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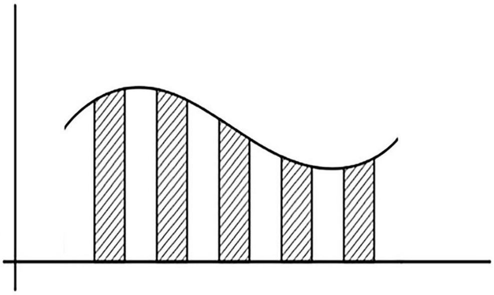

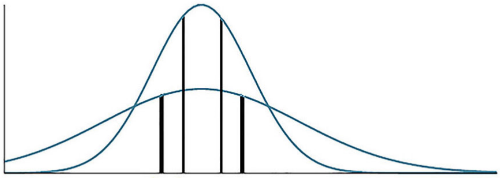

Fedde Benedictus

# Reichenbach: Probability & the A Priori

## Has the Baby Been Thrown Out with the Bathwater?

Faculteit Bètawetenschappen **FI**

# Reichenbach: Probability & the A Priori -
Has the Baby Been Thrown
Out with the Bathwater?

Fedde Benedictus

Benedictus, Fedde

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-Has the Baby Been Thrown out with the Bathwater?-

Reichenbach: Waarschijnlijkheid & het A Priori

-Is het Kind Weggegooid met het Badwater? -

(met een samenvatting in het Nederlands)

Reichenbach: Warskynlikheid & it A Priori

-de Poppe Fuortsmiten mei it Waskwetter? -

(mei in gearfetting yn it Frysk))

Proefschrift

ter verkrijging van de graad van doctor aan de Universiteit Utrecht op gezag van de rector magnificus, prof. dr. G. J. van der Zwaan, ingevolge het besluit van het college voor promoties in het openbaar te verdedigen op donderdag 13 juli 2017 des middags te 4.15 uur

door

Fedde Jan Benedictus

geboren op 21 november 1982

te Sneek

Promotor: Prof.dr. D.G.B.J. Dieks

Zum Philosophieren sind die zwei ersten Erfordernisse diese: erstlich, dass man den Mut habe, keine Frage auf dem Herzen zu behalten, und zweitens, dass man alles Das, was sich von selbst versteht, sich zum deutlichen Bewusstsein bringe, um es als Problem aufzufassen.

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*Arthur Schopenhauer*
*Parerga et Paralipomena*
*(zweiten Bande, par 4)*

# Acknowledgements

My parents have always been very supportive of my academic career. Being academically trained veterinarians themselves, they hold science (and, *ipso facto*, physics) in high esteem. A consequence of this pro-scientific stance is that for my parents the term ‘metaphysical’ has a connotation akin to that of ‘spiritual’. You can imagine their surprise when I told them that the topic of the first international conference I went to was metaphysics! In the years since, I have attempted to convince my parents that one can be sceptical of alternative medicine whilst taking metaphysics seriously. I would like to thank my parents for being an enormously stimulating example without being vicariously ambitious.

Without the never faltering and unrelenting supervision of Professor D. Dieks this dissertation would be poorly researched and full of unsubstantiated claims. Thanks Dennis.

I would also like to thank my wife, Fleur. In the past few years, she has become my anchor in the real world. Last, but perhaps most, I would like to thank my neurosurgeon Dr J Metzemaekers (UMCG, Groningen)—a stranger who saved my life three times.

# Preface

Three years before his decease in 2016 Hilary Putnam commented in an interview by Phillip McReynolds on the philosophical views of his *Doktorvater*[1] Hans Reichenbach. Putnam wanted to illustrate a recurrent phenomenon in the history of philosophy with the development of Reichenbach's philosophy. The phenomenon that Putnam wanted to illustrate is a philosophical version of 'throwing the baby out with the bathwater'.

What Putnam meant is the following. Often in the historical development of philosophy, when philosophical views succeed each other, older and out-of-fashion views are cast aside in their entirety. Valuable elements in outmoded views thus risk being thrown out together with the out-of-date views of which they are part. A result of this, according to Putnam, is that sometimes philosophical errors are repeated.

Putnam's illustration of such an error is the insight[2] of Kant that (in Putnam's words) "Not all physical statements follow on a par: some of them constitute the very lenses through which you see physical phenomena."[3] Putnam argues that the constitutive character of a certain category of physical statements might be one of the elements mentioned above; valuable elements within an outmoded philosophical view. The outmoded view in this case being kantianism. When the kantian bathwater was judged to have become stale, the idea that certain physical statements have a constitutive character was rejected together with the philosophical view of which it was part.

