Lambda\}$ for all $A \in \mathcal{A}$

is obviously a negative ranking function for $\mathcal{A}$ as well and is called the *mixture of* Λ *by* ρ.

It is nice that such mixtures make formal sense. However, we shall see in the course of this paper that the point is more than a technical one; such mixtures will acquire deep philosophical importance later on.

So far, (degree of) disbelief was our basic notion. Was this necessary? Certainly not. We might just as well express things in positive terms:

Definition 4: Let $\mathcal{A}$ be an algebra over W. Then π is a *positive ranking function* for $\mathcal{A}$ iff π is a function from $\mathcal{A}$ into $\mathbf{R}^*$ such that for all $A, B \in \mathcal{A}$:

(6) $\pi(\emptyset) = 0$ and $\pi(W) = \infty$,

(7) $\pi(A \cap B) = \min\{\pi(A), \pi(B)\}$ [*the law of conjunction for positive ranks*].

Positive ranks express *degrees of belief*. $\pi(A) > 0$ says that A is believed (to some positive degree), and $\pi(A) = 0$ says that A is not believed. Obviously, positive ranks are the dual to negative ranks; if $\pi(A) = \kappa(\overline{A})$ for all $A \in \mathcal{A}$, then π is a positive function iff κ is a negative ranking function.

Positive ranking functions seem distinctly more natural. Why do I still prefer the negative version? A superficial reason is that we have seen complete negative ranking functions to be reducible to point functions, whereas it would obviously be ill-conceived to try the same for the positive version. This, however, is only indicative of the main reason. Despite appearances, we shall soon see that negative ranks behave very much like probabilities. In fact, this parallel will serve as our compass for a host of exciting observations. (For instance, in the finite case probability measures can also be reduced to point functions.) If we were thinking in positive terms, this parallel would remain concealed.

There is a further notion that may appear even more natural:

Definition 5: Let $\mathcal{A}$ be an algebra over W. Then τ is a *two-sided ranking function*[6] for A iff τ is a function from $\mathcal{A}$ into $\mathbf{R} \cup \{-\infty, \infty\}$ such that there is a negative ranking function κ and its positive counterpart π for which for all $A \in \mathcal{A}$:

$$\tau(A) = \kappa(\overline{A}) - \kappa(A) = \pi(A) - \kappa(A).$$

Obviously, we have $\tau(A) > 0$, < 0, or $= 0$ according to whether A is believed, disbelieved, or neither. In this way, the belief values of all propositions are expressed in a single function. Moreover, we have the appealing law that $\tau(\overline{A}) = -\tau(A)$. For some purposes this is a useful notion which I shall readily employ. However, its

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[6] In earlier papers I called this a belief function, obviously an unhappy term which has too many different uses. This is one reason fort the mild terminological reform proposed in this paper.

formal behavior is awkward. Its direct axiomatic characterization would have been cumbersome, and its simplest definition consisted in its reduction to the other notions.

Still, this notion suggests an interpretational degree of freedom so far unnoticed.[7] We might ask: Why does the range of belief extend over all the positive reals in a two-sided ranking function and the range of disbelief over all the negative reals, whereas neutrality shrinks to rank 0? This looks unfair. Why may unopinionatedness not occupy a much broader range? Indeed, why not? We might just as well distinguish some positive rank or real z and define the closed interval $[-z, z]$ as the range of neutrality. Then $\tau(A) > z$ expresses belief in A and $\tau(A) < -z$ disbelief in A. This is a viable interpretation; in particular, consistency and deductive closure of belief sets would be preserved.

The interpretational freedom appears quite natural. After all, the notion of belief is certainly vague and can be taken more or less strict. We can do justice to this vagueness with the help of the parameter z. The crucial point, though, is that we always get the formal structure of belief we want to get, however we fix that parameter. The principal lesson of this observation is, hence, that it is not the notion of belief which is of basic importance; it is rather the formal structure of ranks. The study of belief *is* the study of *that* structure. Still, it would be fatal to simply give up talking of belief in favor of ranks. Ranks express beliefs, even if there is interpretational freedom. Hence, it is crucial to maintain the intuitive connection, and therefore I shall stick to my standard interpretation and equate belief in A with $\tau(A) > 0$, even though this is a matter of decision.

Let us pause for a moment and take a brief look back. What I have told so far probably sounds familiar. One has quite often seen all this, in this or a similar form – where the similar form may also be a relational one: as long as only the ordering and not the numerical properties of the degrees of belief are relevant, a ranking function may also be interpreted as a weak ordering of propositions according to their plausibility, entrenchment, credibility etc. Often things are cast in negative terms, as I primarily do, and often in positive terms. In particular, the law of negation securing consistency and the law of disjunction somehow generalizing deductive closure (we still have to look at the point more thoroughly) or their positive counterparts are pervasive. If one wants to distinguish a common core in that ill-defined family of Baconian probability, it is perhaps just these two laws.

[7] I am grateful to Matthias Hild for making this point clear to me.

So, why invent a new name, 'ranks', for familiar stuff? The reason lies in the second fundamental aim associated with ranking functions: to account for the dynamics of belief. This aim has been little pursued under the label of Baconian probability, but it is our central topic for the rest of this part. Indeed, everything stands and falls with our notion of conditional ranks; it is the distinctive mark of ranking theory. Here it is:

Definition 6: Let κ be a negative ranking function for $\mathcal{A}$ and $\kappa(A) < \infty$. Then the *conditional rank* of $B \in \mathcal{A}$ *given* A is defined as $\kappa(B \mid A) = \kappa(A \cap B) - \kappa(A)$. The function κ_A: $B \mapsto \kappa(B \mid A)$ is obviously a negative ranking function in turn and called the *conditionalization of* κ *by A*.

We might rewrite this definition as a law:

(8) $\kappa(A \cap B) = \kappa(A) + \kappa(B \mid A)$ [the *law of conjunction* (*for negative ranks*)].

This amounts to the highly intuitive assertion that one has to add the degree of disbelief in B given A to the degree of disbelief in A in order to get the degree of disbelief in A-and-B.

Moreover, it immediately follows for all $A, B \in \mathcal{A}$ with $\kappa(A) < \infty$:

(9) $\kappa(B \mid A) = 0$ or $\kappa(\bar{B} \mid A) = 0$ [*conditional law of negation*].

This law says that even conditional belief must be consistent. If both, $\kappa(B \mid A)$ and $\kappa(\bar{B} \mid A)$, were > 0, both, B and $\bar{B}$, would be believed given A, and this ought to be excluded, as long as the condition A itself is considered possible.

Indeed, my favorite axiomatization of ranking theory runs reversely, it consists of the definition of conditional ranks and the conditional law of negation. The latter says that min $\{\kappa(A \mid A \cup B), \kappa(B \mid A \cup B)\} = 0$, and this is just the law of disjunction in view of the former. Hence, the only substantial assumption written into ranking functions is conditional consistency, and it is interesting to see that this entails deductive closure as well.

It is instructive to look at the positive counterpart of negative conditional ranks. If π is the positive ranking function corresponding to the negative ranking function κ, definition 6 simply translates into: $\pi(B \mid A) = \pi(\bar{A} \cup B) - \pi(\bar{A})$. Defining $A \rightarrow B = \bar{A} \cup B$ as set-theoretical 'material implication', we may as well write:

(10) $\pi(A \rightarrow B) = \pi(B \mid A) + \pi(\bar{A})$ [*the law of material implication*].

Again, this is highly intuitive. It says that the degree of belief in the material implication $A \rightarrow B$ is added up from the degree of belief in its vacuous truth (i.e., in $\bar{A}$) and the conditional degree of belief of B given A.[8] However, again comparing the negative and the positive version, one can already sense the analogy between probability and ranking theory from (8),but hardly from (10). This analogy will play a great role in the following sections.

Two-sided ranks have a conditional version as well; it is straightforward. If τ is the two-sided ranking function corresponding to the negative κ and the positive π, then we may simply define:

(11) $\tau(B \mid A) = \pi(B \mid A) - \kappa(B \mid A) = \kappa(\bar{B} \mid A) - \kappa(B \mid A)$.

It will sometimes be useful to refer to these two-sided conditional ranks.

For illustration of negative conditional ranks, let us briefly return to our example Tweetie. Above, I already mentioned various examples of if-then sentences, some held vacuously true and some non-vacuously. Now we can see that precisely the if-then sentences non-vacuously held true correspond to conditional beliefs. According to the κ specified, you believe, e.g., that Tweetie can fly given it is a bird (since $\kappa(\bar{F} \mid B) = 1$) and also given it is a bird, but not a penguin (since $\kappa(\bar{F} \mid B \,\&\, \bar{P}) = 2$), that Tweetie cannot fly given it is a penguin (since $\kappa(F \mid P) = 4$) and even given it is a penguin, but not a bird (since $\kappa(F \mid \bar{B} \,\&\, P) = 4$). You also believe that it is not a penguin given it is a bird (since $\kappa(P \mid B) = 1$) and that it is a bird given it is a penguin (since $\kappa(\bar{B} \mid P) = 20$). And so forth.

Let us now unfold the power of conditional ranks and their relevance to the dynamics of belief in several steps.

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[8] Thanks again to Matthias Hild for pointing this out to me.

1.3 Reasons and Their Balance

The first application of conditional ranks is in the theory of confirmation. Basically, Carnap (1950) told us, confirmation is positive relevance. This idea can be explored probabilistically, as Carnap did. But here the idea works just as well. A proposition *A* confirms or supports or speaks for a proposition *B*, or, as I prefer to say, *A* is a reason for *B*, if *A* strengthens the belief in *B*, i.e., if *B* is more strongly believed given *A* than given $\bar{A}$, i.e., iff *A* is positively relevant for *B*. This is easily translated into ranking terms:

Definition 7: Let κ be a negative ranking function for $\mathcal{A}$ and τ the associated two-sided ranking function. Then $A \in \mathcal{A}$ is a *reason for* $B \in \mathcal{A}$ w.r.t. κ iff $\tau(B \mid A) > \tau(B \mid \bar{A})$, i.e., iff $\kappa(\bar{B} \mid A) > \kappa(\bar{B} \mid \bar{A})$ or $\kappa(B \mid A) < \kappa(B \mid \bar{A})$.

If P is a standard probability measure on $\mathcal{A}$, then probabilistic positive relevance can be expressed by $P(B \mid A) > P(B)$ or by $P(B \mid A) > P(B \mid \bar{A})$. As long as all three terms involved are defined, the two inequalities are equivalent. Usually, then, the first inequality is preferred because its terms may be defined while not all of the second inequality are defined. If P is a Popper measure, this argument does not hold, and then it is easily seen that the second inequality is more adequate, just as in the case of ranking functions.[9]

Confirmation or support may take four different forms relative to ranking functions, which are unfolded in

Definition 8: Let κ be a negative ranking function for $\mathcal{A}$, τ the associated two-sided ranking function, and $A, B \in \mathcal{A}$. Then

$$A \text{ is a } \begin{Bmatrix} \textit{additional} \\ \textit{sufficient} \\ \textit{necessary} \\ \textit{weak} \end{Bmatrix} \textit{ reason for } B \text{ w.r.t. } \kappa \text{ iff } \begin{Bmatrix} \tau(B \mid A) > \tau(B \mid \bar{A}) > 0 \\ \tau(B \mid A) > 0 \geq \tau(B \mid \bar{A}) \\ \tau(B \mid A) \geq 0 > \tau(B \mid \bar{A}) \\ 0 > \tau(B \mid A) > \tau(B \mid \bar{A}) \end{Bmatrix}.$$

[9] A case in point is the so-called problem of old evidence, which has a simple solution in terms of Popper measures and the second inequality; cf. Joyce (1999, pp. 203ff.).

If *A* is a reason for *B*, it must obviously take one of these four forms; and the only way to have two forms at once is by being a necessary and sufficient reason.

Talking of reasons here is, I find, natural, but it stirs a nest of vipers. There is a host of philosophical literature pondering about reasons, justifications, etc. Of course, this is a field where multifarious philosophical conceptions clash, and it is not easy to gain an overview over the fighting parties. Here is not the place for starting a philosophical argument[10], but by using the term 'reason' I want at least to submit the claim that the topic may gain enormously by giving a central place to the above explication of reasons.

To elaborate only a little bit: When philosophers feel forced to make precise their notion of a (theoretical, not practical) reason, they usually refer to the notion of a *deductive* reason, as fully investigated in deductive logic. The deductive reason relation is reflexive, transitive, and not symmetric. By contrast, definition 7 captures the notion of a *deductive or inductive* reason. The relation embraces the deductive relation, but it is reflexive, symmetric, and not transitive. Moreover, the fact that reasons may be additional or weak reasons according to definition 8 has been neglected by the relevant discussion, which was rather occupied with necessary and/or sufficient reasons. Pursue, though, the use of the latter terms throughout the history of philosophy. Their deductive explication is standard and almost always fits. Often, it is clear that the novel inductive explication given by definition 8 would be inappropriate. Very often, however, the texts are open to that inductive explication as well, and systematically trying to reinterpret these old texts would yield a highly interesting research program in my view.

The topic is obviously inexhaustible. Let me take up only one further aspect. Intuitively, we weigh reasons. This is a most important activity of our mind. We do not only weigh practical reasons in order to find out what to do, we also weigh theoretical reasons. We are wondering whether or not we should believe *B*, we are searching for reasons speaking in favor or against *B*, we are weighing these reasons, and we hopefully reach a conclusion. I am certainly not denying the phenomenon of inference which is also important, but what is represented as an inference often rather takes the form of such a weighing procedure. 'Reflective equilibrium' is a familiar and somewhat more pompous metaphor for the same thing.

If the balance of reasons is such a central phenomenon the question arises: how can epistemological theories account for it? The question is less well addressed

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[10] I attempted to give a partial overview and argument in Spohn (2001a).

than one should think. However, the fact that there is a perfectly natural Bayesian answer is a very strong and more or less explicit argument in favor of Bayesianism. Let us take a brief look at how that answer goes:

Let P be a (subjective) probability measure over $\mathcal{A}$ and let B be the focal proposition. Let us look at the simplest case, consisting of one reason A for B and the automatic counter-reason $\bar{A}$ against B. Thus, in analogy to definition 7, $P(B \mid A) > P(B \mid \bar{A})$. How does P balance these reasons and thus fit in B? The answer is simple, we have:

(12) $P(B) = P(B \mid A) \cdot P(A) + P(B \mid \bar{A}) \cdot P(\bar{A})$.

This means that the probabilistic balance of reason is a *beam balance* in the literal sense. The length of the lever is $P(B \mid A) - P(B \mid \bar{A})$; the two ends of the lever are loaded with the *weights* $P(A)$ and $P(\bar{A})$ of the reasons; $P(B)$ divides the lever into two parts of length $P(B \mid A) - P(B)$ and $P(B) - P(B \mid \bar{A})$ representing the *strength* of the reasons; and then $P(B)$ must be chosen so that the beam is in balance. Thus interpreted (12) is nothing but the law of levers.

Ranking theory has an answer, too, and I am wondering who else has. According to ranking theory, the balance of reasons works like a *spring balance*. Let κ be a negative ranking function for $\mathcal{A}$, τ the corresponding two-sided ranking function, B the focal proposition, and A a reason for B. So, $\tau(B \mid A) > \tau(B \mid \bar{A})$. Again, it easily proved that always $\tau(B \mid A) \geq \tau(B) \geq \tau(B \mid \bar{A})$. But where in between is $\tau(B)$ located? A little calculation shows the following specification to be correct:

(13) Let $x = \kappa(B \mid \bar{A}) - \kappa(B \mid A)$ and $y = \kappa(\bar{B} \mid A) - \kappa(\bar{B} \mid \bar{A})$. Then
- (a) $x, y \geq 0$ and $\tau(B \mid A) - \tau(B \mid \bar{A}) = x + y$,
- (b) $\tau(B) = \tau(B \mid \bar{A})$, if $\tau(A) \leq -x$,
- (c) $\tau(B) = \tau(B \mid A)$, if $\tau(A) \geq y$,
- (d) $\tau(B) = \tau(A) + \tau(B \mid \bar{A}) + x$, if $-x < \tau(A) < y$.

This does not look as straightforward as the probabilistic beam balance. Still, it is not so complicated to interpret (13) as a spring balance. The idea is that you hook in the spring at a certain point, that you extend it by the force of reasons, and that $\tau(B)$ is where the spring extends. Consider first the case where $x, y > 0$. Then you hook in the spring at point 0 and exert the force $\tau(A)$ on the spring. Either, this force transcends the lower stopping point $-x$ or the upper stopping point y. Then the spring extends exactly till the stopping point, as (13b+c) say. Or, the force $\tau(A)$ is less. Then the spring extends exactly by $\tau(A)$, according to (13d). The second case is that $x = 0$ and $y > 0$. Then you fix the spring at $\tau(B \mid \bar{A})$, the lower point of the interval in which $\tau(B)$ can move. The spring cannot extend below that point, says (13b). But according to (13c+d) it can extend above, by the force $\tau(A)$, but not beyond the upper stopping point. For the third case $x > 0$ and $y = 0$ just reverse the second picture. In this way, the force of the reason, represented by its two-sided rank, pulls the two-sided rank of the focal proposition B to its proper place within the interval fixed by the relevant conditional ranks.

I do not want to assess these findings in detail. You might prefer the probabilistic balance of reasons, a preference I would understand. You might be happy to have at least one alternative model, an attitude I recommend. Or you may search for further models of the weighing of reasons; in this case, I wish you good luck. What you may not do is ignoring the issue; your epistemology is incomplete if it does not take a stand. And one must be clear about what is required for taking a stand. As long as one considers positive relevance to be the basic characteristic of reasons, one must provide some notion of conditional degrees of belief, conditional probabilities, conditional ranks, or whatever. Without some well behaved conditionalization one cannot succeed.