In his early work, which was published before the tide had turned against kantianism, Reichenbach's views stayed close to kantian (or neokantian) philosophy. Reichenbach's early work constitutes a search for the elements of

[1]'Doktorvater' is the German term for PhD Supervisor.

[2]There is not, nor has there ever been, consensus in the community of philosophers on whether the insight of Kant that is described by Putnam is one of the great triumphs of reason or that it is an unwanted vestige of rationalism.

[3]Putnam continues by saying "...and that's a kantian idea. Kant put it in the structure of the mind. After the linguistic turn we say certain things are the very structure of the language at a given time; they are like lenses through which you see the phenomena—they're partly constitutive of the phenomena [...] Reichenbach (in his 1920 book) asked 'was Kant on to something?' rather than saying that Kant is obsolete."

kantian philosophy that should be retained in the face of then-recent developments in theoretical physics (viz Einstein's relativity theories). Putnam argues that the result of this search—the insight that certain physical statements have a constitutive character—allows the early Reichenbach to answer a question that was again (and more famously) posed in a later generation by Thomas Kuhn: Reichenbach's early view shows how scientific progress is possible without introducing any element of irrationality.

In Reichenbach's later work, according to Putnam, the kantian element is no longer present. In his later work Reichenbach has become a logical positivist and his views no longer show traces of kantianism. These considerations lead Putnam to the conclusion that the development of Reichenbach's philosophy from a neokantian beginning to a logical positivist stance is a case of 'throwing the baby out with the bathwater'.

The careful reader will already have noticed that I have adopted Putnam's phrase in the title of this dissertation with a slight modification: I have added a question-mark at its end. It is my aim in this dissertation to show that the baby—which Putnam believes has been thrown out—is still there. I will show that it is at least tenable that the elements of kantian thought that graced Reichenbach's early work return in his later work in a logical positivist guise. We shall see that Reichenbach's ideas about probabilistic posits and his sophisticated realist stance may be argued to be traces of transcendentality.

# Contents

# Preliminaries

## Concepts, Questions and Prefatory Conclusions

Central to this dissertation is the role of the mathematical theory of probability in the scientific philosophy of Hans Reichenbach. The perspective from which we will investigate this role is both historical and philosophical. It is historical in that it pertains to historical episodes in science and its philosophy; it is philosophical in that conceptual developments are analysed from an internal philosophical (and at times mathematical) point of view. This means that sociological factors are not taken into account. Whence this unhistorical exclusion of the sociological context?

First and foremost, our exclusion of the sociological context is the result of a pragmatic choice. The scope of any dissertation must be bounded and this dissertation is restricted to the relation between probability and scientific philosophy outside of their sociological context. But the decision to exclude the external context from our description of conceptual developments, which was pragmatically motivated, has turned out to be justifiable in another way as well: our description of how probability and scientific philosophy developed, shows that a consistent story can be told without explicit reference to sociological factors. Of course, this relates to the fact that we ask questions about logical connections and consequences, and have left explicitly sociological questions (eg about the propagation and acceptance of probabilistic ideas) out of account. Consider, for example, the rejection of the principle that all events in the universe follow laws of cause and effect (the idea of *the principle of sufficient reason*). This principle was rejected by Reichenbach only after he had formulated his theory about a probability function, and can be seen as a logical consequence of this development. The probability function makes the principle of sufficient reason superfluous as a precondition of knowledge (in the Kantian sense), and we do not need sociological explanations[4] to understand

[4]Such as the 'Forman thesis', which attempts to explain the rejection of the principle of sufficient reason by sketching an anti-scientific and anti-deterministic intellectual atmosphere in Germany in the Interbellum ([Forman, 1971]). Note, however, that Forman's account can very well be seen as primarily addressing the issue of the social *acceptance* of indeterminism

the logic of Reichenbach's rejection of the principle of sufficient reason.

The starting point of our analysis is the tension between a mechanicist description of events, which is entirely deterministic, and a description of events in terms of probability. A question that naturally arises in this context is whether a description in terms of probability can be related to a description in terms of causes. Is there any use for the concept of probability in science if natural laws always describe which events follow which events? Arguably there is such a use, because determinism does not imply predictability, and it seems plausible that probability can be used to deal with uncertainty. But this leads us immediately to the further question of whether probability is descriptive of an objective characteristic of reality or whether it is merely a subjective affair.

In comparing the objectivity of different approaches to probability another central concept will come to the fore: the concept of the a priori. How does the a priori tie in with probability? We shall note that the term a priori has had different meanings, and we shall investigate some of them. Further, we shall see that disagreement about what is and what is not a priori sometimes will lead to different interpretations of probability.