1.4 The Dynamics of Belief and the Measurement of Belief

Our next point will be to define a reasonable dynamics for ranking functions that entails a dynamic for belief. There are many causes which affect our beliefs, forgetfulness as a necessary evil, drugs as an unnecessary evil, and so on. From a rational point of view, it is scarcely possible to say anything about such changes.[11] The rational changes are due to experience or information. Thus, it seems we have already solved our task: if κ is my present doxastic state and I get informed about the proposition A, then I move to the conditionalization κ_A of κ by A. This, however, would be a bad idea. Recall that we have $\kappa_A(\bar{A}) = \infty$, i.e., A is believed with

[11] Although there is a (by far not trivial) decision rule telling that costless memory is never bad, just as costless information; cf. Spohn (1978, sect. 4.4).

absolute certainty in κ_A; no future evidence could cast any doubt on the information. This may sometimes happen; but usually information does not come so firmly. Information may turn out wrong, evidence may be misleading, perception may be misinterpreted; we should provide for flexibility. How?

One point of our first attempt was correct; if my information consists solely in the proposition A, this cannot affect my beliefs conditional on A. Likewise, it cannot affect my beliefs conditional on $\overline{A}$. Thus, it directly affects only how firmly I believe A itself. So, how firmly should I believe A? There is no general answer. I propose to turn this into a parameter of the information process itself; somehow the way I get informed about A entrenches A in my belief state with a certain firmness x. The point is that as soon as the parameter is fixed and the constancy of the relevant conditional beliefs accepted, my posterior belief state is fully determined. This is the content of

Definition 9: Let κ be a negative ranking function for $\mathcal{A}$, $A \in \mathcal{A}$ such that $\kappa(A)$, $\kappa(\overline{A}) < \infty$, and $x \in \mathbf{R}^*$. Then the $A{\rightarrow}x$-conditionalization $\kappa_{A\rightarrow x}$ of κ is defined by $\kappa_{A\rightarrow x}(B) = \begin{cases} \kappa(B \mid A) \text{ for } B \subseteq A, \\ \kappa(B \mid \overline{A}) + x \text{ for } B \subseteq \overline{A} \end{cases}$. From this $\kappa_{A\rightarrow x}(B)$ may be inferred for all other $B \in \mathcal{A}$ by the law of disjunction.

Hence, the effect of the $A{\rightarrow}x$-conditionalization is to shift the possibilities in A (upwards) so that $\kappa_{A\rightarrow x}(A) = 0$ and the possibilities in $\overline{A}$ (downwards) so that $\kappa_{A\rightarrow x}(\overline{A}) = x$. If one is attached to the idea that evidence consists in nothing but a proposition, the additional parameter is a mystery. The processing of evidence may indeed be so automatic that one hardly becomes aware of this parameter. Still, I find it entirely natural that evidence comes more or less firmly. Suppose, e.g., my wife is traveling in a foreign country and the train that she intended to take has a terrible accident. Consider five scenarios: (i) a newspaper reports that the only German woman on the train is not hurt, (ii) the ambassador calls me and tells that my wife is not hurt, (iii) I see her on TV shocked, but apparently unharmed, (iv) I see her on TV giving an interview and telling how terrible the accident was and what a great miracle it is that she has survived unhurt, (v) I take her into my arms (after immediately going to that foreign place). In all five cases I receive the information that my wife is not hurt, but with varying and plausibly increasing certainty.

One might object that the evidence and thus the proposition received is clearly a different one in each of the scenarios. The crucial point, though, is that we are dealing here with a fixed algebra $\mathcal{A}$ of propositions and that we have nowhere presupposed that this algebra consists of all propositions whatsoever; indeed, that would be a doubtful presupposition. Hence $\mathcal{A}$ may be course-grained and unable to represent the propositional differences between the scenarios; the proposition in $\mathcal{A}$ which is directly affected in the various scenarios may be just the proposition that my wife is not hurt. Still the scenarios may be distinguished by the firmness parameter.

So, the dynamics of ranking function I propose is simply this: Suppose κ is your prior doxastic state. Now you receive some information A with firmness x. Then your posterior state is $\kappa_{A \to x}$. Your beliefs change accordingly; they are what they are according to $\kappa_{A \to x}$. Note that the procedure is iterable. Next, you receive the information B with firmness y, and so you move to $(\kappa_{A \to x})_{B \to y}$. And so on. This point will acquire great importance later on.

I should mention, though, that this iterability need not work in full generality. Let us call a negative ranking function κ *regular* iff $\kappa(A) < \infty$ for all $A \neq \emptyset$. Then we obviously have that $\kappa_{A \to x}$ is regular if κ is regular and $x < \infty$. Within the realm of regular ranking functions iteration of changes works unboundedly. Outside this realm you may get problems with the rank ∞.

There is an important generalization of definition 9. I just made a point of the fact that the algebra $\mathcal{A}$ may be too coarse-grained to propositionally represent all possible evidence. Why assume then that it is just one proposition A in the algebra that is directly affected by the evidence? Well, we need not assume this. We may more generally assume that the evidence affects some evidential partition $\mathcal{E} = \{E_1,\dots,E_n\} \subseteq \mathcal{A}$ of W and assigns some new ranks to the members of the partition, which we may sum up in a complete ranking function λ on $\mathcal{E}$. Then we may define the $\mathcal{E}\to\lambda$-*conditionalization* $\kappa_{\mathcal{E}\to\lambda}$ of the prior κ by $\kappa_{\mathcal{E}\to\lambda}(B) = \kappa(B \mid E_i) + \lambda(E_i)$ for $B \subseteq E_i$ $(i = 1,\dots,n)$ and infer $\kappa_{\mathcal{E}\to\lambda}(B)$ for all other B by the law of disjunction. This is the most general law of doxastic change in terms of ranking functions I can conceive of. Note that we may describe the $\mathcal{E}\to\lambda$-conditionalization of κ as the mixture of all κ_{E_i} $(i = 1,\dots,n)$. So, this is a first useful application of mixtures of ranking functions.

Here, at last, the reader will have noticed the great similarity of my conditionalization rules with Jeffrey's probabilistic conditionalization first presented in

Jeffrey (1965, ch. 11). Indeed, I have completely borrowed my rules from Jeffrey. Still, let us further defer the comparison of ranking with probability theory. The fact that many things run similarly does not mean that one can dispense with the one in favor of the other, as I shall make clear in part 3.

There is an important variant of definition 9. Shenoy (1991), and several authors after him, pointed out that the parameter x as conceived in definition 9 does not characterize the evidence as such, but rather the result of the interaction between the prior doxastic state and the evidence. Shenoy proposed a reformulation with a parameter exclusively pertaining to the evidence:

Definition 10: Let κ be a negative ranking function for $\mathcal{A}$, $A \in \mathcal{A}$ such that $\kappa(A)$, $\kappa(\bar{A}) < \infty$, and $x \in \mathbf{R}^*$. Then the *$A\uparrow x$-conditionalization* $\kappa_{A\uparrow x}$ of κ is defined by $\kappa_{A\uparrow x}(B) = \begin{cases} \kappa(B \mid A) - y \text{ for } B \subseteq A, \\ \kappa(B \mid \bar{A}) + x - y \text{ for } B \subseteq \bar{A}, \end{cases}$ where $y = \min\{\kappa(A), x\}$. Again, $\kappa_{A\uparrow x}(B)$ may be inferred for all other $B \in \mathcal{A}$ by the law of disjunction.

The effect of this conditionalization is easily stated. It is, whatever the prior ranks of A and $\bar{A}$ are, that the possibilities within A improve by exactly x ranks in comparison to the possibilities within $\bar{A}$. In other words, we always have $\tau_{A\uparrow x}(A) - \tau(A) = x$ (in terms of the prior and the posterior two-sided ranking function).

It is thus fair to say that in $A\uparrow x$-conditionalization the parameter x exclusively characterizes the evidential impact. We may characterize the $A\to x$-conditionalization of definition 9 as *result-oriented* and the $A\uparrow x$-conditionalization of definition 10 as *evidence-oriented*. Of course, the two variants are easily interdefinable. We always have $\kappa_{A\to x} = \kappa_{A\uparrow y}$, where $y = x - \tau(A)$. Still, it is sometimes useful to change perspective from one variant to the other.[12]

For instance, the evidence-oriented version helps to some nice observations. We may note that conditionalization is reversible: $(\kappa_{A\uparrow x})_{\bar{A}\uparrow x} = \kappa$. So, there is always a possible second change undoing the first. Moreover, changes always commute: $(\kappa_{A\uparrow x})_{B\uparrow y} = (\kappa_{B\uparrow y})_{A\uparrow x}$. In terms of result-oriented conditionalization this law would look more awkward. Commutativity does not mean, however, that one could comprise the two changes into a single change. Rather, the joint effect of

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[12] Generalized probabilistic conditionalization as originally proposed by Jeffrey was result-oriented as well. However, Garber (1980) observed that there is also an evidence-oriented version of generalized probabilistic conditionalization.

two conditionalizations according to definition 9 or 10 can in general only be summarized as one step of generalized $\mathcal{E}\rightarrow\lambda$-conditionalization. I think that reversibility and commutativity are intuitively desirable.

Change through conditionalization is driven by information, evidence, or perception. This is how I have explained it. However, we may also draw a more philosophical picture, we may also say that belief change according to definition 9 or 10 is driven by reasons. Propositions for which the information received is irrelevant do not change their ranks, but propositions for which that information is positively or negatively relevant do change their ranks. The evidential force pulls at the springs and they must find a new rest position for all the propositions for or against which the evidence speaks, just in the way I have described in the previous section.

This is a strong picture captivating many philosophers. However, I have implemented it in a slightly unusual way. The usual way would have been to attempt to give some substantial account of what reasons are on which an account of belief dynamics is thereafter based. I have reversed the order. I have first defined conditionalization in definition 6 and the more sophisticated form in definitions 9 and 10. With the help of conditionalization, i.e., from this account of belief dynamics, I could define the reason relation such that this picture emerges. At the same time this means to dispense with a more objective notion of a reason. Rather, what is a reason for what is entirely determined by the subjective doxastic state as represented by the ranking function at hand. Ultimately, this move is urged by inductive skepticism as enforced by David Hume and reinforced by Nelson Goodman. But it is not a surrender to skepticism. On the contrary, we are about to unfold a positive theory of rational belief and rational belief change, and we shall see how far it carries us.

If one looks at the huge literature on belief change, one finds discussed predominantly three kinds of changes: expansions, revisions, and contractions. Opinions widely diverge concerning these three kinds. For Levi, for instance, revisions are whatever results form concatenating contractions and expansions according to the so-called Levi identity and so investigates the latter (see his most recent account in Levi 2005). The AGM approach characterizes both, revisions and contractions, and claims nice correspondences back and forth by help of the Levi and the Harper identity (cf., e.g., Gärdenfors 1988, chs. 3 and 4). Or one might object to the characterization of contraction, but accept that of revision, and hence reject these identities. And so forth.

I do not really want to discuss the issue. I only want to point out that we have already taken a stance insofar as expansions, revisions, and contractions are all special cases of our $A \rightarrow x$–conditionalization. This is easily explained in terms of result-oriented conditionalization:

If $\kappa(A) = 0$, i.e., if A is not disbelieved, then $\kappa_{A \rightarrow x}$ represents an *expansion* by A for any $x > 0$. If $\kappa(\bar{A}) = 0$, the expansion is genuine, if $\kappa(\bar{A}) > 0$, i.e., if A is already believed in κ, the expansion is vacuous. Are there many different expansion? Yes and no. Of course, for each $x > 0$ another $\kappa_{A \rightarrow x}$ results. On the other hand, one and the same belief set is associated with all these expansions. Hence, the expanded belief set is uniquely determined.

Similarly for revision. If $\kappa(A) > 0$, i.e., if A is disbelieved, then $\kappa_{A \rightarrow x}$ represents a genuine *revision* by A for any $x > 0$. In this case, the belief in $\bar{A}$ must be given up and along with it many other beliefs; instead, A must be adopted together with many other beliefs. Again, there are many different revisions, but all of them result in the same revised belief set.

Finally, if $\kappa(A) = 0$, i.e., if A is not disbelieved, then $\kappa_{A \rightarrow 0}$ represents contraction by A. If $\kappa(\bar{A}) > 0$, i.e., if A is even believed, the contraction is genuine; then belief in A is given up after contraction and no new belief adopted. If $\kappa(\bar{A}) = 0$, the contraction is vacuous; there was nothing to contract in the first place. If $\kappa(A) > 0$, i.e., if $\bar{A}$ is believed, then $\kappa_{A \rightarrow 0} = \kappa_{\bar{A} \rightarrow 0}$ rather represents contraction by $\bar{A}$.

As I observed in Spohn (1988, footnote 20), it is easily checked that expansions, revisions, and contractions thus defined satisfy all of the original AGM postulates (K*1-8) and (K^{-}1-8) (cf. Gärdenfors 1988, pp. 54-56 and 61-64) (when they are translated from AGM's sentential framework into our propositional or set-theoretical one). For those like me who accept the AGM postulates this is a welcome result. For the others, it means finding fault with $A \rightarrow x$-conditionalization or with ranking theory or reconsidering their criticism of these postulates.

For the moment, though, it may seem that we have simply reformulated AGM belief revision theory. This is not so; $A \rightarrow x$-conditionalization is much more general than the three AGM changes. This is clear from the fact that there are many different expansions and revisions which the AGM account cannot distinguish. It is perhaps clearest in the case of vacuous expansion which is no change at all in the AGM framework, but may well be a genuine change in the ranking framework, a redistribution of ranks which does not affect the surface of beliefs. Another way to state the same point is that weak and additional reasons also drive doxastic changes, which, however, are inexpressible in the AGM framework.

This is not yet the core of the matter, though. The core of the matter is *iterated belief change*, which I have put into the center of my considerations in Spohn (1988). As I have argued there, AGM belief revision theory is essentially unable to account for iterated belief change. I take almost 20 years of unsatisfactory attempts to deal with that problem as confirming my early assessment. By contrast, changes of the type $A \rightarrow x$-conditionalization are obviously infinitely iterable.

In fact, my argument in Spohn (1988) was stronger. It was that if AGM belief revision theory is to be improved so as to adequately deal with the problem of iterated belief change, ranking theory is the only way to do it. I always considered this to be a conclusive argument in favor of ranking theory.

This may be so. Still, the AGM theorists, and others as well, remained skeptical. “What exactly is the meaning of numerical ranks?” they asked. One may well acknowledge that the ranking apparatus works in a smooth and elegant way, has a lot of explanatory power, etc. But all this does not answer this question. Bayesians have met this challenge. They have told stories about the operational meaning of subjective probabilities in terms of betting behavior, they have proposed an ingenious variety of procedures for measuring this kind of degrees of belief. One would like to see a comparative achievement for ranking theory.

It exists. Matthias Hild first presented it in a number of talks around 1997. I independently discovered it later on and presented it in Spohn (1999), a publication on the web. So far, this is the only public presentation, admittedly an awkward one. There is no space here to fully develop the argument. However, the basic point can easily be indicated so as to make the full argument at least plausible.

The point is that ranks do not only account for iterated belief change, but can reversely be measured thereby. This may at first sound unhelpful. $A \rightarrow x$-conditionalization refers to the number x; so even if ranks can somehow be measured with the help of such conditionalizations, we do not seem to provide a fundamental measurement of ranks. Recall, however, that contraction by A (or $\overline{A}$) is just $A \rightarrow 0$-conditionalization and is thus free of a hidden reference to numerical ranks; it only refers to rank 0 which has a clear operational or surface interpretation in terms of belief. Hence, the idea is to measure ranks by means of iterated contractions; if that works, it is really a fundamental measurement of ranks which is based only on the beliefs one now has and one would have after various iterated contractions.[13]

How does the idea work? Recall our observation above that the positive rank of a material implication $A \rightarrow B$ is the sum of the degree of belief in B given A and the degree of belief in the vacuous truth of the $A \rightarrow B$, i.e., of $\overline{A}$. Hence, after contraction by $\overline{A}$, belief in the material implication $A \rightarrow B$ is equivalent to belief in B given A, i.e., to the positive relevance of A to B. This is how the reason relation, i.e., positive relevance, manifests itself in beliefs surviving contractions. Next observe that positive relevance can be expressed by certain inequalities for ranks, by certain differences between ranks being positive. This calls for applying the theory of difference measurement, as paradigmatically presented by Krantz et al. (1971, ch. 4).

This application is indeed successful. The resulting theorem says the following: Iterated contractions behave thus and thus if and only if differences between ranks behave thus and thus; and if differences between ranks behave thus and thus, then there is a ranking function measured on a ratio scale, i.e., unique up to a multiplicative constant, which exactly represents these differences.

On the one hand, this provides for an axiomatization of iterated contraction (going beyond Darwiche, Pearl (1997), in my view so far the best characterization of iterated revision and contraction on the level of beliefs); this axiomatization is assessible on intuitive or other grounds. On the other hand, one knows that if one accepts this axiomatization of iterated contraction one is bound to accept ranks as I have proposed them. Ranks do not fall from the sky, then; on the contrary, they uniquely represent contraction behavior.

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[13] In section 2.1 I have suggested that one need not confine unopinionatedness to the two-sided rank 0, one may as well represent it by two-sided ranks within an interval $[-z, z]$. Note, however, that this suggestion deprives contraction of its uniqueness. Rather, any kind of $A \rightarrow x$-conditionalization would amount to a contraction as long as $x \leq z$. Hence, the measurement idea we are about to pursue would then become problematic. It works properly only if unopinionatedness is confined to the two-sided rank 0, as originally presented.

12. Laws and Their Confirmation

12.1 A Plan for the Next Chapters

[Now the numbering refers to the book of which this is the chapter 12.]

After the introductory chapters 1 – 4, the detailed exposition of ranking theory in the chapters 5 – 9, and the comparative interlude in the chapters 10 and 11, I now turn to the second half of this book. This exposition has shown that ranking theory, *the* theory of belief, is a rich theory well worth to be developed on its own, and we have met many issues and perspectives in need of further inquiry. However, in the first place I consider ranking theory as a tool for pursuing the goals of philosophical epistemology that are much broader than merely stating the static and dynamics of rational doxastic states. In being such a tool ranking theory unfolds its real power and beauty; there it finds its deeper justification. This is why I shall develop the philosophical applications so extensively.

In the first part of the philosophical applications I shall be dealing with topics of philosophy of science, laws, dispositions, causation, explanation, etc. These chapters are not independent; they rather present a continuous story that will reach a certain completion only in chapter 15 on objectivization. All the more, some general introductory words are in order before going *in medias res*.

The overarching goal of the subsequent chapters is perhaps best described as trying to understand empirical or natural necessity or modality. There are confusingly many modalities. There is logical necessity and relative logical necessity, i.e., relative to some theory or some axioms; they are well understood. There is metaphysical necessity, analyticity, and apriority; only the latter, being of an epistemological kind, will occupy us in the final chapters of this book. There are deontic modalities, clearly not our topic. Some count all the propositional attitudes among the modalities because of their intensionality. And so on. Finally, there is the class I am concerned with: nomic necessity, causal necessity, probability, counterfactuality, and determination, dispositions, powers, forces, and capacities – none of which can be understood as relative logical necessity. These are not all the same, but they are obviously closely related and thus subsumable under the common label “natural modality”.

Hume's rejection of necessary connexions has firmly anchored natural modalities in our philosophical agenda, forever it seems. Hardly any topic was so hotly debated in the recent decades. The views on it have become ever more embracive and ever more detailed and refined. But nothing is settled. Here, I want to contribute a further view – or rather, it seems to me, though I shall not engage in exegetical discussions, Hume's old view elaborated in detail and at the height of the present debate.

The core idea is that natural modalities are covertly epistemological notions, i.e., that they can only or best be understood through their relation to overtly epistemological notions, the philosophical task being to uncover that relation. For instance, subjective probability is an overtly epistemological notion; and if Lewis (1980, p. 266) is right in claiming that his so-called Principal Principle relating credence and chance is all we know, and need to know, about objective probability, the latter turns out to be a covertly epistemological notion. The father of this idea, is, of course, Hume himself, whose skeptical reasoning culminated in the conclusion that causation is an idea of reflexion (in his sense); cf. Hume (1739, p. 224).

How this idea is to be carried out in detail remains to be seen. However, it is clear that if the elaboration is successful, it is a realization of the program labeled Humean projection by Blackburn (1993) and Grice (1991) and succinctly described by Blackburn (1980) in the opening paragraph of a paper partially about Ramsey:

> Ramsey was one of the few philosophers who have fully appreciated the fundamental picture of metaphysics that was originally sketched by Hume. In this picture the world – that which makes proper judgement true or false – impinges on the human mind. This, in turn, has various reactions: we form habits of judgement and attitudes, and modify our theories, and perhaps do other things. But then – and this is the crucial mechanism – the mind can express such a reaction by 'spreading itself on the world'. That is, we regard the world as richer or fuller through possessing properties and things that are in fact mere projections of the mind's own reactions: there is no reason for the world to contain a fact corresponding to any given projection. So the world, on such a metaphysic, might be much thinner than common sense supposes it. Evidently the picture invites us to frame a debate: how are we going to tell where Hume's mechanism operates? Perhaps everywhere: drawing us to idealism, leaving the world entirely noumenal; or perhaps just somewhere; or nowhere. Hume's most famous applications of his mechanism, to values and causes, are extended by Ramsey to general propositions, which to him represented not judgements but projections of our habits of singular belief, and also to judgements of probability, which are projections of our degrees of confidence in singular beliefs.

I shall be less concerned with the general philosophy of this program; that would mean engaging in ontological dispute much more thoroughly than is within the scope of this book. I am rather concerned with the specific constructive details for which ranking theory will indeed prove to be an ideal tool; and I am convinced that this is, even if tedious, much more helpful to the program than more of general philosophizing. In Spohn (forthcoming) I have suggested a projectivistic treatment of objective probability that I shall not repeat here. Clearly, the present chapters will provide the full background for that paper that I could not extend there. Also, I shall not attend to counterfactuality; though convinced that the same strategy applies to it in principle, too, I have repeatedly stated my despair at the linguistic complexities of the counterfactual and conditional idiom. However, I shall specifically deal with all the other natural modalities.

Despite my abstention from ontological dispute it should be clear that the program of Humean projection attempts to be a full-blown alternative to Lewis' program of Humean supervenience that has been a continuous challenge to me. One may think that the two programs are at cross-purposes because the one is purely ontological and the other epistemological. The quote from Blackburn makes clear, however, that this would be a mistake; the two programs compete. In Spohn (forthcoming) I have argued that objective chance indeed remains the 'big bad bug' Lewis (1986b, p. xiv) had feared and Lewis (1994) thought he had cured, i.e., that the cure does not work and that he retains more epistemological ingredients than Humean supervenience officially allows. However, Lewis' doctrine is so impressive because it is worked out in so detailed a way. Also for this reason my emphasis is on the constructive details; only thereby the alternatives are at eye-level in the general philosophical arena. Hence, I shall have little direct argument with Lewis' doctrine, except in chapter 14 on causation. It may suffice to have pointed to the case in these strategic introductory remarks.

12.2 The Problem of Lawlikeness

Let us first consider nomic necessity; it will turn out to be the most simple and basic modality, though it will take a chapter full of formal details to see the simplicity. A law without adjective is throughout to be understood as a strict or deterministic law as opposed to a statistical one. So, what is a law? A true lawlike sentence or proposition. Philosophers are accustomed to separate the two dimensions. Kepler's laws have a lawlike status even though they have turned out to be not exactly true, i.e., literally false. And although there may be general worries as to whether truth pertains to lawlike propositions, the more relevant problem for philosophers of science has been to explain lawlikeness itself. This has turned out to be a surprisingly recalcitrant problem.

However, it is easy to form intuitions. Physicists tend to refer to their proudest examples, conservation laws and symmetry principles, as the proper laws of nature. This tendency is as understandable as it is dangerous. It leaves very few laws and rather conceals the nature of lawlikeness. It defers the status of lawhood to very abstract principles, and the more it is deferred, the more difficult it is to analyze. Many think that the more special laws prove to be lawlike by being derived from the fundamental laws. I think it is the other way around; the fundamental laws inherit that status from all the other laws they systematize – and should hence not form our starting point.

There are all the other laws of physics, well known from its history, even though they have turned out to be derivative and at best approximately true: Kepler's laws, the law of gravitation, the ideal gas law, Hooke's law, etc. Hooke's law about the proportionality of extending force and spring extension is already a complicated case. It is a typical ceteris paribus law obtaining only under narrow conditions, and the deeper explanations in terms of the intermolecular forces within the material a spring is made of have revealed those conditions to a large extent. How one might understand such cases and related phenomena will be discussed only in the next chapter.

Then there are all the other sciences intending to state laws about their part of reality, the laws of chemical reactions, the Mendelian laws, the laws of volcanism, the laws of color vision, the law of supply and demand, and so forth. These are examples for sharpening and broadening our intuitions. Lange (2000) takes great care to discuss a large sample of representative laws from the various sciences.

But then there are all the silly counter-examples that may look like stating laws, but clearly don't: "all of the coins in my wallet today are made of silver", "all mountains in UK are less than 5000 feet in height", etc. Moreover, there are various doubtful cases, and it is most revealing to study what might be doubtful about them. Lange (2000, p. 13) discusses the fact, e.g., that all native Americans have blood type 0 or A that looks lawlike, but derives, as presently conjectured, from the fact that all native Americans descend from the first few immigrants crossing the Bering Strait and accidentally all being of blood type 0 or A. My favorite doubtful case, suggested to me by Köhler (2004), is the Titius-Bode law about the exponential increase of the distance of the planets from the sun. If it is true for the first planets up to Saturn, must it be true for the next one, Uranus? It was indeed found to be true for Uranus, strongly supporting its lawhood. Neptun, though, turned out to be a problem case. The issue – accident or law? – seems still to be contested. I shall return to this case. See also Lange (2000, ch. 1) for many further examples.

Like many others I shall focus here on puzzle pairs of two assertions that look very similar, although it seems intuitively clear that one expresses a law (is *a*cceptable) and the other does not (is *b*ad). The first pair is:

(12.1) (a) All (past, present, or future) humans on earth are less than 150 years old.
(b) All persons (presently or ever) in this room are less than 80 years old.

Clearly, (b) is not a law, even if it should be true, whereas (a) may well be a law. What, however, is so different about 80 and 150 or about this room and the earth?

The most famous pair is perhaps:

(12.2) (a) All uranium spheres have a diameter of less than one mile.
(b) All gold spheres have a diameter of less than one mile.

Again, (b) may well be true of our universe, but everybody denies it to be a law, whereas (a) is at least a derived law; uranium spheres even of much smaller size are unstable and decay rapidly.

And a final example: I have a bowl at home into which I only put green apples, perhaps for aesthetic reasons, and nobody else happens to put anything into it. On the other hand, biotechnologists have produced a strange kind of bowl. Whenever you put an apple into it, it turns and remains green. It is a mystery how that mechanism works, but it obviously works; at least it has done so many times. So, we have two true assertions:

(12.3) (a) All apples in mystery bowl are green.

(b) All apples in my bowl are green.

But only the first has a lawlike character. I think all these are acceptable intuitive judgments. How could we account for them? And how could we thus obtain an explication of lawlikeness?

In the 50's when the issue acquired its importance, the focus was on *essential generality*, and the hope was that it can be captured in syntactic or semantic terms provided by extensional logic. This led nowhere. That is the point of (12.1) and (12.2). (12.1a) suggests that there might be laws for very special objects at a very special place, and if one tends to reject this suggestion, then (12.2b) makes clear that there may well be universal accidental truths.

One might see a difference between the uranium and the gold spheres lying in the fact that only (12.2a) is part of a more embracive systematization. So, the nature of laws might be their *essential systematicity*, as it is also almost entailed by the best-system analysis of laws of Lewis (1994, sect. 3), which he refers back to Ramsey (1928) (we shall reach different conclusions in agreement with Ramsey 1929). I am doubtful. This proposal reverses the order of analysis; according to it we first need to know what the true laws are and can then explain lawlikeness (since any nonsense can be systematized by greater nonsense, if one takes the trouble). Moreover, it denies a further explanation of the lawlikeness of the axioms of the systematization, the most fundamental laws. Finally, I do not see why there cannot be isolated laws. To be sure, the best-system analysis does not exclude isolated laws (this is why I said that it only almost entails essential systematicity), but they always bring along an unbalance in simplicity and strength. Kepler's laws, for instance, were isolated in the beginning. Did they turn out to be laws only by their derivation from Newton's theory of gravitation? I think not. Were they considered to be laws from the beginning only because of the standing hope that they will be systematized? Again, I think not. The uncontested lawlikeness of Kepler's laws must have a better explanation. Surely, we attempt to systematize laws, and we might say a lot about what it means to systematize. However, this does not exhaust the nature of laws.

Another obvious point is *modality*. The laws in (12.1a + 2a + 3a) speak about all *possible* apples, human beings, and uranium spheres; uranium spheres *must* be smaller than one mile in diameter. By contrast, (12.1b + 2b + 3b) are accidentally true; gold spheres *may* be so large, even if they are not. Alas, the modality used here is exactly nomic necessity that we are attempting to elucidate; there is no antecedently understood modality we could plug in here. The circle is too short to be instructive.

Since the 70's, then, inquiry has focused on three further features of laws and lawlike propositions, as unanimously laid out in van Fraassen (1989, ch. 2, sect. 4), Lange (2000, sect. 1.2), and other places. Laws *support counterfactuals*, another aspect of the modal content. If I were to take that red apple and to put it into my bowl, the apple would stay red and (12.3b) would be false; if I were to put it into mystery bowl, it would turn green and (12.3a) would remain true. If that huge sphere would consist of uranium, it would explode; if it would consist of gold, I would be rich.

Laws have *explanatory force*. The apples in mystery bowl are green because they are in mystery bowl, whereas the apples in my bowl are there perhaps because they are green (if I so strictly attend to my aesthetic standards). This sphere is so small because it is made of uranium, and that sphere is so small, yes, because it is made of gold *and* because it is too expensive or too nonsensical to produce larger ones. The close relation between if-then- and because-constructions is, of course, familiar (cf., e.g., Rott (1986).

Finally, lawlikeness is wedded to *inductive inference*; laws are *confirmed* by, or *projectible* from, their instances. These locutions are usually taken to be synonymous. We shall, however, come to distinguish them; the confirmation of laws by observed instances, enumerative induction, and its projection to new instances will be understood to be two different matters. That the first of the ten apples in my bowl is green makes it likelier that all apples in my bowl are green only in the trivial sense that nine apples are less likely to have a different color than ten apples. If, however, we suspect a law or a lawlike mechanism behind mystery bowl, confirmation by the first green apple and projection to the next green apples is boosted tremendously. The same applies to the age of people in the case of (12.1a+b).

No doubt, these examples are a bit shaky, because scientific practice shows us that the confirmation of laws is much more complicated than enumerative induction; it is hard to force examples simply on the basis of the latter. On the other hand, enumerative induction is primitive not only in the sense of being too simple, but also in the sense of being basic. We cannot hope to understand the more complicated forms of scientific inference, if we cannot account for enumerative induction. This is why philosophers tend to focus on such shaky examples and such oversimplified inductive inferences, and I shall do as well.

After the first three ideas, essential generality, essential systematicity, and modality, did not elucidate lawlikeness, can we use these three basic features for further analysis? This is not clear. One might well say that such an analysis would be a case of *obscurum per obscurius*; our intuitions about lawlikeness seem firmer than the intuitions about those features. This view is endorsed by Lewis (1994, pp. 478f.) as well when he suggests that the best-system analysis of laws entails those features; it is a stand-off, as he says, and I agree. Still, I find it disappointing to take lawlikeness or essential systematicity as primitive; we shall see how fruitful it can be to reverse the order of analysis.

If we do so, the next question is which of the three basic features of lawlikeness should be taken as primary, as the starting point of analysis. There is no use in engaging in strategic argument. The only way to resolve the issue is to propose specific analyses and afterwards to compare their degree of success. However, it should be clear what my preferences are. Explanation is closely related to causation, and both seem to be difficult ideas to the analysis of which I shall proceed only in chapter 14. Moreover, I have expressed my uneasiness about counterfactuals several times. Indeed, my analysis of causation (1983) led me from Lewis' (1973a) theory of counterfactuals over Gärdenfors' (1978, 1981) epistemic account of counterfactuals, the predecessor of the AGM theory of belief revision, ever deeper into the theory of induction and thus to ranking theory. In any case, the feature relating lawlikeness to inductive inference is obviously the one most directly accessible to ranking theory that *is* a theory of inductive inference. Hence, this feature will be the entrance to my analysis of lawlikeness. This decision finds strong support, I find, in Lange (2000), for whom the relation between laws and counterfactuals is the heuristic starting point, but who also arrives at induction as the most basic issue.

Before we proceed in this sense, we face, however, a deeper issue. How are we to understand the relation between lawlikeness and its basic features? The prevailing idea has been, it seems, that the inquiry of lawlikeness should lead us to something *entitling* us to use laws in induction, explanation, and counterfactuals in the way we do. Thus, for instance, if induction is really the most basic aspect, lawlikeness should be something that *justifies* the role of laws in induction. However, this desire for justificatory insights issued in perplexity; there is just no good further justificatory candidate in sight.

The alternative is to say that lawlikeness is exhaustively characterized by its basic features, i.e., if my preference is correct, that it *consists* in the characteristic role in induction. This means to deny any deeper justification. It should be clear that I endorse this denial; I did so already at the crucial turning point of our discussion in section 4.2 (cf. also section 6.4 and the end of section 11.2). This is, as emphasized, the consequence of inductive skepticism from Hume to Goodman that taught us not to presuppose deeper justification, but to inquire instead how far rationality carries us in inductive matters. Lange (2000) takes a similar attitude when explaining what he calls the root commitment concerning the inductive strategies associated with laws.

The best-system analysis of laws also denies deeper justification; this is not to be complained. However, even then it appears disappointing. It just claims that optimal systematicity entails the three basic features, but does not explain how it does so. The strategy I shall pursue will provide more insight into the basic features of lawlikeness and their relation, even though it also refuses further justification from outside the basic features.

We are thus to study the role of laws in induction, i.e., their confirmability and projectibility. Hempel (1945) started qualitative confirmation theory, and great efforts were spent on this project in the 50's and 60's. It was, however, abandoned in the 70's, mainly because the efforts were not successful at all. Niiniluoto (1972) gives an excellent survey that displays all the unresolved incoherencies. (A further reason may have been the rise and success of counterfactual logic that answered many problems in philosophy of science, though not the problem of induction, and thus attracted a lot of the motivation originally directed to an account of confirmation.)

The only remaining viable alternative for confirmation theory then was Bayesianism, which thus came to dominate in philosophy of science. It is not clear to me why it still does, why philosophers of science mainly ignored all the offers from formal epistemology. Levi's sophisticated epistemology did not widely radiate into philosophy of science; AGM belief revision theory made a career in AI; defeasible and non-monotonic reasoning was something for logicians and not for scientists, and so forth. For instance, as much as I can agree with Lange (2000) he also turns to Bayesianism as a basis of his reflections on induction.

In any case, I believe that philosophy of science went entirely wrong in the last 30 years in this respect; the despair of qualitative confirmation theory was premature. My suggestion is, of course, that ranking theory is suitable for providing such a theory and, if our envisaged strategy works, for thus accounting for lawlikeness. In fact, my suggestion will be that the parallel between probability and ranking theory observed in chapter 10 fully extends to this field. Bayesianism is related to statistical laws and good for statistical methodology, whereas it is rather ranking theory that is related to strict or deterministic laws, i.e., the kind of laws that are here of our primary interest.

More specifically, my suggestion will be that de Finetti's (1937) story, which offered a perfect account of the relation between subjective and objective probability (although it was intended as an elimination of the latter) carries over to ranking theory and thereby offers an equally perfect account of lawlikeness, of deterministic laws and their confirmation.

This is what I shall constructively develop in the rest of the chapter. My discussion will take two dialectic turns possibly not immediately perspicuous, the move away from instantial relevance to what I shall call persistence in section 12.4 and the rise to second order attitudes in section 12.5. Some results will appear perfectly natural, others may look artificial, but are necessitated by the well-founded characteristics of ranking theory. I trust that the picture emerging in the end will be found convincing.