The relation between Reichenbach's 1935 probability concept and Johannes von Kries' conception of probability is a case in point, which we will discuss. Summarising this discussion already now, we can say that underlying von Kries' approach is the assumption that all physical events are ruled by the principle of sufficient reason. Von Kries takes this principle to be a priori in an *empirical* sense (ie the principle is justified independently of observation). The belief that all events are connected through causal laws leads von Kries to the idea that the possible outcomes of a probabilistic experiment must be connected to specific classes of initial and boundary conditions. For example, the outcome 'six' when we cast a die corresponds to the class of all those initial (perhaps microscopic) situations in the experimental set-up that will result in 'six' by virtue of the dynamical laws. In this sense, the outcomes of a probabilistic experiment and their associated probabilities reflect an objective physical structure—the relations between the areas in phase space connected to the various outcomes. But there is also an unavoidable subjective element involved: because of the fact that we do not know the exact initial situation, we are unable to predict with certainty which outcome will be realised. Although von Kries bases his account of probability on physical structures and symmetries, he therefore is compelled to admit a subjective element having to do with our uncertainty about the exact situation.

On the other hand, as we shall see, Reichenbach from 1925 onwards no

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in Weimar Germany—as stated, we will not be concerned with questions of this kind.

longer considers the principle of sufficient reason to be a priori; instead, he considers the belief that the principle of sufficient reason is valid to be justified only insofar as the theories which are based on it are empirically verified. Instead of the kantian idea that strict causation is a necessary precondition of scientific knowledge, Reichenbach now introduces the weaker a priori of the existence of a probability function: it should be possible, in order to acquire scientific knowledge, to extrapolate from measured relative frequencies and to obtain in this way approximations of stable probabilities. The existence of these probabilities as limits of empirical frequencies must be assumed a priori—this is the principle of the existence of a probability function.

Reichenbach's 1935 interpretation of probability is therefore a relative frequency interpretation: degrees of probability are obtained as extrapolations of observed relative frequencies. Von Kries would not disagree that degrees of probability are related to observed relative frequencies and can be estimated using them. But he in addition believes that such relative frequencies must be the result of a pre-existing physical structure about which we do not know the details. As a result of this the status of von Kries' statements of probability differs from that of Reichenbach's statements. In Reichenbach's account from 1935 the statement that some event $E$ has probability $p$ just means that we have so far observed $E$ with relative frequency $p$ (eg within a sequence of results of some experiment) and that we may expect the relative frequency $p$ in the long term. Reichenbach does not connect this with the idea that it is necessarily possible to narrow down the uncertainty of the probabilistic predictions by a more precise determination of the initial conditions. For von Kries this is exactly opposite: for him, probabilities always involve a shortcoming of principle in the preciseness of our description—however hopeless it may be in practice to improve on the probabilistic treatment.

Of course, both in von Kries' and Reichenbach's account it is possible that after more experiments the relative frequencies that occur are different from the relative frequencies that resulted from our earlier observations. Suppose that such deviating relative frequencies are actually observed. Then Reichenbach would extrapolate the newly observed relative frequencies and update his assumptions about the form of the probability function accordingly. Von Kries would also regard the newly observed relative frequency as a better approximation of the probability, but for him this would be a reason to adapt his ideas about the underlying physical structure. As we see, the disagreement between Reichenbach and von Kries on the a priori status of the principle of sufficient reason has the consequence that Reichenbach's probability statements have a different status from those made by von Kries.

In this thesis we will further analyse the significance of the a priori for the notion of probability in Reichenbach's scientific philosophy. Our analysis will result in a primary finding and in a finding of a more peripheral nature. Firstly, our analysis will demonstrate that the delicate entwinement of apriority and probability in Reichenbach's work allows an element of his earlier neokantian philosophy to be preserved in his later work. More specifically, the application of Reichenbach's relativised a priori to his frequentist probability concept allows him, as already mentioned, to answer the (neo)kantian transcendental question how scientific knowledge is possible at all (to answer the transcendental question we need to assume that we can extrapolate observed relative frequencies of events to limiting relative frequencies; Reichenbach's frequentist probability concept requires limiting relative frequencies to exist—this is a relativised a priori). Reichenbach in his later writings comes to explicitly distance himself from Kant, but our analysis will show that Reichenbach's philosophy shows continuous traces of transcendentality.