12.3 Laws, Symmetry, and Instantial Relevance

Our study of the inductive behavior of strict laws must start with fixing the relevant possibility space W and propositional algebra $\mathcal{A}$ over W. I shall assume an infinite sequence $a_1, a_2, \ldots$ of objects that fall under finitely many *mutually exclusive* and *jointly exhaustive* properties F_i ($i \in I$) for some finite index set I (these correspond to the Q-predicates of Carnap 1950/62, §31). We can then state generalizations of the form "all objects are F_1 or … or F_j" or "none of the objects are F_1 or … or F_j". This is quite general a framework. The objects might be material objects, F_1 might be "black raven", F_2 "non-black raven", F_3 "black non-raven", and F_4 "non-black non-raven"; and the generalization considered may be "no object is F_2 ", i.e., "all ravens are black". Or the objects might be actual movements of a point in a finite state space over finitely many points of time. For instance, the moving point may represent the position and the momentum of finitely many bodies measured within millimeters of a large space, and the points of time may be seconds within a large period. A generalization might then state that each such object, i.e., movement of a point in the state space, has a certain shape, e.g., obeys a certain difference equation.

This frame generalizes upon Spohn (2005) where I was content with computing the simplest case of two properties F and non-F. Obviously, though, I have not chosen the most general framework; a countably infinite set of objects has to be enough of infinity. I shall not strive for more mathematical generality, also because I have not inquired how ranking theory combines with continuum mathematics.

Let us fix the notation we shall use. As usual, $\mathbf{N}$ is the set of non-negative integers and $\mathbf{N}^+ = \mathbf{N} \cup \{\infty\}$. Moreover, $\mathbf{N}' = \mathbf{N} - \{0\}$ is to be the set of positive integers. Having exactly one of the properties F_i ($i \in I$) is tantamount to taking a value from I. Hence, we may represent each possibility as a sequence $\mathbf{w} = (w_1, w_2, \ldots)$, where each $w_n \in I$. $\mathbf{w}$ says that the first object a_1 has F_{w_1}, a_2 has F_{w_2}, etc. In short, our possibility space is $W = I^{\mathbf{N}'}$. Equivalently, we may represent the objects $a_1, a_2, \ldots$ by an infinite sequence of variables $X_1, X_2, \ldots$, all taking values in I and generating W in the way defined in chapter 2 so that $X_n(\mathbf{w}) = w_n$. Since we shall be dealing only with complete ranking functions, we may allow any subset of W to be a proposition; i.e., $\mathcal{A} = \mathcal{P}(W)$.

We are particularly interested in generalizations or regularities. So, for each $J \subseteq I$, let G_J be the proposition $\{\mathbf{w} \in W \mid \text{for all } n\ w_n \in J\} = J^{\mathbf{N}'}$ saying that all objects take values in J, and $G^J = \{\mathbf{w} \in W \mid \text{for all } n\ w_n \notin J\} = (I - J)^{\mathbf{N}'}$ saying that no object takes a value in J. The negative way is perhaps more useful, since the generalizations $G^i = G^{\{i\}}$ ($i \in I$) can be taken as basic and as defining all other generalizations by $G^J = \bigcap_{i \in J} G^i$. I still avoid speaking of laws, since in the end we shall not identify laws with such general propositions or regularities, although they will be closely related.

We shall moreover need a way of denoting (sequences of) singular facts. If $i \in I$ and $J \subseteq I$, let us use $\{w_n = i\}$ and $\{w_n \in J\}$, respectively, as short for $\{\mathbf{w} \in W \mid w_n = i\}$ and $\{\mathbf{w} \in W \mid w_n \in J\}$, i.e., for the proposition that the variable X_n takes the value i or some value in J. Similarly for $\{w_{n_1} = i_1, \ldots, w_{n_k} = i_k\}$ and other variations. Finally, let $\mathcal{A}_n$ be the complete algebra of propositions only about the first n

objects, i.e., generated by $X_1, \ldots, X_n$ or by propositions of the form $\{w_1 = x_1, \ldots, w_n = x_n\}$.

The conclusion of section 10.2 set the task to study the inductive behavior of laws. What might this mean? The first and most plausible guess (which will be disappointed in an instructive way) is that it means to study how such generalizations are confirmed by such sequences of singular facts – where confirmation is now to be taken in a ranking-theoretic sense, i.e., as positive relevance or the reason relation as defined in (6.1); we shall not need any more sophisticated measure of confirmation.

On which ranking function is confirmation to be based? Not on a specific one; our study is supposed to be general. We must, however, make plausible restrictions; otherwise, our investigation will lead nowhere. So, let us first restrict our study to complete negative ranking functions; I shall speak about no others. We may thus omit these two adjectives. This restriction may not be necessary; I shall explain at the end of section 12.5 where our results depended on completeness.

Let us secondly assume regularity, since confirmatory relations are undefined for infinite ranks. However, we must loosen our definition 5.21 when dealing with the uncountable possibility space W. Carnap (1971b, p. 101) also required that only all molecular propositions must have inductive probability > 0. The same effect is produced by

Definition 12.4: A ranking function κ for $\mathcal{A}$ is *regular* iff for all $n \in \mathbf{N}'$ and all non-empty $A \in \mathcal{A}_n$ $\kappa(A) < \infty$.

Regularity is henceforth to be understood in this weaker sense.

Let us thirdly conform to the maxim: "Under the law all are equal." Less cryptically, we assume that the relevant ranking function is *purely qualitative* in the sense that it is only the distribution of properties that matters and not the specific objects instantiating the properties. Certainly, the properties themselves are intended to be purely qualitative, too. This intention is, however, not written into the formal framework, and the explication of its meaning would deeply involve us in ontological dispute. That is, grue-like properties are not excluded by the formal framework as such. Of course, our condition on ranking functions is nothing but the classic assumption of *symmetry*, as precisely defined in

Definition 12.5: A ranking function κ for $\mathcal{A}$ is *symmetric* iff for all sequences x, $y \in W$ and all permutations φ of $\mathbf{N}$' $\kappa(\{w_1 = x_1, \ldots, w_n = x_n\}) = \kappa(\{w_1 = y_{\varphi(1)}, \ldots, w_n = y_{\varphi(n)}\})$ if $x_k = y_{\varphi(k)}$ for $k = 1, \ldots, n$.

The symmetry assumption has a most venerable history that need not be recapitulated here. Let me only mention that it played a fundamental role in de Finetti's philosophy of probability (under the name "exchangeability"). And it was a basic postulate in all versions of Carnap's inductive logic (in relation to the objects; in relation to the properties it turned out to be more problematic). The most comprehensive treatment is found in van Fraassen (1989, parts III and IV), who indeed went so far as to argue that symmetry should take the key role in scientific reasoning replacing the allegedly confused idea of lawlikeness. We shall attend to this claim. In any case, we restrict our study to regular and symmetric ranking functions.

What counts for symmetric ranking functions is only how many times the properties are instantiated. Let us introduce a bit of notation for this purpose. I shall use $\mathbf{n} \in \mathbf{N}^I$ as variables for families or I-tuples $(n_i)_{i \in I}$ of non-negative integers indexed by I. I often use the corresponding italic n for denoting $n = \sum_{i \in I} n_i$. By speaking of I-tuples I sometimes use I also to denote its own cardinality. For $J \subseteq I$ we set $\mathbf{n}_J = (n_i)_{i \in J}$ and $\mathbf{n}^J = (n_i)_{i \in I-J}$, so that $\mathbf{n} = (\mathbf{n}_J, \mathbf{n}^J)$; hence, in particular $\mathbf{n}_i = n_i$. Furthermore, $(\mathbf{n}^i, m_i)$ is to denote the I-tuple $\mathbf{n}$ with n_i replaced by m_i, $(\mathbf{n}^{i,j}, m_i, m_j)$ the I-tuple $\mathbf{n}$ with n_i and n_j replaced by m_i and m_j, etc. $\mathbf{0}$ is to be the I-tuple $(0, \ldots, 0)$. Moreover, $\mathbf{m} \leq \mathbf{n}$ says that $m_i \leq n_i$ for all $i \in I$ and $\mathbf{m} < \mathbf{n}$ says that $\mathbf{m} \leq \mathbf{n}$ and $m_i < n_i$ for at least one $i \in I$. Finally, let us denote by $E_{\mathbf{n}}$ the proposition that the properties F_i realize with the absolute frequencies n_i ($i \in I$) in the first n objects. With this notation we can first define:

Definition 12.6: A function f from $\mathbf{N}^I$ into $\mathbf{N}$ is *non-decreasing* iff $\mathbf{m} \leq \mathbf{n}$ entails $f(\mathbf{m}) \leq f(\mathbf{n})$ for all $\mathbf{m}, \mathbf{n} \in \mathbf{N}^I$. It is *minimitive* iff for all $\mathbf{n} \in \mathbf{N}^I$ $f(\mathbf{n}) = \min_{i \in I} f(\mathbf{n}^i, n_i + 1)$. It *represents* a regular symmetric ranking function κ or is the *representative function* of κ iff $\kappa(E_{\mathbf{n}}) = f(\mathbf{n})$.

Then we have the obvious

Theorem 12.7: Each regular symmetric ranking function κ is represented by some function f from $\mathbf{N}^I$ into $\mathbf{N}$. A function f is a representative function (of some ranking function) if and only if it is non-decreasing, minimitive, and $f(\mathbf{0}) = 0$.

So, our study of regular symmetric ranking functions will reduce to the study of such representative functions.

Symmetry has a surprising consequence. Recall first that one could prove the so-called null confirmation of laws within Carnap's λ-continuum of inductive methods. That is, each confirmation function obeying the so-called λ-principle (that entails regularity and symmetry) gives, a priori and a posteriori, probability 0 to each generalization G_J ($J \subset I$) (see already Carnap 1950/62, §110F, or Stegmüller 1973, pp. 501f.). Sloppily put, there is nothing to confirm about laws. This was a shocking discovery. Carnap (1950/62, §110G) thus proposed to replace the issue of the confirmation of laws by the issue of the confirmation of the next instance(s). Who cares for the infinite generalization? What matters are only the instances we meet in our life time. In this way, enumerative induction turned into the principle of positive instantial relevance (that was derivable from the λ-principle) and found a Bayesian home (after the failure of qualitative confirmation theory).

Not all were convinced by this step and rather rejected Carnap's λ-principle. In particular, Hintikka (1966) showed how to do better by proposing a two-dimensional continuum of inductive methods that respects regularity and symmetry and gives positive a priori probability to a generalization that a posteriori converges to 1 or 0, depending on its truth or falsity (cf. Hintikka, Niiniluoto 1976, and Kuipers 1978). I am unsure about the present sympathies for that proposal.

Anyway, the issue is radically avoided within the ranking-theoretic context. Given symmetry there is no difference between belief in the next instance and belief in the generalization about the infinitely many future instances. Suppose that after having observed the first n objects you believe that the next object will not have F_i; that is, $\kappa(\{w_{n+1} = i\}) = r$ for some $r > 0$. Because of symmetry you then believe that any future object will not have F_i; that is $\kappa\{w_{n+k} = i\} = r > 0$ for any $k \geq 1$. Since κ is complete, the law 5.4(c) of infinite disjunctions holds; and this entails that you believe with the same strength r that *none* of the future objects has F_i; that is $\kappa(\bigcup_{k\geq 1} \{w_{n+k} = i\}) = r > 0$. I welcome this conclusion, even though it will sound unintuitive for the probabilistically trained. Whether we can return to naïve or unbiased intuitions is not clear to me. In any case, the conclusion follows from the well-founded principles of ranking theory. (One may avoid the conclusion by giving up completeness. However, this is not the point here. Those complaining about the conclusion will also complain about the fact that the next instance not having F_i is as firmly expected as the next million instances not having F_i.)

In this way, Carnap's problem of the null confirmation of infinite generalizations dissolves in the ranking-theoretic frame, and the transition to instantial relevance that was a doubtful auxiliary move in the Bayesian context turns out fully legitimate here. So, the ranking function governing the confirmation of generalizations should obey instantial relevance. This can come in a stronger or weaker form. The *principle of positive instantial relevance* (*PIR*) says that, given any evidence $E \in \mathcal{A}_n$ concerning the first n objects, the n+1st object a_{n+1} having the property F_i confirms a_{n+2} having F_i. The weaker principle of *non-negative instantial relevance* (*NNIR*) requires only that the contrary is not confirmed, i.e., that given any E_n the two-sided rank of a_{n+2} having F_i is not lowered by a_{n+1} having F_i.

Confirmation might be given here various senses or measures. Let us consider only two basic senses; there is no point in this chapter in being more sophisticated in this respect. One sense is that the a posteriori two-sided rank (i.e., after evidence E) of a_{n+2} having F_i given a_{n+1} has F_i is higher than given a_{n+1} lacks F_i. The other sense is that this rank conditional on a_{n+1} having F_i is higher than the unconditional rank of a_{n+2} having F_i:

Definition 12.8: Let κ be a regular symmetric ranking function for $\mathcal{A}$ and τ the corresponding two-sided ranking function. Then κ *satisfies PIR_c* (i.e., *PIR* in the *conditional* sense) iff for all $E \in \mathcal{A}_n$ ($n \geq 0$) and all $i \in I$ $\tau(\{w_{n+2} = i\} \mid E \cap \{w_{n+1} = i\}) > \tau(\{w_{n+2} = i\} \mid E \cap \{w_{n+1} \neq i\})$. And κ *satisfies PIR_n* (i.e., *PIR* in the *non-conditional* sense) iff for all $E \in \mathcal{A}_n$ and all $i \in I$ $\tau(\{w_{n+2} = i\} \mid E \cap \{w_{n+1} = i\}) > \tau(\{w_{n+2} = i\} \mid E)$. Finally, κ *satisfies*, respectively, *$NNIR_c$* or *$NNIR_n$* iff the weak inequality $\geq$ holds in *PIR_c* or *PIR_n* instead of the strict >.

It is an obvious consequence of the law (5.18a) of disjunctive conditions that PIR_n entails PIR_c, that $NNIR_c$ entails $NNIR_n$, and that none of the reverse entailments holds. Which of the two senses should we prefer? We had already discussed the issue. PIR_c just says that a_{n+1}'s having F_i is a *reason* for a_{n+2}'s having

F_i (conditional on $E_{\mathbf{n}}$) in the sense of definition 6.1 (or 6.3). There, before (6.2), I have already argued that PIR_c is preferable to PIR_n. Likewise, NNIR_c is preferable to NNIR_n, as is also clear from the fact NNIR_n is even compatible with the negation of NNIR_c, i.e., with *negative* instantial relevance in the conditional sense.

Whichever sense we consider, we are in, however, for unpleasant surprises. A minor observation is this: Whereas symmetry implies NNIR in the probabilistic context (where we need not distinguish NNIR_c and NNIR_n) (cf. Humburg 1971, p. 228), this is not so in the ranking context; it is easy to construct examples for both, representative functions violating NNIR_c and representative functions satisfying NNIR_c. The main observation, though, is: Whereas regularity, symmetry, and the so-called Reichenbach axiom imply PIR in the probabilistic context (cf. Humburg 1971, p. 223), PIR turns out to be unsatisfiable in the ranking context even in its weaker sense PIR_c.

Theorem 12.9: There is no regular symmetric ranking function for $\mathcal{A}$ satisfying PIR_c.

A direct proof would be tedious and not particularly revealing. I shall defer the proof after (12.20), when we have collected all the pieces explaining the unsatisfiability of PIR_c.

What does this negative result teach us about lawlikeness? Since the confirmation of the next instances is tantamount to the confirmation of generalizations (concerning all future instances) relative to symmetric ranking functions and since PIR_c is not generally realizable, we must conclude that generalizations or regularities are not always confirmed by their positive instances. As the proof of (12.9) will show, this conclusion cannot be hedged. All generalizations G_J (concerning future instances) can be confirmed by at most finitely many positive instances. Hence, it seems we have reached a dead end; we cannot use enumerative induction, i.e., confirmability by positive instances for characterizing strict laws.

We may settle for the weaker NNIR_c, if this is the only ranking-theoretically feasible way. This seems unsatisfactory, though, since it leaves arbitrary room for instantial irrelevance; it seems hard to accept that the confirmation by positive instances should fail most of the time. We shall eventually discover, however, that we are deceived at this point; we shall be able to fully restore positive instantial relevance or enumerative induction, though only after some dialectic labor. For the time being we should accept the negative conclusion.

12.4 Laws and Persistence

We have to take a new start. What else might it mean to study the inductive behavior of laws? One might say that the basic fault of the considerations so far was that they inquired the confirmation of generalizations or regularities, while our intention is to apply enumerative induction only to laws, which somehow are something more special. However, the study above gave no hint what the specialty might be.

One might also say that the previous considerations were misguided insofar as they inquired PIR_c and $NNIR_c$ conditional on any evidence whatsoever, even evidence that outright falsifies the generalization in question. It is clear, though, that this objection does not literally apply. I carefully stated that what gets confirmed (or not) is always the generalization concerning the future (or unobserved) instances and not the full generalization. However, this sounds like a lame excuse. It still appears odd to worry about the confirmation of a generalization about the future that has been falsified in the past, perhaps many times.

In a way, my point will be that this is not so odd, after all. This has to do with the projectibility of strict laws. I had mentioned in section 12.2 that the projectibility of a law is usually equated with its confirmability; we project its past success onto the future. This we do with lawlike, not with accidental generalizations. But we may give projectibility a stronger reading, not as the extension of past observations to future cases, but simply as the continuous application of the law to future cases, whatever the past. I have to explain this dim suggestion.

Let us ask: *What does it mean to believe in a law?* As long as we do not know what a law is it is hard to answer. It is clear, though, what it is to believe in a regularity or generalization. A generalization is some proposition $G_J \in \mathcal{A}$ $(J \subset I)$, and to believe in it means $\kappa(\overline{G_J}) > 0$, relative to any negative ranking function κ. However, this belief can be realized in many ways, even if we assume κ to be symmetric; the inductive relations among the various instances can take many different forms. Which form may or should they take? It is clear that as long as one observes only positive instances the belief in G_J is maintained or even strengthened; this is what $NNIR_c$ implies at least for generalizations of the form G^i ($i \in I$). What happens, though, when one observes negative instances? If the negative instances get believed, then G_J gets disbelieved according to any κ; this is a trivial matter of deductive logic. However, with regard only to the future instances, anything might happen according to κ, even if we impose symmetry and $NNIR_c$. Let us more closely look at two paradigmatic (and extreme) responses to negative evidence in order to better understand the spectrum of possible responses. I call them the persistent and the shaky attitude.

If you have the *persistent* attitude, your belief in further positive instances is unaffected by observing negative instances, i.e., $\tau(\{w_{n+1} \in J\} \mid \{w_1, \ldots, w_n \notin J\}) = \tau(\{w_{n+1} \in J\}) > 0$. If, by contrast, you have the *shaky* attitude, your belief in further positive instances is destroyed by the first negative instance (and due to $NNIR_c$ also by several negative instances), i.e., $\tau(\{w_{n+1} \in J\} \mid \{w_1, \ldots, w_{n-1} \in J\} \cap \{w_n \notin J\}) \leq 0$.

I want to suggest that the different attitudes are distinctive of treating generalizations as lawlike or accidental. Let us look at our puzzle pairs (12.1 – 3). You may believe (12.1b) that all persons in this room are less than 80 years old, for whatever reason, perhaps because you have seen only younger ones or perhaps because someone has indicated that allowance to this room is subject to a rule. Your belief may be strengthened by meeting further people below 80, but it fades when you find an older one. That is, in this case you do not only believe that not all persons in the room are below 80 (this is dictated by logic), but you also lose your confidence that there will be no further exceptions. Your attitude is shaky. By contrast, you also believe (12.1a) that all humans on earth are below 150. Now you encounter someone claiming to be older. Probably, you would not accept the claim, whatever her credentials are. Perhaps, her birth certificate is a fake. But even if you cannot find fault with her claim and tend to accept it, you would think that she remains an exception and expect the next people you meet to be below 150. That is, your attitude tends to be persistent.

Or take (12.2). If you bump into a one mile gold sphere on your intergalactic journey, you would be surprised – and start thinking there might well be further ones. If, however, you stumble upon a uranium sphere that large (and survive it), you would be surprised again; but then you would start investigating this extraordinary case while sticking to your reasons for declaring such a case impossible and expecting no further exception.

If you hear of my bowl you might well believe that all apples in it are green (12.3b). But if I offer you a red apple from it, you lose your trust in the story; the other apples in the bowl might then have any color. By contrast, if you get convinced of mystery bowl (12.3a) and then find a red apple in it, you might think that its mysterious mechanism is not always working properly or that some special apples are apparently immune to the mechanism. But you would continue projecting the rule into the future.

The Titius-Bode law mentioned above is a real-life example where astronomers had split attitudes. Some were shaky and took an apparent counter-example as evidence that it was never a law in the first place and that further counter-examples would be no surprise. Others were persistent or became persistent after the discovery of Uranus, trying to explain or explain away the later apparent counter-example of Neptune and thus to restore the law. As far as I see, the distinction between the persistent and the shaky attitude fits to other examples discussed in the literature as well, as listed, e.g., in Lange (2000, pp. 11f.).

I am aware that these illustrations sound only partially convincing. One would be better prepared to say how one would respond to such examples if they would be described in more detail, especially concerning the evidence that led one to believe in the relevant generalization in the first place. However, I am painting black and white in order to elaborate the opposition between the two ideal types of the persistent and the shaky attitude. I admit there is a lot of grey as well. Still, I insist on my ideal types. There are four considerations to elucidate the grey area.

First, our intuitions are likely to be probabilistically biased. A description of the examples in terms of subjective probabilities would certainly diverge from one in terms of beliefs and expectations. This might be part of explaining a reluctance of accepting my presentation above.

Second, my ideal types delimit a broad range of less extreme attitudes. Being shaky means to be very shaky; the belief in further positive instances may instead fade more slowly. And being persistent means to be strictly persistent; the belief in further positive instances may instead fade so lately that we never come to the point of testing it. Our investigation of symmetry and NNIR in section 12.5 will give a clearer impression of all the possibilities within this range. Maybe a less persistent or a less shaky attitude fits the examples still better.

Third, it is clear that when confronted with apparent counter-instances to a conjectured generalization, we never just accept them shaking our generalization or persistently write them off. Rather, as already indicated in my illustrations, we carefully study the apparent counter-instances in order to find out whether they really are exceptions and what makes them exceptions. Thereby we hope to be able to state appropriate qualifications that turn the original generalization into an exceptionless law. This process is often accompanied by widening the conceptual field, by considering more properties to be potentially relevant. In short, we take the original law only as a ceteris paribus law. This is an issue I shall more thoroughly discuss only in the next chapter, which will certainly contribute to the overall adequacy of my explications.

The fourth point is, I think, the most important. My discussion of the examples was misleading in a way. I simply asked you for your inductive intuitions in all these cases and tried to push them to the persistent or the shaky extreme. But this was an unguarded maneuver, and you may have been reluctant to yield because I was pushing you into an obviously absurd direction. The persistent attitude sticks to its predictions come what may, even given overwhelming counterevidence; it is incapable to learn. And this is just silly and contrary to any inductive intuitions.

However, this would be a subtle misunderstanding against which I can guard only after having invited it. I started with the belief in a generalization, observed that it can be realized in many ways in ranking terms, and distinguished two extreme ways. Then I suggested that to treat the generalization as a deterministic law means to have the persistent attitude. That is, *if* you would firmly believe in the law and nothing else, then you would have the persistent attitude; this was to be my claim. However, you never simply believe in the law and nothing else. You more or less tentatively believe in the law, you always reckon with alternative laws, and you always are inmidst processing factual information. Your inductive situation is never so unambiguous, and therefore you may have rightfully doubted my description of the examples. Hence, it is important to observe my proviso "if you would firmly believe only in the law"; then, I submit, the persistent attitude is much more plausible as an expression of lawlikeness.

This observation also preliminarily dissolves the complaint about the silly learning incapability of the persistent attitude. The doxastic attitude that expresses the belief in a law need not display learning capability by itself. What is rationally required is only that one is not tied to such a doxastic attitude, but is able to change it in response to the evidence. In fact, we have arrived here at a most important point. I have given the ranking-theoretic account of the change of belief in propositions already in chapter 5, and thus derivatively in this chapter the ranking-theoretic account of the confirmation of propositions and of generalizations in particular. If, however, the belief in a law is a certain kind of doxastic attitude, we so far have no account of the change and of the confirmation or disconfirmation of such an attitude i.e., of the belief in a law. Thus, the first-order account of the confirmation of propositions needs to be complemented by a so far missing second-order account of the confirmation of such first-order attitudes. Thereby, and only thereby, the complaint can be definitely rejected. This will be our task in the next section; and I can promise complete success. For the moment, the point was only to distinguish the (so far unexplained, second-order) confirmation of laws from the (ranking-theoretically explained, first-order) confirmation of propositions, thus clarifying possible confusions concerning my claim about what is characteristic of a belief in a law.

My suggestion is idiosyncratic at most insofar as it is definitely couched in ranking-theoretic terms. Apart from this, however, it has ample historic precedent. My chief witness, no doubt, is Ramsey (1929) who states very clearly: "Many sentences express cognitive attitudes without being propositions; and the difference between saying yes or no to them is not the difference between saying yes or no to a proposition" (pp. 135f.). And "… *laws are not either*" (namely propositions – my emphasis) (p. 150). Rather: "The general belief consists in (*a*) A general enunciation, (*b*) A habit of singular belief" (p. 136). This is what has become known also as the view that laws are not general statements, but rather inference rules or licenses, as it was advanced by Schlick (1931), Ryle (1949, ch. 5), Toulmin (1953, ch. 4), and others. And it is exactly what I have suggested above: a belief in a law is not only the belief in a generalization, but is expressed by the persistent attitude that is perfectly characterized as a habit of singular belief.

Of course, one should not read too much of the present context into Ramsey's writings; he was occupied with the problems of his days, for instance, with rejecting Russell's acknowledgment of general facts and with thus finding a different interpretation for general sentences (cf. Sahlin 1991, pp. 138ff.). Sahlin (1997, p. 73) also mentions that Ramsey's view of general propositions was apparently influenced by Hermann Weyl. Still, his attempts to construct a pragmatic theory of belief, which are also reflected in my brief quotation, are surprisingly modern. There does not appear to be a 70 years gap when Lange (2000) says in his much more elaborate book that "the root commitment that we undertake when believing in a law involves the belief that a given inference rule possesses certain objective properties, such as reliability" (p. 189). (Lange then continues to explain the differences between the older inference license literature and his position which hide in the reference to "certain objective properties such as reliability".)

The inference license view of laws was not generally well received, mainly, I think, because it was hard to see from a purely logical point of view what the difference might be between accepting the axiom "all *F*'s are *G*'s" and accepting the inference rule "for any *a*, infer *Ga* from *Fa*". The only difference is that the rule is logically weaker; the rule is made admissible by the axiom, but the axiom cannot be inferred by the rule. What else besides this unproductive logical point could be meant by the slogan "laws are inference rules" was always difficult to explain.

Still, one might say that the inference license view puts more emphasis on what to do in the single case. This emphasis need not be mere rhetorics. It is reflected, I find, in my notion of persistence and thus receives a precise induction-theoretic basis. We could often observe (e.g., in section 11.5) that ranking functions can be understood as (possibly quite complex) defeasible inference rules that can be taken to be reliable, even if they are not universally valid.

Hence, the mark of laws is not their universal validity that breaks down with one counter-instance, but rather their operation in each single case that need not be impaired by exceptions. This point is most prominent in Cartwright (1983, 1989) and her continuous forceful efforts to argue that what we have to attend to are the capacities and their (co-)operation taking effect in the single case. I shall elaborate on my concurrence with Cartwright in the next chapter.

Finally, I should mention that the idea of persistence closely resembles the notion of resilience that is central to Skyrms' (1980, part I) analysis of probabilistic causation. However, since that context is somewhat different and since there is no point now in a comparative discussion, I preferred to choose a different label.

So much for some important agreements. The most obvious disagreement is with Popper, of course. Given how much philosophy of science owes to Popper, my account is really ironic, since it concludes in a way that it is the mark of laws that they are *not* falsifiable by negative instances (this is the persistent attitude); only accidental generalizations are so falsifiable (this is the shaky attitude). Of course, the idea that the belief in laws is not given up so easily is familiar at least since Kuhn's days, and even Popper (1934, ch. IV, § 22) insisted from the outset that the falsification of laws proceeds by more specialized counter-laws rather than simply by counter-instances. However, I have not seen the point elsewhere being so radically stripped to its induction-theoretic bones.

Let me sum up this discussion. My core claim was that the belief in a strict law consists in the persistent attitude. The latter in turn is characterized, in formally precise terms, by the ranking-theoretic *independence* of the instantiations of the law, i.e., of the variables X_1, X_2, … In other words, if ξ is any negative ranking function for I, we may define λ_ξ as the independent and identically distributed infinite repetition of ξ, the representative function f_ξ of which is given by $f_\xi(\mathbf{n}) = \sum_{i \in I} n_i \cdot \xi(i)$. If ξ is regular, λ_ξ is regular, too. Of course, λ_ξ is symmetric. To say that λ_ξ satisfies (any version of) NNIR is correct, but misleading, since λ_ξ is distinguished by perfect instantial irrelevance. λ_ξ persists in ξ for the next instance whatever the previous experience.

Does the core claim help us to say what a law *is*? So far, I was careful only to analyze the *belief* in a law. The tricky point is that the belief in a law turned out, following Ramsey, to be not (merely) the belief in something; "belief in a law" is a not further parsable phrase. One might object that this shows that the approach is the wrong one to start with, and I would retort by pointing to the not so impressive alternatives. Let us not repeat this. On the contrary, I tend to say that such a λ_ξ *is* a law (and not just a belief in a law); but this looks obviously like a rhetorical trick. We are here in a real linguistic predicament.

To resolve the predicament, I propose to call the λ_ξ's *subjective laws*. This is sufficiently artificial a term to indicate the rhetorical move explicitly, not surreptitiously. And there is a twofold justification behind that move. First, we shall be able in the next section to fully explain the phrase "belief in a law" in a parsable manner, as belief in something, namely exactly in such λ_ξ's; this has to do with the distinction of first-order and second-order attitudes already indicated. Secondly, the adjective "subjective" is not merely to signify that subjective laws are still something doxastic, i.e., entertained by doxastic subjects. The adjective's significance runs deeper. Let me explain.

I had emphasized from the beginning that lawlikeness abstracts from the truth dimension of laws. Likewise, the talk of belief in laws is silent about the truth of laws. Thus, it is clear that subjective laws are only possible laws that may or may not obtain. Still, laws in that wider sense must be something that *can* obtain, can be true or false. Subjective laws, however, are generally not of this kind. ξ may so far be *any* ranking function for the index set I of properties or, what comes to the same, for the next instance. And we do not yet know what it could mean to call such a ranking function to obtain, to be true or false; it can at best be true *of* a subject. Likewise, we do not know what it means to call the subjective law, the infinite independent repetition λ_ξ of ξ, true or false. λ_ξ is as subjective as ξ; this is the adjective's real significance.

In chapter 15 I shall advance an account of objectivization of ranking functions distinguishing those ranking functions that can be called true or false. Of course, I shall call then λ_ξ an objective law if it is based on an objectivizable ξ. This is presently a hardly intelligible announcement. It indicates, though, the strategy behind my terminology and the fact that our present topic of lawlikeness will be completed only then. In this chapter we must be content with having worked up at least to the notion of a subjective law. Let us conclude our efforts with

Definition 12.10: Let Ξ be the set of negative ranking functions for *I*. For any $\xi \in \Xi$ λ_ξ is to be that ranking function for $\mathcal{A}$ according to which all of the X_n ($n \in \mathbf{N}'$) are distributed according to ξ and are mutually independent, i.e., the representative function f_ξ of which is defined by $f_\xi(\mathbf{n}) = \sum_{i \in I} n_i \cdot \xi(i)$. λ_ξ is called the *subjective law* for $\mathcal{A}$ *based* on ξ. λ is a *subjective law* for $\mathcal{A}$ iff $\lambda = \lambda_\xi$ for some $\xi \in \Xi$. The set of all subjective laws for $\mathcal{A}$ will be denoted by Λ.

I would like to point out here that $\xi \in \Xi$ and $\mathbf{n} \in \mathbf{N}^I$ are entities of the same formal kind; both are functions from *I* into **N**. (There is the difference that ξ must take 0 as a value, whereas **n** need not; to get rid of the difference, consider $\mathbf{n} - \min n_i$ instead of **n**.) This remark appears to be a whim. However, I think it has a deep significance. It states that ranking functions (something subjective in the mind) and absolute frequencies (or surplus of absolute frequencies) (something objective in the external world) are of the same kind. Subjective probabilities and relative frequencies are also entities of the same kind; and their relation is fundamental to probabilistic epistemology. Thus, the analogy is that subjective ranking functions likewise find their external anchoring in absolute frequencies, a point already indicated in section 10.3 (under the label "reality aspect'") and repeated here as the formal occasion arose. It will be deepened in chapter 15.

12.5 The Confirmation of Laws

Having studied the ranking-theoretic confirmation of generalizations in section 12.3 with instructive, even if unexpected results, we concluded that we should rather study the confirmation of laws. Now, (subjective) laws turned out to be not propositions, but a special kind of ranking functions. What it could mean to confirm such laws is, however, as yet unexplained. Can we make precise sense of it?

Yes, we can. Fortunately, there is clear precedent provided by Bruno de Finetti's philosophy of probability. Given the close similarity between probability and ranking theory, we may attempt to translate de Finetti's account of statistical laws. We shall see that this attempt indeed works in a satisfactory way.

What sets the attempt going is the observation that (in the simplest case) a statistical hypothesis consists in a Bernoulli measure for an infinite sequence of random variables according to which these variables are independent and identically distributed. Thus, *what I called a subjective law is nothing but the ranking-theoretic analogue to a Bernoulli measure*! Positivistically minded, de Finetti was suspicious of objective probabilities and of statistical laws hypothesizing them. In his (1937) he thus developed an account perfectly explaining them away. His famous representation theorem showed that each symmetric probability measure for the infinite sequence of random variables is a unique mixture of Bernoulli measures. If the symmetric measure expresses your subjective probabilities and the Bernoulli measures represent hypotheses about objective probabilities, your subjective opinion is hence a unique mixture of objective statistical hypotheses, the weights of the mixture representing your credence in these hypotheses. The mixture changes through evidence that favors the hypotheses close to the observed frequencies and disfavors the other ones. In fact, if the evidence converges to a certain limit of relative frequencies the mixture converges to the Bernoulli measure taking these limiting relative frequencies as objective probabilities (provided this measure is in the so-called carrier of the original mixture). Thus, the learning process satisfies the so-called Reichenbach axiom (under the proviso mentioned). (For all this see, e.g., Loève 1978, Sect. 30.3 and 32.4, Jeffrey 1971, sect. 10, or the relevant contributions to Jeffrey 1980.)

(Note, by the way, that de Finetti's representation theorem is a special case of the ergodic theorem proved by George D. Birkhoff in 1931. Despite the efforts of

Skyrms (1984, pp. 52ff.) it is not so clear, however, whether the more general ergodic theorem can be given the same philosophical significance.)

De Finetti intended his story to be eliminativistic. Since the mixtures are always unique, he could confine himself to talking only of the first-order symmetric subjective probabilities. All the rest, objective statistical hypotheses and mixtures thereof expressing second-order subjective probabilities in objective hypotheses, may be taken as mere as-if constructions.

The positivistic motive is not mine. Also, the business about objectivity and subjectivity is not yet ours. I had emphasized that subjective laws are still subjective and that the objectivity issue will be raised only later on. But apart from this, every detail of de Finetti's account is perfectly suited for translation. Well, almost; there are some niceties that did not show up in the special case of two properties, i.e., $I = \{1, 2\}$, belabored in Spohn (2005) and hence were underestimated there. They add to the list of divergences between probability and ranking theory.

So, we start from the set Λ of possible subjective laws for $\mathcal{A}$, and we consider a complete negative ranking function ρ for Λ that is to represent our (dis-)belief in laws (or rather in subsets of Λ, i.e., types of laws or law-propositions). This time, this makes proper sense; ρ represents second-order beliefs in independent first-order objects, i.e., subjective laws. We can mix then the possible first-order subjective laws by the second-order attitude ρ, as defined in (5.30), and we thus arrive at some first-order ranking function κ for $\mathcal{A}$ that expresses our (dis-)belief in factual propositions; of course, factual disbelief is never guided by just one law taken for sure.