Our second finding is of a different nature. In the course of our analysis we will have occasion to defend, to some extent, the claim that logical positivism is not the antiquated and out-of-date philosophical school that is presented by many modern textbooks on the topic. We shall argue our case by indicating that many issues that have been seen as problematic for logical positivism are addressed in Reichenbach's work[5]. At first sight it might seem as if this has but little to do with apriority and probability, but the relation between our finding and the issues that are central to this dissertation becomes clear when we realise that it is the traces of transcendentality in Reichenbach's work that make his logical positivist stance much more subtle than often assumed. The continuous traces of transcendentality in Reichenbach's philosophy connect to several themes typical of modern philosophy of science. For example, Reichenbach accords due significance to the problem of induction—which has been haunting philosophers ever since the days of Hume—and in this context elaborates his idea that certain very general probability statements should be regarded as instances of the relativised a priori; and his idea of a relativised a priori to some extent preshadows the Kuhnian notion of a paradigm.

We will begin the next chapter with a sketch of the historical context[6] of the emergence of the concept of probability.

[5]Reichenbach does not refer to his own philosophy as logical positivist, he prefers the label 'logical empiricist'. In this dissertation we will not distinguish between logical positivism and logical empiricism.

[6]Though not the sociological context.

# Part I

# Introduction

# Chapter 1

# Mechanicism & Probability: the Problem

**The contents of this chapter**

1. Probability & Mechanicism
2. Mechanicism Spurs Probability

**Figure 1.1:** René Descartes (1596-1650).

In our first chapter we aim to shed light on the earliest beginnings of what we now call probability theory[1]. We will defend the claim that it is at least plausible that the rise of mechanicist philosophy had a formative influence on the emergence of probability theory. Many of the explanations of unpredictable events (divine intervention, fate or blind chance as explanations of the way the dice roll) that were available for the philosopher before the rise of mechanicist philosophy, were no longer considered as unproblematic explanations during and after the Scientific Revolution[2]. The problematisation of the explanations used before that time stimulated philosophers to look for alternative explanations of unpredictable events—it stimulated thinking about probability. In the next sections we will first analyse the ideas of mechanicism and probability and then argue that ideas about mechanicism spur ideas about the concept of probability.

## Probability & Mechanicism

Ideas about probability date back at least as far as the time of Aristotle. The concept of probability has had many different connotations (cf. [Hacking, 2003]). The connotation that we are concerned with in this dissertation is the concept of probability that is used to describe events about whose occurrence we are not certain. The more specific type of probability that we are interested in in the current chapter distinguishes itself from earlier ideas about probability in a clear way: theories of probability as understood in this chapter not only assign degrees of probability to the occurrence of events, but they also assign numerical values to degrees of probability. The theories of probability that we are interested in involve a *calculus* of probability.

Tradition has it that the history of probability theories has an unusually specific starting point in the year 1654; the year in which the Parisian mathematician Blaise Pascal received a letter from his Toulousain colleague Pierre de Fermat to discuss a problem regarding the proper division of prize-money between two players of a gambling game. [David, 1990] convincingly argues that probabilistic mathematics started out as being regarded as a pastime for

[1] Comprehensive sources on the history of probability theory that we have made use of are, among others, [Hacking, 2003] and [Daston, 1995].

[2] It might be objected that divine intervention plays an essential role in Newton's philosophy (for the stability of the solar system and perhaps also to explain why there exists a universal, nonlocal, law of gravitation). We note that Newton did not use divine intervention as an explanation, but rather as a description ("hypotheses non fingo", he wrote). Moreover, Newton was criticised for his unexplained nonlocal universal gravity in the 18$^{\text{th}}$ century. This criticism is indicative of a discontentedness with the use of divine intervention as a scientific explanation. The same point can be made with regard to fate and blind chance; we do not say that these concepts were completely abandoned during the Scientific Revolution, but their use was problematised.

mathematicians of which the Pascal/Fermat correspondence is an illustration. Probability theory worked well in making predictions in games of chance, but it was not accorded the same significance as other branches of mathematics.

At the risk of oversimplifying things, ‘mechanicism’ can be defined as *the philosophy that searches for a description of nature in terms of the functioning of a causal mechanism*[3]. Mechanicist philosophy is usually regarded as having its roots in the writings of René Descartes (1596-1650). In Descartes’ philosophy everything in the visible universe is guided by a *causal principle*. According to this principle every cause must be as real as its effect, which implies that nothing can come from nothing—every effect must be caused[4].

The task that Descartes set himself is to discover the causal mechanism behind the effects that can be observed in nature ([Descartes, 1644], p.249). At several points in his writings Descartes uses the workings of a clock as a metaphor to illustrate how his mechanistic universe functions ([Shapin, 1998], p.32 ff.). Descartes’ implicit argument is as follows. Even though we cannot see the constituents of a clock we believe that the clock’s outward behaviour follows from its constituents obeying laws of cause and effect. The mechanicist philosopher extrapolates this inference to all motions in nature. It follows that if we see something moving in nature then that movement, too, must have a cause. With such comparisons Descartes attempts to convince his readers of the idea that *the whole of nature* follows the same mechanical laws.