Referring to such mixtures as weighted, or weighted averages, may raise too firm probabilistic associations. Let us better call ρ an *impact function*; the subjective law λ has *impact* $\rho(\lambda)$ on the mixture κ. This loosely agrees with our picture in section 6.3 that the balancing of ranks is rather a matter of forces, of pulls and pushes, and not a matter of weighing. Of course, the impact of λ is the larger, the *smaller* $\rho(\lambda)$, since ρ expresses disbeliefs.

Our task thus is to investigate the precise relation between such second-order mixtures of subjective laws by impact functions and first-order attitudes or regular symmetric ranking functions defined for factual propositions (or their representative functions).

Analytically, i.e., within a real-valued framework, the issue would be quite straightforward. There, a representative function would be any non-decreasing function for I-tuples of non-negative reals starting at the origin (minimitivity would have no analogue in the real-valued framework), subjective laws would simply be I-dimensional hyperplanes passing through the origin, the impact function ρ would shift the laws or hyperplanes upwards, and the mixture of all the shifted hyperplanes (as defined in 5.30) is just their lower envelope. This makes clear that, if and only if a non-decreasing function is concave, it can be conceived as the lower envelope of its tangential or supporting hyperplanes. This geometric picture helps intuition enormously, and it makes clear that we are going to move on mathematically well-trodden paths. We only have to translate the picture into our discrete framework. (I am indebted here to Günter M. Ziegler and Friedrich Roesler for crucial hints to convex geometry and analysis.)

First, we have to take care of regularity:

Definition 12.11: The impact function ρ for Λ is *proper* iff $\rho(\lambda_\xi) < \infty$ for at least one regular $\lambda_\xi \in \Lambda$ (or regular $\xi \in \Xi$).

Theorem 12.12: The mixture κ of Λ by ρ is regular in the sense of (12.4) if and only if ρ is proper.

Proof: If ρ is proper, κ is obviously regular. Conversely, let ρ not be proper. So, whenever $\rho(\lambda_\xi) < \infty$, there is some $i \in I$ such that $\xi(i) = \infty$. Hence, if $\mathbf{1} = (1, \ldots, 1)$, $\kappa(E_\mathbf{1}) = \infty$, since $\lambda_\xi(E_\mathbf{1}) = \infty$ for all λ_ξ with $\rho(\lambda_\xi) < \infty$. □

Our further inquiry best proceeds by embedding our representative functions mapping $\mathbf{N}^I$ into $\mathbf{N}$ (and thus being subsets of $\mathbf{N}^{I+1}$) into I+1-dimensional Euclidean space $\mathbf{R}^{I+1}$. (Recall $\mathbf{R}$ is the set of real numbers and $\mathbf{R}^+$ the set of non-negative reals.) For each set $S \subseteq \mathbf{R}^{I+1}$ the *convex hull* of S is defined as $\mathcal{H}(S) = \{\mathbf{x} \in \mathbf{R}^{I+1} \mid \mathbf{x} = \sum_{k=1}^{n} \alpha_k \mathbf{x}_k$ for some $\mathbf{x}_1, \ldots, \mathbf{x}_n \in S$ and $\alpha_1, \ldots, \alpha_n \geq 0$ such that $\sum_{k=1}^{n} \alpha_k = 1\}$. More specifically, for each non-decreasing function f from $\mathbf{N}^I$ into $\mathbf{N}$ let $\mathcal{H}(f)$ denote the convex hull of $\{(\mathbf{n}, x) \mid \mathbf{n} \in \mathbf{N}^I$ and $f(\mathbf{n}) \geq x \in \mathbf{R}\}$. Thus, $\mathcal{H}(f)$ is not simply the convex hull of $\{(\mathbf{n}, f(\mathbf{n})) \mid \mathbf{n} \in \mathbf{N}^I\}$, but includes all points below ("below" taken relative to the I+1st dimension). Obviously, $\mathcal{H}(f)$ is closed and the upper boundary of $\mathcal{H}(f)$ is given by some function g from $(\mathbf{R}^+)^I$ into $\mathbf{R}^+$ such that $g(\mathbf{x}) = \max\{y \mid (\mathbf{x}, y) \in \mathcal{H}(f)\}$. $\mathcal{H}(f)$ thus is what is called the *hypograph* of g. Since

$\mathcal{H}(f)$ is convex, g is *concave* (or *convex from below*) in the sense that for all $\mathbf{x}$, $\mathbf{y}$ $\in (\mathbf{R}^+)^I$ and $\alpha \in [0, 1]$ $g(\alpha\, \mathbf{x} + (1-\alpha)\, \mathbf{y}) \geq \alpha\, g(\mathbf{x}) + (1-\alpha)\, g(\mathbf{y})$. (For all these terms and concepts cf. Stoer, Witzgall 1970 or Rockafellar 1970.)

Let us call the concave function g just constructed the *real spreading* of f and denote it by $f^{\mathbf{R}}$. With it, we can most naturally explain concavity also for the original representative functions:

Definition 12.13: A non-decreasing function f from $\mathbf{N}^I$ into $\mathbf{N}$ is *concave* iff for all $\mathbf{n} \in \mathbf{N}^I$ $f^{\mathbf{R}}(\mathbf{n}) = f(\mathbf{n})$, i.e., iff the real spreading of f is an extension of f. A regular symmetric ranking function κ is *concave* iff its representative function is concave.

In other words, f is concave iff all points $(\mathbf{n}, f(\mathbf{n}))$ are boundary points of $\mathcal{H}(f)$, and f is not concave iff for some $\mathbf{n}$ $f(\mathbf{n}) < f^{\mathbf{R}}(\mathbf{n})$ so that $(\mathbf{n}, f(\mathbf{n}))$ is an inner point of $\mathcal{H}(f)$.

Thereby, we immediately arrive at a first representation result:

Theorem 12.14: A ranking function κ for A is regular, symmetric, and concave if and only if it is the mixture of the set Λ of subjective laws by some proper impact function ρ.

Proof: If κ is such a mixture, it is regular because of (12.12) and symmetric because all $\lambda_\xi \in \Lambda$ are symmetric. It is also concave: Let f_ξ be the representative function of the law λ_ξ. Then, $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ is just a non-descending linear function or an I-dimensional hyperplane, and $\mathcal{H}(f_\xi + \rho(\lambda_\xi))$ is just the closed half-space below $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$. Since the representative function f of κ is given by $f = \min_\xi (f_\xi + \rho(\lambda_\xi))$, $\mathcal{H}(f)$ is simply the intersection of all the $\mathcal{H}(f_\xi + \rho(\lambda_\xi))$, and each $(\mathbf{n}, f(\mathbf{n}))$ indeed a boundary point of H(f). Hence, f and κ are concave.

Conversely, suppose κ is regular, symmetric, and concave. We know that any closed convex set C in $\mathbf{R}^{I+1}$ is the intersection of the closed half-spaces containing it (cf. Stoer, Witzgall 1970, p. 98). It is indeed the intersection of the *supporting half-spaces H* of C (defined as minimally containing C; i.e., no closed half-space properly contained in H contains C), since each boundary point of C lies on at least one *supporting hyperplane* (defined as the boundary hyperplane of a supporting half-space) (cf. Stoer, Witzgall 1970, p. 103).

This applies to $\mathcal{H}(f)$, too. Since f and hence $f^{\mathbf{R}}$ are non-descending (i.e., not descending in any direction of a unit vector $(\mathbf{0}^i, 1)$), the hyperplanes supporting $\mathcal{H}(f)$ are non-descending as well. And since all $(\mathbf{n}, f(\mathbf{n}))$ are integer-valued, the gradient or derivative of $f^{\mathbf{R}}$ at any point into any direction $(\mathbf{0}^i, 1)$ is integer-valued as well. That is, the hyperplanes supporting $\mathcal{H}(f)$ can be chosen to be of the form $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ $(\xi \in \Xi)$. Hence, $f^{\mathbf{R}}$ is the envelope of the supporting hyperplanes of that form, and since f is concave, i.e., since each $(\mathbf{n}, f(\mathbf{n}))$ is a boundary point of $\mathcal{H}(f)$, $f = \min\,(f_\xi + \rho(\lambda_\xi))$, the minimum taken over these supporting hyperplanes. So, κ is the mixture of Λ by some ρ (which must be proper according to 12.12). □

In a way, this is already our main result. Still, we are not finished at all. We have, later on, to discuss the philosophical content of (12.14). Before, however, we should inquire in more detail by which kind of mixtures concave ranking functions can be represented. For this we need a further piece of notation: For $\xi, \zeta \in \Xi$ let us write $\xi \leq \zeta$ iff for all $i \in I$ $\xi(i) \leq \zeta(i)$ and $\xi < \zeta$ iff $\xi \leq \zeta$ and for some $i \in I$ $\xi(i) < \zeta(i)$. Then, a noteworthy observation is

Theorem 12.15: If κ is the mixture of Λ by some proper ρ, it is a *finite* mixture of Λ by some ρ in the sense that $\rho(\lambda_\xi) < \infty$ only for finitely many $\xi \in \Xi$.

Proof: If κ is a mixture of Λ by some proper ρ and f its representative functions, then $f^{\mathbf{R}}$ is the envelope of the hyperplanes of the form $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ supporting $\mathcal{H}(f)$. Since f or $f^{\mathbf{R}}$ is concave, these hyperplanes become never steeper. More precisely, if $\rho(\lambda_\zeta) > \rho(\lambda_\xi)$, then not $\zeta \geq \xi$, i.e., $\zeta(i) < \xi(i)$ for some $i \in I$. However, any sequence $\xi_1, \xi_2, \ldots$ in Ξ such that not $\xi_r \geq \xi_s$ for all $r < s$ must be finite, because in each dimension $i \in I$ one can descend from ∞ to 0 only in finitely many steps. Therefore, $f^{\mathbf{R}}$ is the envelope of finitely many hyperplanes of the form $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ supporting $\mathcal{H}(f)$ and κ a finite mixture of Λ. □

In other words, theorem 12.15 states that for any f representing a regular symmetric concave ranking function κ the hypograph $\mathcal{H}(f)$ is a convex polyhedron.

There still remains the question whether we can say something more specific than (12.15). In particular, it would be nice to know whether the mixture generating a concave κ is unique in some sense, as de Finetti had been able to show in the probabilistic case. In that case any tiny weight made a difference. This is different in the ranking case. Here the impact of a mixture component λ_ξ may be so small, i.e., $\rho(\lambda_\xi)$ so large, that it does not change the mixture at all. This was clear from (12.15). If the impact of all but finitely many λ_ξ had not been chosen to be infinite, but just large enough, this would not have changed the resulting mixture. Hence, uniqueness of the mixing impact function ρ is never to be expected.

This non-uniqueness of the mixing impact function, i.e., this amount of insensitivity vis à vis the precise impacts is ultimately a consequence of the basic law 5.4(b) of disjunction for negative ranks. One may see here an advantage of the probabilistic side. The situation is ambiguous, though. One may as well praise this insensitivity, since it allows remote subjective laws (or hypotheses or possibilities in general) to have finite ranks, but no manifest influence on beliefs. We had occasion to observe this blessing in a slightly different guise already in section 7.2 after (7.15y).

Be this as it may, since we know that a finite mixture in the sense of (12.15) suffices to generate a concave κ, we may hope that there is a suitable sense of a minimal mixture uniquely generating κ. This is indeed the case. The geometric picture is quite simple. Take any concave representative function f and consider the convex polyhedron $\mathcal{H}(f)$ generated by f, which is the hypograph of the real spreading $f^{\mathbf{R}}$ of f. Such a polyhedron has *faces* that are defined as the intersection of the polyhedron with a supporting hyperplane (that intersects the polyhedron only at boundary points – see the definition in the proof of 12.14). These faces may have any dimension from 0-dimensional vertices up to I-dimensional faces spanning the entire supporting hyperplane (cf. Stoer, Witzgall 1970, pp. 38ff.).

Now we look for those supporting hyperplanes (= subjective laws with an impact factor) that intersect with $\mathcal{H}(f)$ at *maximal faces* or faces of maximal dimension in the sense that there is no supporting hyperplane intersecting with $\mathcal{H}(f)$ at a larger or higher-dimensional face. Those *maximally* supporting hyperplanes obviously are those minimally needed for generating $\mathcal{H}(f)$. We do not need more, since all the other supporting hyperplanes meet $\mathcal{H}(f)$ in lower-dimensional faces contained in the maximal faces. And we cannot do with less, since any relative interior point of such a maximal face (which, by definition, is not contained in a lower-dimensional face) is met only by such a maximally supporting hyperplane.

The following characterization of these maximally supportive hyperplanes will prove useful:

Theorem 12.16: $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ ($\xi \in \Xi$) is a hyperplane maximally supporting $\mathcal{H}(f)$ iff it is a supporting hyperplane and there is no $\zeta < \xi$ such that $f_\zeta^{\mathbf{R}} + \rho(\lambda_\xi)$ is a supporting hyperplane.

Proof: Let $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ be a supporting hyperplane. Thus there is an $\mathbf{n} \in \mathbf{N}^I$ with $f(\mathbf{n}) = f_\xi(\mathbf{n}) + \rho(\lambda_\xi)$. Suppose that for some $\zeta < \xi$ $f_\zeta^{\mathbf{R}} + \rho(\lambda_\xi)$ is also a supporting hyperplane. Then ζ can be chosen such that $\zeta(i) < \xi(i)$ and $\zeta(j) = \xi(j)$ for $j \neq i$. Hence, $f(\mathbf{n}^i, n_i + 1) < f_\xi(\mathbf{n}^i, n_i + 1) + \rho(\lambda_\xi)$. Since f is concave, ζ can be even so chosen that $\xi(i) - \zeta(i) = f_\xi(\mathbf{n}^i, n_i + 1) + \rho(\lambda_\xi) - f(\mathbf{n}^i, n_i + 1)$, i.e., that $f(\mathbf{n}^i, n_i + 1) = f_\zeta(\mathbf{n}^i, n_i + 1) + \rho(\lambda_\xi)$. This shows that $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ is not maximally supporting.

Conversely, suppose that $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ is not maximally supporting. Thus there is a supporting hyperplane $f_\zeta^{\mathbf{R}} + \rho(\lambda_\zeta)$ meeting $\mathcal{H}(f)$ at a higher-dimensional face. Let $i \in I$ be (one of) the additional dimension(s). Hence, there is in particular some $\mathbf{n} \in \mathbf{N}^I$ such that $f(\mathbf{n}) = f_\xi(\mathbf{n}) + \rho(\lambda_\xi)$, but $f(\mathbf{n}^i, n_i + 1) < f_\xi(\mathbf{n}^i, n_i + 1) + \rho(\lambda_\xi)$. Since f is concave, ζ can again be chosen as before; i.e., the supporting hyperplane meeting $\mathcal{H}(f)$ at a larger face can be chosen to be of the form $f_\zeta^{\mathbf{R}} + \rho(\lambda_\xi)$ with $\zeta < \xi$. □

Does this description of $\mathcal{H}(f)$ in terms of maximally supporting hyperplanes survive the coarsening to natural numbers and thus apply also to the original representative function f? Yes and no. There is a subtle distinction that does not show up as long as we move within Euclidean space:

Definition 12.17: For any $\xi \in \Xi$, λ_ξ is a *non-redundant component* of the mixture of Λ by ρ iff for some proposition $A \in \mathcal{A}$ $\min \{\lambda_\zeta(A) + \rho(\lambda_\zeta) \mid \zeta \in \Xi\} < \min \{\lambda_\zeta(A) + \rho(\lambda_\xi) \mid \zeta \in \Xi - \{\xi\}\}$; otherwise, it is a *redundant component*. Furthermore, λ_ξ is a *latently non-redundant component* of the mixture of Λ by ρ iff for some proposition $A \in \mathcal{A}$ $\min\{\lambda_\zeta(A) + \rho(\lambda_\zeta) \mid \text{not } \xi < \zeta \in \Xi\} < \min\{\lambda_\zeta(A) + \rho(\lambda_\zeta) \mid \text{not } \xi \leq \zeta \in \Xi\}$; otherwise, it is a *strongly redundant component*. Finally, the mixture of Λ by ρ is *minimal* iff for each strongly redundant component λ_ξ of that mixture $\rho(\lambda_\xi) = \infty$ and for each latently non-redundant component λ_ξ the mixture of Λ by ρ differs from the mixture of Λ by ρ' whenever $\rho'(\lambda_\xi) < \rho(\lambda_\xi)$ and $\rho'(\lambda_\zeta) = \rho(\lambda_\zeta)$ for $\zeta \neq \xi$.

Let me explain these definitions. λ_ξ is a non-redundant component of the mixture κ of Λ by ρ iff there is a proposition A for which λ_ξ *solely* determines the mixture minimum $\kappa(A)$. Hence, one should think that the required sense of a minimal mixture derives from this notion; all the redundant components should receive impact ∞.

However, this idea does not agree with the geometric picture developed before, for the following reason: For a concave representative function f, all the vertices or extreme points of the hypograph $\mathcal{H}(f)$ are points of the function f, i.e., of the form $(\mathbf{n}, f(\mathbf{n}))$. This is not to say, though, that each $(\mathbf{n}, f(\mathbf{n}))$ is a vertex of $\mathcal{H}(f)$; it may also be a relative interior point of a face of $\mathcal{H}(f)$. Now, at each vertex of $\mathcal{H}(f)$ several hyperplanes supporting $\mathcal{H}(f)$, even several maximally supporting ones, may intersect. Hence, it may also be that some maximally supporting hyperplane meets f only at vertices $(\mathbf{n}, f(\mathbf{n}))$ that are contained also in other maximally supporting hyperplanes. In that case this hyperplane (or the corresponding subjective law) would not be required for generating f – it would be a redundant component –, even though it would be required for generating $\mathcal{H}(f)$ or $f^{\mathbf{R}}$.