An idea that is closely related to this is the idea of determinism. According to this idea the world is deterministic if and only if, given a specified way things are at a time $t$, the way things go thereafter is fixed as a matter of the mechanicist’s laws ([Hoefer, 2016]). It is not a great leap from mechanicism to determinism, and it should therefore not surprise us that the traditional reference for the idea of determinism is the work of that champion of mechanicism, P.S. Laplace. We will investigate the words of Laplace on the matters of determinism and mechanicism in a later chapter (ch.3).

Descartes wrote extensively about science and the methods that are involved in it[5]. In 1644 Descartes’ *Principia Philosophiae* (Principles of Philosophy) was published. In this work, originally intended as a textbook, Descartes presented a comprehensive overview of his mechanicist philosophy, in which the notion of a material mechanism (collisions between particles) is central. The approach of Descartes underwent a significant modification in the work of Newton: the modern notion of mechanicism was developed in Newton’s

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[3] Cf. [Dijksterhuis, 1950].

[4] This principle has become known as the *causal adequacy principle*. The *locus classicus* for this principle is p.58 of Descartes’ *Meditations on First Philosophy* (1641).

[5] In the parlance of his time Descartes would not have styled himself a scientist but a natural philosopher.

Principia. Here it is no longer the notion of a material mechanism that is primordial, but rather the more abstract structure of deterministic mechanical equations (usually differential equations). The elaborations, emendations and generalisations of Descartes' work in the work of the natural philosophers of the Scientific Revolution (such as Boyle and Newton) set a new standard. Mechanicist philosophy became prevalent (and a revolutionary novelty) in the 1650's — the time of the Pascal/Fermat correspondence.

## Mechanicism Spurs Probability

In this section we want to make the case that the philosophy of mechanicism and the concurrent mathematisation of natural philosophy have had a formative influence on the development of probability theory. We distinguish the two following claims:

- (M) The emergence of probability theory and the rise of a mathematised mechanicism occurred at the same time.
- (T) Mechanicism has played a stimulating role in thinking about how probability should be interpreted.

The first of these claims (M) can be found in the literature. Together with T it goes a long way towards explaining why the formal theory of probability emerged when it did. Together the two claims suggest that the rise of the philosophy of mechanicism in the 17$^{\text{th}}$ century sowed the seeds for the 'classical' theory of probability.

Although it is impossible to prove beyond doubt that a connection between mechanicism and probability exists, it is our aim in this section to make at least plausible the view that mechanicist thought was conducive to a change in probabilistic thinking.

(M) The first claim is unproblematic and well supported by the literature. Although the theory of probability's traditional starting point in the year 1654 should be taken with a grain of salt, the writings of Graunt, Huygens and Leibniz on the topic show that [Hacking, 2003] is right when he states that in the 1650's the mathematics of probability was "in the air" (p.11). That this period was also the time of mechanicism and mathematisation in physics is described in detail by, *inter alia*, [Dijksterhuis, 1950] and [Dear, 2009]. Primary sources can be found in the works written by the giants of the scientific revolution such as Descartes and Newton.

There may seem to be exceptions to the contemporariness of mechanicism and modern probability theory, for example Galileo's essay *Concerning the*

*Investigations on dice*, published in 1612, and other even older work on probabilities (eg by Cardano). However, we are not concerned with the use of probabilities (or something similar to probabilities) per se, but with the emergence of a theory of probability that goes beyond intuition and elementary counting procedures. Seen from this perspective it seems that the historical records give us no reason to doubt M.

(T) Mechanicism problematises, and thereby stimulates, probabilistic thinking. To see this, suppose that a natural philosopher with an inclination towards Descartes' mechanicist thinking considers a physical situation she believes can develop in a number of ways. In our example the philosopher considers a toss with a coin[6]; how can she account for the fact that there can be different outcomes? How does she account for *potential* outcomes of the toss? In a pre-mechanicist worldview it would not be difficult to account for the fact that different outcomes are possible. In such a worldview there are many kinds of explanations available for the fact that the toss has one rather than another outcome. The outcome might be attributed to the (free) will of the tosser, to divine intervention, or may be considered blind chance (implying that there is no link whatsoever between the toss and its outcome).

For an observer with a mechanicist mindset the aforementioned methods of explanation are problematic. In the mechanicist view of the world, divinity and man's intentions play only a role on the sidelines and blind chance is not an explanation at all[7]. We stated above that the philosophy of mechanicism is closely related to that of determinism. It is therefore a pressing matter for our mechanicist philosopher to explain the relation between determinism and the occurrence of unpredictable events. She must either 1) describe how the occurrence of unpredictable events can be reconciled with determinism; or, 2) show how a violation of determinism is to be reconciled with mechanicism. Both of these options involve considerations about predictability and therefore about the concept of probability.