We better stick to the geometric picture, not only because it is mathematically more useful, but rather for the substantial reason that redundant components may become non-redundant through learning; this point will be explained below after (12.21). Therefore, definition 12.17 introduces the notion of latent non-redundancy. λ_ξ is a latently non-redundant component of the mixture κ of Λ by ρ iff at one point A it solely determines the mixture minimum $\kappa(A)$ taken only over those $\zeta \in \Xi$ such that not $\xi < \zeta$. Hence, as (12.17) makes clear, these latently non-redundant components λ_ξ precisely define the hyperplanes $f_\xi^{\mathbf{R}} + \rho(\lambda_\xi)$ maximally supporting $\mathcal{H}(f)$. Since the latter were those minimally needed for generating $\mathcal{H}(f)$, we have actually shown:

Theorem 12.18: For each regular symmetric concave ranking function κ for $\mathcal{A}$ there is a unique impact function ρ for Λ such that κ is the minimal mixture of Λ by ρ in the sense of (12.17).

This is as close as we can get to de Finetti's representation theorem in terms of ranking theory. It should have been clear all along that my account indeed is an ironic commentary to the basic theme of van Fraassen (1989). In no way I doubt the profound and increasing significance of the notion of symmetry as developed by van Fraassen in grand historic lines as well as in many detailed analyses. Also, I agree with the direction of van Fraassen's criticism of various current approaches to laws of nature (though I would not subscribe to all of his arguments). However, there is no need to bring the notion of symmetry into a principled opposition to that of a law. On the contrary, (12.14) and (12.18) tell us how tightly they are knit together: no laws without symmetry, and no symmetry (and concavity) without laws! The relation is as close in the deterministic case as de Finetti has shown it to be in the probabilistic case.

From the mathematical point of view our results seem perfectly satisfactory; the geometric notion of concavity was the one we needed. However, the philosophical significance is still wanting; we should know what concavity says in terms of inductive behavior or in terms of instantial relevance. Unfortunately, I can offer only a partial answer:

Theorem 12.19: A regular symmetric concave ranking function satisfies NNIR_c.

Proof: In terms of the representation function f NNIR_c says for any $\mathbf{n} \in \mathbf{N}^I$ and $l \in I$:

(a) $f(\mathbf{n}^l, n_l + 2) + \min_{i,j \neq l} f(\mathbf{n}^{i,j}, n_i + 1, n_j + 1) \leq 2 \cdot \min_{i \neq l} f(\mathbf{n}^{i,l}, n_i + 1, n_l + 1)$.

Suppose that the minimum on the RHS is realized by k. Thus, what we have to show is:

(b) $f(\mathbf{n}^l, n_l + 2) + \min_{i,j \neq l} f(\mathbf{n}^{i,j}, n_i + 1, n_j + 1) \leq 2 f(\mathbf{n}^{k,l}, n_k + 1, n_l + 1)$.

Now, either the minimum on the LHS of (b) is $f(\mathbf{n})$. Then $f(\mathbf{n}^{k,l}, n_k + 1, n_l + 1) = f(\mathbf{n}^l, n_l + 1)$ so that (b) reduces to

(c) $f(\mathbf{n}^l, n_l + 2) + f(\mathbf{n}) \leq 2 f(\mathbf{n}^l, n_l + 1)$.

This, however, is a direct application of concavity. Or the minimum on the LHS of (b) is $> f(\mathbf{n})$. Then $f(\mathbf{n}^l, n_l + 2) = f(\mathbf{n})$ and $f(\mathbf{n}^{k,l}, n_k + 1, n_l + 1) = f(\mathbf{n}^k, n_k + 1)$ so that (b) reduces to

(d) $f(\mathbf{n}) + \min_{i,j \neq l} f(\mathbf{n}^{i,j}, n_i + 1, n_j + 1) \leq 2f(\mathbf{n}^k, n_k + 1)$.

Since the minimum on the LHS is $\leq f(\mathbf{n}^k, n_k + 2)$, (d) further simplifies to (c) with l replaced by k, which again is entailed by concavity. □

(12.19) does not reverse, however; concavity is clearly stronger than NNIR_c. I doubt that there is an equivalent or a sufficient condition expressible in terms of instantial relevance; maybe there is one in terms of arbitrarily long sequences of observations being non-negatively relevant for arbitrarily long sequences of predictions. Thus, I admit that the philosophical justification for the concavity of ranking functions needs to be improved.

In a very special case, though, concavity reduces to NNIR_c:

Theorem 12.20: If $I = \{1, 2\}$, i.e., if only two properties F_1 and F_2 are considered, then NNIR_c entails concavity.

Proof: A two-dimensional representative function f is severely restricted in form. For any $\mathbf{n} = (n_1, n_2)$ minimitivity entails that $f(\mathbf{n}) = f(n_1 + 1, n_2) \leq f(n_1, n_2 + 1)$ (or the other way around, admitting the same reasoning). Let $\xi(1) = 0$ and $\xi(2) = f(n_1, n_2 + 1) - f(\mathbf{n})$. Consider then the plane $g = f_\xi^{\mathbf{R}} + f(\mathbf{n}) - f_\xi(\mathbf{n})$: It intersects with $\mathcal{H}(f)$, since $g(\mathbf{m}) = f(\mathbf{m})$ for all $\mathbf{m} = (n_1 + k, n_2)$ ($k \geq 0$) and $g(n_1, n_2 + 1) = f(n_1, n_2 + 1)$. However, it does not intersect with the interior of $\mathcal{H}(f)$, since NNIR_c, or inequality (c) in the proof of (12.19), together with minimitivity entails that $g(\mathbf{m}) \geq f(\mathbf{m})$ for all other $\mathbf{m}$. Hence, g is a plane supporting $\mathcal{H}(f)$. Since $g(\mathbf{n}) = f(\mathbf{n})$, also $f_\xi(\mathbf{n}) = f(\mathbf{n})$. Thus f is concave at $\mathbf{n}$. □

In Spohn (2005) I only considered the case $I = \{1, 2\}$. Hence, things looked deceptively simple there. In particular the notion of concavity did not yet surface; it is required only in the general case.

However, (12.20) finally helps us to see why positive instantial relevance could not be satisfied by symmetric ranking functions:

Proof of Theorem 12.9: For any $i \in I$ PIR_c states a condition on κ referring in effect only to the two properties F_i and non-F_i. Hence, it suffices to consider the thus coarsened frame. Within this frame, PIR_c trivially entails NNIR_c, NNIR_c entails concavity (by 12.20), and concavity entails that (the coarsened) κ is a finite mixture of laws (by 12.15). This means that from some $\mathbf{n}$ onwards (the coarsened) f defines a plane, i.e., is identical with some $f_\xi + \rho(\lambda_\xi)$. That is, given $E_\mathbf{n}$, κ is the same as the law λ_ξ and can no longer exhibit positive instantial relevance, but only instantial irrelevance like the law λ_ξ. Thus, PIR_c entails non-PIR_c. □

Our duplication of de Finetti's philosophy of probability is not yet completed; we have to inquire a final important issue. We know now that exactly the regular symmetric concave ranking functions are minimal mixtures of subjective laws. How does the mixture change, though, through evidence? What does evidence teach us about the subjective laws? These questions finally address the topic left obscure in section 12.4, the confirmation of laws.

The answer can be directly read off from the results reached so far. Suppose that we start with the regular symmetric concave ranking function κ that is the minimal mixture of Λ by some proper impact function ρ and has representation function f and that we now collect the evidence $E_\mathbf{n}$ about the first n instances. Thereby, we arrive at the a posteriori ranking function $\kappa_\mathbf{n}$. Suppose further, in order to make things simple, that we learn the evidence for sure so that $\kappa_\mathbf{n} = \kappa(. \mid E_\mathbf{n})$. Hence, $\kappa_\mathbf{n}$ is regular and symmetric, too, thus having representative function $f_\mathbf{n}$, and concave, thus being the unique minimal mixture of Λ by some posterior impact function $\rho_\mathbf{n}$. What can we say about $\rho_\mathbf{n}$ and its relation to ρ? This is a straightforward calculation:

f is generated by the mixture by ρ. Hence, $f(\mathbf{n}) = \min_{\xi \in \Xi} [f_\xi(\mathbf{m}) + \rho(\lambda_\xi)]$. Since $f_\mathbf{n}$ results from f by conditionalization, we simply have $f_\mathbf{n}(\mathbf{m}) = f(\mathbf{n} + \mathbf{m}) - f(\mathbf{n})$, for all $\mathbf{m} \in \mathbf{N}^I$. Therefore

$$f_\mathbf{n}(\mathbf{m}) = \min_{\xi \in \Xi} [f_\xi(\mathbf{n} + \mathbf{m}) + \rho(\lambda_\xi)] - f(\mathbf{n}) = \min_{\xi \in \Xi} [f_\xi(\mathbf{m}) + \rho(\lambda_\xi) + f_\xi(\mathbf{n}) - f(\mathbf{n})].$$

This suggests to define $\rho_\mathbf{n}$ by $\rho_\mathbf{n}(\lambda_\xi) = \rho(\lambda_\xi) + f_\xi(\mathbf{n}) - f(\mathbf{n})$; this seems how the impacts on the mixture get rearranged by evidence.

However, $\rho_\mathbf{n}$ has to be a minimal mixture, and this entails a modification. In the geometric representation it is clear which one. The hypograph $\mathcal{H}(f_\mathbf{n})$ is obtained from the hypograph $\mathcal{H}(f)$ simply by moving the origin of Euclidean space from $\mathbf{0}$ to $(\mathbf{n}, f(\mathbf{n}))$ and cutting off all parts of $\mathcal{H}(f)$ which fall outside the positive quadrant $\{\mathbf{x} \mid \mathbf{x} \geq \mathbf{0}\}$ by this translation of the origin. Thus, hyperplanes not maximally supporting $\mathcal{H}(f)$ (= strongly redundant components of the mixture of Λ by ρ) remain to be so with respect to $\mathcal{H}(f_{\mathbf{n}})$. Hyperplanes maximally supporting $\mathcal{H}(f)$ (= latently non-redundant components) remain to be so if and only if they intersect with $\mathcal{H}(f)$ at vertices not cut off after the translation of the origin, i.e., at vertices $(\mathbf{m}, f(\mathbf{m}))$ with $\mathbf{m} > \mathbf{n}$. This means that those redundant, but latently non-redundant components λ_ξ of the mixture of Λ by ρ which agree with f only for $\mathbf{m}$ not $> \mathbf{n}$, i.e., for which $f(\mathbf{m}) = f_\xi(\mathbf{m}) + \rho(\lambda_\xi)$ only if not $\mathbf{m} > \mathbf{n}$, become strongly redundant in the a posteriori mixture; these λ_ξ are too steep, as it were, to be still included. This consideration already proves

Theorem 12.21: Define for all $\xi \in \Xi$ $\rho_{\mathbf{n}}(\lambda_\xi) = \rho(\lambda_\xi) + f_\xi(\mathbf{n}) - f(\mathbf{n})$, if $f_\xi(\mathbf{m}) = f(\mathbf{m}) + \rho(\lambda_\xi)$ for some $\mathbf{m} > \mathbf{n}$ and $\rho_{\mathbf{n}}(\lambda_\xi) = \infty$ otherwise. Then $\kappa_{\mathbf{n}}$ is the minimal mixture of Λ by $\rho_{\mathbf{n}}$.

This consideration also explains why we had to distinguish non-redundant and latently non-redundant components of mixtures. According to definition 12.17 it would have sufficed to mix only non-redundant components in order to produce κ. However, an initially redundant component may become a posteriori non-redundant. This is the case when and only when the component is initially redundant, but latently non-redundant and some of the components making it initially redundant drop out from the posterior mixture as described in (12.21). Thus, if the initial mixture had contained only the non-redundant components, we might have been unable to define the posterior mixture from it. Only by including the redundant, but latently non-redundant components from the outset theorem 12.21 can work for all posterior $\kappa_{\mathbf{n}}$ ($\mathbf{n} \in \boldsymbol{N}^I$).

Let me point out a number of consequences of (12.21) and our representation theorems 12.14 and 12.18. The most important is perhaps that we can thereby fully restore positive instantial relevance with respect to the subjective laws instead of the next instances. Suppose we have already acquired evidence $E_{\mathbf{n}}$ and now observe the n+1st object to have F_i so that the evidence is $E_{\mathbf{n}'}$, where $\mathbf{n}' = (\mathbf{n}^i, n_i + 1)$. How do the mixture weights shift thereby? (12.21) immediately entails

Theorem 12.22: With the notation just introduced and $f_{\mathbf{n}}(i)$ being short for $f_{\mathbf{n}}(\mathbf{0}^i, 1)$, we have $\rho_{\mathbf{n}'}(\lambda_\xi) - \rho_{\mathbf{n}}(\lambda_\xi) = \xi(i) - f_{\mathbf{n}}(i)$, if $\lambda_\xi(\mathbf{m}) \leq f_{\mathbf{n}}(\mathbf{m})$ for some $\mathbf{m} \geq (\mathbf{0}^i, 1)$; otherwise $\rho_{\mathbf{n}'}(\lambda_\xi) - \rho_{\mathbf{n}}(\lambda_\xi) = \infty$.

What does this mean? If $\xi(i) > f_{\mathbf{n}}(i)$, i.e., if the n+1st object being F_i was more disbelieved according to λ_ξ than in $\kappa_{\mathbf{n}}$, then the impact of λ_ξ increases proportionally, i.e., λ_ξ gets proportionally more *dis*believed by the additional observation. If $\xi(i) = f_{\mathbf{n}}(i)$, i.e., if the observation was exactly as unexpected according to λ_ξ as according to $\kappa_{\mathbf{n}}$, the impact of λ_ξ does not change. And if $\xi(i) < f_{\mathbf{n}}(i)$, then λ_ξ gets proportionally confirmed, i.e., less disbelieved. This is how we intuitively expect the confirmation of laws to behave. In particular, if $f_{\mathbf{n}}(i) = 0$, i.e., if after evidence $E_{\mathbf{n}}$ the n+1st object was not excluded to be F_i, then the laws also not excluding this observation keep their impact while all the other laws get disconfirmed according to the strength of their expectation being disappointed. Again, this is as it should be.

The only exceptional case is when not only $\xi(i) > f_{\mathbf{n}}(i)$, but indeed $\lambda_\xi(\mathbf{m}) > f_{\mathbf{n}}(\mathbf{m})$ for all $\mathbf{m} \geq (\mathbf{0}^i, 1)$. Then the subjective law λ_ξ is definitely refuted, not because it has met counter-instances, but because the disbeliefs in all possible ways of realizing the properties F_i ($i \in I$) in future objects have become weaker than is representable by λ_ξ. This may seem to be an artificial consequence of my notion of a subjective law; it is, however, theoretically required. It should not distract, though, from our restitution of positive instantial relevance with respect to laws, i.e., on the level of second-order attitudes. This positive relevance is blurred by the mixture and thus weakens to NNIR_c on the level of first-order attitudes. (12.9) has shown us that the weakening is unavoidable, but now we can see that it is only an artifact of the mixture.

(12.22) also allows us to study the limiting behavior of $\rho_{\mathbf{n}}$, as $\mathbf{n}$ increases infinitely. The consideration of a normal case may suffice here. Normally, if n_i goes to infinity with $\mathbf{n}$, i.e., if the property F_i is instantiated infinitely often, $f_{\mathbf{n}}(i)$ will converge to 0, i.e., *be* 0 from some point onwards. (The contrary, i.e., maintaining the disbelief in instances of F_i even in the face of infinitely many instances of F_i would be weird.) In this normal case, $\rho_{\mathbf{n}}(\lambda_\xi)$ diverges to infinity as well, if and only if $\xi(i) > 0$. In other words, precisely those subjective laws that do not exclude the properties infinitely often instantiated do not drop out of the mixture. All the other ones get disbelieved with ever greater firmness diverging to infinity. This observation rounds off my ranking-theoretic translation of de Finetti's account of the confirmation of statistical hypotheses. It would be most desirable to compare the account presented here with formal learning theory as presented by Kelly (1996, 1999), since it is here where the two theories have the largest topical overlap.

A final consequence of our representation theorems still needs to be considered; it has no probabilistic counterpart and opens interesting perspectives. I discuss it in the last section of this chapter. Before, let me conclude this section with briefly recapitulating the effects of restricting this chapter to complete ranking functions.

As far as I see, completeness was not required for our basic representation theorem 12.14 and its supplement 12.18 about minimal mixtures. Also, the consequences for the confirmation of subjective laws (12.21 and 12.22) do not depend on it. However, we used completeness in section 12.3 (after 12.6) when identifying the (dis-)belief in the next instantiation with the (dis-)belief in the corresponding infinite generalization (but we noticed that this identification is no more objectionable than the identity with respect to finite generalizations that does not depend on completeness).

The most important use of completeness, of course, was in in theorem 12.15 about the finiteness of mixtures, where the crux of the argument was that, if ranks are well-ordered (as is required by completeness), descending sequences of ranks must be finite; this was also the crucial point of theorem 12.9 about the unsatisfiability of positive instantial relevance. If ranks are real-valued, this argument no longer obtains, and in such a frame positive instantial relevance may well make sense. Hence, it may appear now that theorem 12.9 was just a dramatic device in order to trigger the subsequent considerations in sections 12.4 + 5. Well, if it served this purpose, it cannot have been so bad a device. All the considerations thus motivated (with the exception of 12.15) continue to hold also within such a more general real-valued frame.

One might therefore think that it would have been better to base this chapter on real-valued ranking functions, all the more as PIR could have been saved thereby. I am not so sure. In any case, I have thought through all the philosophical applications of ranking theory in terms of complete ranking functions (out of habit, I suppose), and I feel that the interpretations developed in these applications are quite sensitive and would need more or less extensive readjustments within a real-valued frame. For instance, the close relation between absolute frequencies and subjective laws, which we had noticed after (12.10) and which appears philosophically significant to me, would be lost in the more general frame. Finally, one must never forget the fundamental reason for assuming completeness that I had explained after definition (5.5), namely that an infinite conjunction of truths is still true and an infinite disjunction of falsities still false.

12.6 A Priori Lawfulness?

I had just indicated a final, specifically ranking-theoretic, but philosophically significant consequence of the results of section 12.5. It concerns the special role of the subjective law $\lambda_{\mathbf{0}}$ where $\mathbf{0}(i) = 0$ for all $i \in I$ so that $\lambda_{\mathbf{0}}(E_{\mathbf{n}}) = 0$ for all $\mathbf{n} \in \mathbf{N}^I$. $\lambda_{\mathbf{0}}$ falls under our definition of a subjective law, but it cannot be said to express a strict law; it rather amounts to total agnosticism expressing belief in lawlessness instead of lawfulness. Is it also contained in the minimal mixture κ of Λ by some impact function ρ as represented by f? Yes, perhaps. It immediately follows from (12.18) that $\rho(\lambda_{\mathbf{0}}) = \sup f$. This may realize in three different ways. Let me explain what they mean.