Mechanicism renders pre-mechanicist explanations of probability problematic, forcing the mechanicist philosopher to re-interpret the concept of probability. We see that mechanicism indeed stimulates thinking about the interpretation of probability—$T$ holds true[8].

[6]But our argument also holds for a philosopher considering tomorrow's weather or some other physical situation with an element of chance in it.

[7]This is illustrated by Descartes' *deistic* view of the universe.

[8]One might wonder: if mechanicism is conducive to a change in probabilistic thinking, then why did not Descartes himself (or Newton or Boyle) come up with a calculus of probability? It is of course impossible to give anything but a very speculative answer to this question, but we might surmise the following. A mechanicist probabilistic calculus requires a recognition of cases which are equally possible. In all situations other than games of chance such cases are not easily recognisable. Perhaps that is why mathematicians analysing games

Now that we have made plausible the idea that there exists a tension between thinking in terms of causes and thinking in terms of probability we shall investigate in the next section the notion of apriority.

of chance were the first to come up with the probability calculus. Furthermore, it was the aim of the natural philosopher to describe how things actually are, not how things could be or could have been.

# Chapter 2

# Apriority

**The contents of this chapter**

1. Aristotelian A Priori
2. Kant's Transcendental Idealism
3. Analytic/Synthetic
4. A Priori/A Posteriori
5. Synthetic A Priori
6. Noneuclidean Geometry
7. Reichenbach's Relativised A Priori

## Aristotelian A Priori

We begin our second chapter with an analysis of the metamorphoses of the concept of the a priori. The characterisation of the term a priori as a form of justification that is independent of observation has only been current since the time of Kant. Before that time a priori was used in a proof-theoretical sense to denote proofs that proceed from causes to their effects. Such use of the term a priori has its origin in the work of Aristotle[1]. To illustrate the aristotelian characterisation of the a priori we look at a syllogism of a type that Aristotle might have used:

[1] Cf. [Mittelstrass, 1977] for an account of how the meaning of the term a priori has changed.

Everything in the heavens is inclined towards perfection.
All perfect things move in circles.

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Planets (being celestial objects) are inclined towards circular motion $\therefore$

The aristotelian a priori proof of the above conclusion rests on the assumption that the first premiss is true. Because the second premiss is true by definition (according to Aristotle, that is), the conclusion necessarily follows. Aristotle would call such a line of reasoning a priori because it runs from what he believed to be a cause (the inclination of heavenly objects) to the effect of that cause (the planetary inclination towards circular motion).

An a posteriori proof in the aristotelian sense would go in the opposite direction. It would start with the premiss that planets move in circles and from that infer the first premiss of the above syllogism. The a posteriori status of the inference in this example does not depend on the origin of the premiss about planets moving in circles. This premiss may be derived from observations of planets, but it could also have come from, for example, intuition. The aristotelian distinction between a priori and a posteriori arguments is therefore independent of whether the premisses and conclusion come from experience or not.

This conception changed with the development of Kant's philosophical ideas. The term a priori as used by Kant carries the meaning of being prior to observation. More precisely, Kant's a priori refers to statements that can be justified independently of observation. We will investigate the role of the a priori in Kant's philosophy—which we will term the empirical a priori.

## Kant's Transcendental Idealism

Central to Kant's epistemology is his *Transcendental Idealism*. The interpretations of Kant's writings on this are many[2], but they all agree that Kant makes a distinction between on the one hand the world as it exists independently of us, about which it is impossible to have any knowledge (Kant calls this "die Dinge an sich"; the things-in-themselves); and on the other hand the world as it appears to us in our observations (Kant's "die Dinge für uns"; the things-for-us). Having defined this distinction, Kant asks the *transcendental question*: how must the world as it appears to us in our observations (the things-for-us) be structured so as to make our knowledge of that world possible? What characteristics, Kant wonders, must the world have to make it intelligible?

[2]For a concise overview of the two major ways to interpret Kant's Transcendental Idealism, cf. [Rohlf, 2010].

## Analytic/Synthetic

In his attempt to answer this question, Kant analyses the human understanding ("Vernunft"). He divides all judgements that we can make into two classes: analytic judgements and synthetic judgements. According to Kant, an analytic judgement is a judgement whose predicate concept is contained in its subject concept. An example of such a judgement is 'all bachelors are unmarried'. The statement expressed in this judgement is true by virtue of the meanings of the terms it involves.

A synthetic judgement, on the other hand, is a judgement whose predicate concept is not contained in its subject concept: it is a *synthesis* of knowledge about the concepts the judgement is about and information about the external, physical world. A simple example of such a synthetic judgement is 'the apple is red'.

Kant's analytic/synthetic distinction is to a certain extent comparable to the analytic/factual distinction that Hume coined. The similarity is easily stated: Kant's analytic judgements can be expressed as analytic statements. The comparison becomes more difficult, however, when we attempt to relate synthetic judgements to factual statements. Both synthetic judgements and factual statements entail a claim about the physical world, but Kant's synthetic judgement differs from a merely factual claim: sometimes it expresses an empirically a priori constitutive relation between the observer's cognitive faculty and that which is observed. The idea behind this formative relation will be explained below.

## A Priori/A Posteriori

Besides the distinction between analytic and synthetic judgements Kant also distinguishes between judgements a priori and judgements a posteriori. Kant uses the term a priori here not in the sense in which Aristotle used it: it does not necessarily pertain to any cause/effect-relation. Rather, a judgement is a priori in Kant's understanding of the term if it can be justified independently of any observation. The term a posteriori pertains to judgements whose justification necessarily involves empirical results—a posteriori judgements have factual content.

Now that we have described these two distinctions (analytic/synthetic and a priori/a posteriori) we may investigate the relation between these two distinctions. From our characterisation of the terms it seems to follow naturally that the set of analytic judgements corresponds to the set of judgements that are a priori and that synthetic judgements are a posteriori, because for the justification of an analytic judgement we need only analyse the concepts used—an analysis which can be made before any observation is done. A synthetic judgement, on the other hand, can in general only be justified after observations have been made.

## Synthetic A Priori

We can now understand Kant's answer to the transcendental question. His revolutionary proposal was that besides the two types of judgements that we just discussed (the analytic a priori and the synthetic a posteriori) there is yet a third kind of judgement: the *synthetic a priori*. Synthetic a priori judgements say something about the world as it appears to us in our senses, yet their truth lies beyond observation. The apparent fact that we can have knowledge of the world around us implies that the (observable) world must be such that our knowledge of it is possible. There are synthetic a priori elements in our knowledge responsible for this structure of the observable world.

The synthetic a priori elements in our knowledge correspond to certain structural features of reality that make human knowledge possible. The examples of such synthetic a priori elements that Kant gives are the validity of the concept of substance, of space and time, and of a universally valid causal principle. Knowledge is only possible in a world where material 'things' (substance) exist in space and in time and events involving them are categorisable into causes and effects. Kant had in mind here the Newtonian worldview in which the notions of absolute space and time act as a sort of four-dimensional container in which material bodies can interact following Newton's laws of motion.

Kant not only held that there are synthetic a priori elements in our knowledge, he also believed that these elements are unchangeable because they always remain necessary for the very possibility of knowledge.

## Noneuclidean Geometry

This unchangeability of Kant's synthetic a priori became problematic for (neo)kantian philosophy when in the $19^{\text{th}}$ century noneuclidean geometries were discovered. In Kant's system, euclidean geometry had received a synthetic a priori status. Because euclidean geometry is an essential ingredient in human knowledge, we must assume a priori that euclidean geometry is applicable to the world we observe—euclidean geometry must be synthetic a priori.

The synthetic a priori nature of euclidean geometry became controversial when around 1830 the Hungarian mathematician János Bolyai and the Russian mathematician Nikolai Ivanovich Lobachevsky both discovered a solution to a problem that had plagued mathematicians for more than two millennia. This problem was that of Euclid's infamous parallel-postulate.

Around 300 B.C. the Greek mathematician Euclid of Alexandria wrote the 'Elements', in which he proved numerous geometrical propositions starting from 23 definitions and five postulates. It is the fifth of these postulates that is very important for our story. The essence of Euclid's fifth postulate is that through any point that lies outside a given straight line only one straight line can be drawn that does not cross the first straight line (in other words: any straight line has only one parallel through any point not on the line itself).

Euclid's original formulation of his fifth postulate is rather verbose as compared to his other postulates. This verbosity is perhaps the reason that made many a mathematician after Euclid wonder whether it be possible to reduce this postulate to the other postulates.

More than 2000 years later Lobachevski and Bolyai separately published treatises with the revolutionary insight that the negation of the axiom of the parallels does not lead to inconsistencies, but rather gives rise to a family of geometries that are different from Euclidean geometry. These novel geometries were called noneuclidean geometries. The discovery of Lobachevski and Bolyai showed that the millennia-old assumption that the parallel-postulate applies to the space we inhabit is not necessarily true.

The discovery of alternative geometries was a severe blow to kantianism. Although the possibility of noneuclidean geometries does not invalidate the very idea that there are synthetic a priori elements in our knowledge, it does prove that it is not a necessary truth that euclidean geometry is one of those synthetic a priori elements.