First, f may increase indefinitely so that $\sup f = \infty$. Thus, also $\rho(\lambda_{\mathbf{0}}) = \infty$, and indeed $\rho_{\mathbf{n}}(\lambda_{\mathbf{0}}) = \infty$ for all $\mathbf{n} \in \mathbf{N}^I$. In this case, ρ embodies the maximally firm belief that some genuine subjective law (i.e., different from $\lambda_{\mathbf{0}}$) will obtain. This belief is invariable, not refutable by very long sequences of apparent random behavior of the instances distributing over all properties F_i ($i \in I$).

Second, $\sup f$ may be finite, but not vanishing so that $f(\mathbf{n}) = \sup f > 0$ for some $\mathbf{n}$ and $f(\mathbf{m}) = \sup f$ for all $\mathbf{m} \geq \mathbf{n}$. Then, lawlessness is initially disbelieved with impact $\rho(\lambda_{\mathbf{0}}) = \sup f$. Since $\rho_{\mathbf{n}}(\lambda_{\mathbf{0}}) = \sup f - f(\mathbf{n})$, it gets less disbelieved with each unexpected observation. After too many disappointments (that may be very many) κ will have eventually lost the belief in lawfulness and any belief about the behavior of the future instances, the belief in lawlessness being the only remaining option.

Third, there is the possibility that $\sup f = 0$. This, however, means that lawlessness is maximally firmly believed from the onset, since all other subjective laws are thereby rendered strongly redundant.

The third alternative is completely unreasonable; it leaves nothing to be believed and nothing to be learned. The first alternative is not so bad, but does not appear very reasonable, either. We might well despair of finding an acceptable law for the empirical realm considered, at least one statable with the given properties F_i ($i \in I$). So, the second alternative seems to be the most reasonable. However, it is not really attractive, either. It may always be criticized from both sides, for accepting lawlessness too early or too late.

Before reproaching ranking theory with this unsatisfactory alternative, we must recall, however, what I said in section 12.4 in comment to the apparent implausibility of the persistent attitude. One comment, leading to section 12.5, was that we never fully believe in a single law and thus never actually have the persistent attitude. The other comment can now be repeated. Our normal response is neither that we stubbornly insist in lawfulness with respect to the given properties F_i ($i \in I$), whatever the evidence (in case $\sup f = \infty$), nor that we simply acquiesce in lawlessness with respect to F_i ($i \in I$) initially or at some later point (in case $\sup f < \infty$). The normal response rather is to search for suitable enlargements of the given space of properties that allow to (re-)establish lawfulness in a more convincing way. Then the game starts all over again within that larger space of properties, till a further enrichment may seem required.

Another reasonable response to variegated evidence is to turn statistical and to take a probabilistic attitude. If deterministic laws do not seem maintainable, we may be content with inquiring statistical regularities. Then we treat the counter-instances to our conjectured deterministic laws not as apparent exceptions to be explained away, but as more or less rare events falling under some statistical distribution.

When exactly we switch to that probabilistic response is unclear. There is a grey area dividing ranking and probability theory, the deterministic and the probabilistic attitude, which, as I had emphasized at the end of section 10.3, needs to be cleared up in order to better understand the relation of probability and ranking theory. However, there is presumably no need to resolve which attitude to take. What we actually do is to promote and try to get the best of both of them; the attitudes pragmatically coexist.

Let me express our observation about λ_0, the belief in lawlessness, in more traditional terms. Kant tried to overcome Hume's objectivity skepticism generally with his transcendental logic and its synthetic principles a priori and Hume's inductive scepticism particularly with his a priori principle of causality. This principle ascertained rather only the rule- or law-guidedness of everything going on in nature and was thus as well called the principle of the uniformity of nature (cf. e.g., Salmon 1966, pp. 40ff.). As was often observed, this principle does not offer any constructive solution of the problem of induction, since it does not give any direction whatsoever concerning specific rules or laws or specific inductive inferences. Still, it provides, if a priori true, an abstract guarantee that our inductive efforts are not futile in principle. Is it a priori true?

Here, as well as on later occasions, we slip into the issue how to deal with the notion of apriority in our ranking theoretic framework. A systematic discussion of this issue will be taken up only much later in chapter 17. Preliminarily we can say at least this:

The notion of apriority is certainly one of the deepest philosophical notions provoking extensive controversy. It has emerged that this notion is at least ambiguous, although there is not much agreement about how exactly to describe the ambiguities. Still, one distinction that has gained prominence is that between unrevisable and defeasible apriority. A proposition is *unrevisably a priori* iff it is rationally to be believed independently of all experience, i.e., iff the belief in it is unrevisable and cannot be changed by any evidence whatsoever. This is certainly the notion which Kant used, though he associated it with further attributes, and which Quine attacked when attacking analyticity. By contrast, a proposition is *defeasibly a priori* iff it is rationally to be believed independently of experience in a different sense, namely without any evidence or prior to all experience. This is much weaker than unrevisable apriority; defeasibly a priori beliefs may well be revised on the basis of evidence.

These notions fit perfectly into our ranking theoretic scheme. Our initial ranking function κ, I have argued, is to be regular, symmetric, and concave (well, the latter was not fully argued). And whenever $\sup f > 0$, an extremely reasonable assumption, as we saw, this is tantamount to the disbelief in lawlessness, i.e., $\rho(\lambda_0) > 0$. In other words, the belief in lawlessness is at least defeasibly a priori.

If even $\sup f = \infty$, i.e., $\rho(\lambda_0) = \infty$, the belief in lawfulness is taken to be unrevisably a priori. This assumption appeared doubtful, at least with respect to any fixed (finite) set of properties. Still, it may be unrevisably a priori that there is *some* set of properties with respect to which lawfulness, i.e., the principle of the uniformity of nature holds. I am not prepared to argue about this issue. The foregoing considerations should, however, make this issue better amenable to clear argument.

The principle of the uniformity of nature is not the only explication of the principle of causality. Chapter 14 will allow us to state principles that are more obviously related to causation. Again, the issue of the apriority of such principles will come up, which, however, will be discussed only in chapter 17. This discussion will supersede my present indecision concerning the apriority of lawfulness.

Bibliography

Bacon, Francis (1620), *Novum Organum.*

Blackburn, Simon (1980), “Opinions an Chances”, in: D. H. Mellor (ed.), *Prospects for Pragmatism*, Cambridge: Cambridge University Press; also in: Blackburn (1993), pp. 75-93.

Blackburn, Simon (1993), *Essays in Quasi-Realism*, Oxford: Oxford University Press.

Carnap, Rudolf (1950, 1962), *The Logical Foundations of Probability*, Chicago: Chicago University Press, 2nd ed. 1962.

Carnap, Rudolf (1971b), “A Basic System of Inductive Logic, Part I”, in: Carnap, Jeffrey (1971), pp. 33-165.

Carnap, Rudolf, Richard C. Jeffrey (eds.), *Studies in Inductive Logic and Probability*, *Vol.* I, Berkeley: University of California Press.

Cartwright, Nancy (1983), *How the Laws of Physics Lie*, Oxford: Clarendon Press.

Cartwright, Nancy (1989), *Nature’s Capacities and Their Measurement*, Oxford: Clarendon Press.

Cohen, L. Jonathan (1970), *The Implications of Induction*, Methuen, London.

Cohen, L. Jonathan (1977), *The Probable and the Provable*, Oxford University Press, Oxford.

Cohen, L. Jonathan (1980), “Some Historical Remarks on the Baconian Conception of Probability”, *Journal of the History of Ideas* 41, 219-231.

Darwiche, Adnan., Judea Pearl (1997), “On the Logic of Iterated Belief Revision”, *Artificial Intelligence* 89, 1-29.

Dempster, A.P. (1967), “Upper and Lower Probabilities Induced by a Multivalued Mapping”, *Annals of Mathematical Statistics* 38, 325-339.

Dempster, A.P. (1968), “A Generalization of Bayesian Inference”, *Journal of the Royal Statistical Society, Series B,* 30, 205-247.

de Finetti, Bruno (1937), “La Prévision: Ses Lois Logiques, Ses Sources Subjectives”, *Annales de l’Institut Henri Poincaré* 7. Engl. translation: “Foresight: Its Logical Laws, Its Subjective Sources”, in: H.E. Kyburg jr., H.E. Smokler (eds.), *Studies in Subjective Probability*, New York: John Wiley & Sons 1964, pp. 93-158.

Dubois, Didier, Henri Prade (1988), *Possibility Theory: An Approach to Computerized Processing of Uncertainty*, Plenum Press, New York.

Gabbay, Dov M., et al. (eds.) (1994), *Handbook of Logic in Artificial Intelligence and Logic Programming, vol. 3, Nonmonotonic Reasoning and Uncertainty Reasoning*, Oxford University Press, Oxford.

Garber, Dan (1980), “Field and Jeffrey Conditionalization”, *Philosophy of Science* 47, 142-145.

Gärdenfors, Peter (1978), “Conditionals and Changes of Belief”, in: I. Niiniluoto, R. Tuomela (eds.), *The Logic and Epistemology of Scientific Change* (= *Acta Philosophica Fennica* 30), Amsterdam: North-Holland, pp. 381-404.

Gärdenfors, Peter (1981), “An Epistemic Approach to Conditionals”, *American Philosophical Quarterly* 18, 203-211.

Gärdenfors, Peter (1988), *Knowledge in Flux*, MIT Press, Cambridge, Mass.

Goldszmidt, Moises, Judea Pearl (1996), “Qualitative Probabilities for Default Reasoning, Belief Revision, and Causal Modeling”, *Artificial Intelligence* 84, 57-112.

Hacking, Ian (1975), *The Emergence of Probability*, Cambridge University Press, Cambridge.

Halpern, Joseph Y. (2003), *Reasoning About Uncertainty*, MIT Press, Cambridge, Mass.

Hempel, Carl G. (1945), “Studies in the Logic of Confirmation”, *Mind* 54, 1-26 + 97-120; reprinted in Hempel (1965, ch. 1).

Hintikka, Jaakko (1962), *Knowledge and Belief*, Cornell University Press, Ithaca, N.Y.

Hintikka, Jaakko (1966), “A Two-Dimensional Continuum of Inductive Methods”, in: J. Hintikka, P. Suppes (eds.), *Aspects of Inductive Logic*, Amsterdam: North-Holland, pp. 113-132.

Hintikka, Jaakko, Niiniluoto, Ilkka (1976), "An Axiomatic Foundation for the Logic of Inductive Generalization", in: M. Przelecki, K. Szaniawski, R. Wójcicki (eds.), *Formal Methods in the Methodology of Empirical Sciences*, Dordrecht: Reidel, pp. 57-81.

Humburg, Jürgen (1971), "The Principle of Instantial Relevance", in: Carnap, Jeffrey (1971), pp. 225-233.

Hume, David (1739), *A Treatise of Human Nature, Book I, Of the Understanding*.

Jeffrey, Richard C. (1965), *The Logic of Decision*, University of Chicago Press, Chicago, 2nd ed. 1983.

Jeffrey, Richard C. (1971), "Probability Measures and Integrals", in: Carnap, R. C. Jeffrey (1971), pp. 167-223.

Jeffrey Richard C. (ed.), *Studies in Inductive Logic and Probability, vol. II*, Berkeley: University of California Press

Joyce, James (1999) *The Foundations of Causal Decision Theory*, Cambridge University Press, Cambridge.

Kelly, Kevin T. (1996), *The Logic of Reliable Inquiry*, Oxford: Oxford University Press.

Kelly, Kevin T. (1999), "Iterated Belief Revision, Reliability, and Inductive Amnesia", *Erkenntnis* 50, 11-58.

Krantz, David H., R.D. Luce, P. Suppes, A. Tversky (1971), *Foundations of Measurement, Vol. I*, Academic Press, New York.

Krüger, Lorenz, et al. (1987), *The Probabilistic Revolution. Vol. 1: Ideas in History, Vol. 2: Ideas in the Sciences*, MIT Press, Cambridge, Mass.

Köhler, Eckehart (2004), "Physical Intuition as Inductive Support", in: F. Stadler (ed.), *Induction and Deduction in the Sciences*, Dordrecht: Kluwer, pp. 151-167.

Kuipers, Theo A.F. (1978), *Studies in Inductive Logic and Rational Expectation*, Dordrecht: Reidel.

Lange, Marc (2000), *Natural Laws in Scientific Practice*, Oxford: Oxford University Press.

Levi, Isaac (1967), *Gambling with Truth*, A. A. Knopf, New York.

Levi, Isaac (2005), *Mild Contraction: Evaluating Loss of Information Due to Loss of Belief*, Oxford University Press, Oxford.

Lewis, David K. (1973a), *Counterfactuals*, Oxford: Blackwell.

Lewis, David K. (1980), "A Subjectivist's Guide to Objective Chance", in: Jeffrey (1980), pp. 263-293.

Lewis, David K. (1986b), *Philosophical Papers, vol. II*, Oxford: Oxford University Press.

Lewis, David K. (1994), "Humean Supervenience Debugged", *Mind* 103, 473-490.

Loève, Michel (1978), *Probability Theory*, Heidelberg: Springer, 4th edition in two volumes.

Niiniluoto, Ilkka (1972), "Inductive Systematization: Definition and a Critical Survey", *Synthese* 25, 25-81.

Pearl, Judea (1988), *Probabilistic Reasoning in Intelligent Systems: Networks of Plausible Inference*, Morgan Kaufman, San Mateo, Ca.

Popper, Karl R. (1934), *Logik der Forschung*, 9th ed, Mohr, Tübingen 1989; engl. translation: *The Logic of Scientific Discovery*, Hutchinson, London 1959.

Ramsey, Frank Plumpton (1928), "Universals of Law and of Fact", in: F.P. Ramsey, (1978), pp. 128-132.

Ramsey, Frank Plumpton (1929), "General Propositions and Causality", in: F.P. Ramsey, (1978), pp. 133-151.

Rescher, Nicholas (1964), *Hypothetical Reasoning*, North-Holland, Amsterdam.

Rescher, NNicholas (1976), *Plausible Reasoning*, Van Gorcum, Assen.

Rockafellar, R. Tyrrell (1970), *Convext analysis*, Princeton: Prineton University Press.

Rott, Hans (1986), „Ifs, Though, and Because", *Erkenntnis* 25, 345-370.

Ryle, Gilbert (1949), *The Concept of Mind*, London: Hutchinson.

Sahlin, Nils-Eric (1991), "Obtained by a Reliable Process and Always Leading to Success", *Theoria* 57, 132-149.

Sahlin, Nils-Eric (1997), "'He Is No Good For My Work'. On the Philosophical Relations Between Ramsey and Wittgenstein", *Poznan Studies in the Philosophy of Science and the Humanities* 51, pp. 61-84.

Salmon, Wesley C. (1966), *The Foundations of Scientific Inference*, Pittsburgh: University Press.

Schlick, Moritz (1931), "Die Kausalität in der gegenwärtigen Physik", *Die Naturwissenschaften* 19, 145-162.

Shackle, George L. S. (1949), *Expectation in Economics*, Cambridge University Press, Cambridge.

Shackle, George L. S. (1969), *Decision, Order and Time in Human Affairs*, Cambridge University Press, Cambridge, 2nd ed.

Shafer, Glenn (1976), *A Mathematical Theory of Evidence*, Princeton University Press, Princeton.

Shafer, Glenn (1978), „Non-Additive Probabilities in the Work of Bernoulli and Lambert", *Archive for History of Exact Sciences* 19, 309-370.

Shenoy, Prakash P. (1991), „On Spohn's Rule for Revision of Beliefs", *International Journal of Approximate Reasoning* 5, 149-181.

Skyrms, Brian (1980), *Causal Necessity. A Pragmatic Investigation of the Necessity of Laws*, New Haven: Yale University Press.

Skyrms, Brian (1984), *Pragmatics and Empiricism*, Yale University Press.

Spohn, Wolfgang (1978), *Grundlagen der Entscheidungstheorie*, Kronberg/Ts.: Scriptor; out of print, pdf-version at:
http://www.uni-konstanz.de/FuF/Philo/Philosophie/Mitarbeiter/spohn.shtml

Spohn, Wolfgang (1983), *Eine Theorie der Kausalität*, unpublished Habilitationsschrift, Universität München, pdf-version at:
http://www.uni-konstanz.de/FuF/Philo/Philosophie/philosophie/files/habilitation.pdf

Spohn, Wolfgang (1988), "Ordinal Conditional Functions. A Dynamic Theory of Epistemic States", in: W.L. Harper, B. Skyrms (eds.), *Causation in Decision, Belief Change, and Statistics, vol. II*, Kluwer, Dordrecht, pp. 105-134.

Spohn, Wolfgang (1999), "Ranking Functions, AGM Style", in B. Hansson, S. Halldén, N.-E. Sahlin, W. Rabinowicz (eds.), *Internet-Festschrift for Peter Gärdenfors*, http://www.lucs.lu.se/spinning/

Spohn, Wolfgang (2001a), "Vier Begründungsbegriffe", in: T. Grundmann (ed.), *Erkenntnistheorie. Positionen zwischen Tradition und Gegenwart*, Mentis, Paderborn, pp. 33-52.

Spohn, Wolfgang (2005), "Enumerative Induction and Lawlikeness", *Philosophy of Science* 72, 164-187.

Stoer, Josef, Christoph Witzgall (1970), *Convexity and Optimization in Finite Dimensions I*, Berlin et al.: Springer.

Toulmin, Stephen (1953), *The Philosophy of Science*, London, Hutchinson.

van Fraassen, Bas C. (1989), *Laws and Symmetry*, Oxford: Oxford University Press.

Zadeh, L.A. (1975), "Fuzzy Logics and Approximate Reasoning", *Synthese* 30, 407-428.

Zadeh, L.A. (1978), "Fuzzy Sets as a Basis for a Theory of Possibility", *Fuzzy Sets and Systems* 1, 3-28.