The arguments against Kant's ideas gained extra force when, early in the $20^{\text{th}}$ century, Einstein published his relativity theory. In the theory of general relativity the space-time we inhabit is described as a differentiable manifold. The geometry of this manifold, relativity theory shows us, may not be assumed to be euclidean.

Before the advent of the theory of general relativity the apodictic character of euclidean geometry had already been denied by Poincaré, among others[3], and the second quarter of the $20^{\text{th}}$ century became a still more hostile environment for kantianism. Moritz Schlick, one of the leading members of the Vienna Circle, categorically denied the possibility of the synthetic a priori in his 'Is there a factual a priori?' [Schlick, 1949], and in the generation following Schlick it seems to have been generally accepted that only analytic statements can be a priori[4].

[3] [Poincaré, 1905], p50; Poincareé saw the axioms of Euclidean geometry not as necessary preconditions for our knowledge of space, but rather as conventions that had a preferred status because of their simplicity.

[4] Carnap and Russell are traditionally associated with this view. Later proponents are

## Reichenbach's Relativised A Priori

The critical attitude towards Kantianism was not shared by all early $20^{\text{th}}$ century philosophers of science. We want to focus here on the early work of Reichenbach. Reichenbach's 1916 dissertation was wholly neokantian in spirit. In the work Reichenbach defends a frequentist view on probability in which he makes use of transcendental argumentation. In 1920, in his habilitation thesis ("The Theory of Relativity and A Priori Knowledge"), Reichenbach attempts to reconcile kantian philosophy (or at least the kantian idea of a transcendental deduction) with the then-recent theory of general relativity.

Reichenbach's analysis of the relation between kantian philosophy and relativity theory may be regarded as a salvage operation. Reichenbach wanted to pinpoint the element in the kantian transcendental deduction that is responsible for the contradiction between Kantian transcendentalism and relativity theory.

Reichenbach considers it to be an error of Kant to assume that there are elements in our knowledge which are synthetic a priori in a strictly empirical sense. We should not, Reichenbach argues, assume that there is some "preestablished harmony"([Reichenbach, 1920], p.60) between our scientific knowledge and the reality it describes. Reichenbach's analysis of kantian transcendentalism leads him to recognise two different aspects of Kant's synthetic a priori. On the one hand the synthetic a priori could be said to possess a formative quality, in that it has a formative influence on knowledge. It is constitutive of concepts because objects in our knowledge/experience cannot be thought about without the synthetic a priori elements being in place. The synthetic a priori elements are therefore necessary for the possibility of knowledge. Secondly, Kant's synthetic a priori has an apodictic quality; it is immutable. As long as there is knowledge, those elements that make possible this knowledge must exist. Therefore, Kant's synthetic a priori cannot change.

What the development of relativistic physics shows us, Reichenbach argues, is not that the kantian approach *in toto* is flawed. Rather, Einstein's theories show us that the synthetic a priori does not have an apodictic quality—the a priori elements should not be thought of as immutable.

Reichenbach replaces Kant's idea of the immutable synthetic a priori with a kind of flexible a priori—something that has later become known as the relativised a priori [Friedman, 2009]. The a priori of Reichenbach has the constitutive quality that Kant believed his synthetic a priori had, but it is no longer apodictic. As an example of this we may consider the a priori status of euclidean geometry. Reichenbach believes that euclidean geometry does not

A.J. Ayer and C.G. Hempel (see, for example, [Ayer, 1936] and [Hempel, 1945]).

have a synthetic a priori status in Kant's sense of the term as relativity theory shows us that euclidean geometry is not an essential ingredient in knowledge (at least not in *scientific* knowledge). However, a more general concept of geometry (geometry *per se*) is still necessary to be able to say something about how objects behave. We cannot say, however, whether physics will not progress to a point where even this more general geometry is proved to be non-essential (perhaps scientific knowledge can do without geometry and be based on a form of nongeometric topology). Reichenbach states this quite clearly: "there are no 'most general' concepts". ([Reichenbach, 1920], p.80)

An important result of Reichenbach's analysis is that it unveils the conditional quality of certain propositions that the newtonian physicist in Kant's time would have regarded as self-evident. Reichenbach considers the example of the vectorial addition of forces; "the theorem of the parallelogram of forces" ([Reichenbach, 1920], p.55). This theorem is evidently true, Reichenbach argues, *if* the forces in the theorem are indeed vectors. The theorem's truth is therefore *conditional*: whether its application is justified depends on the vectorial nature of the objects it is applied to. What Reichenbach calls relativised a priori here is not the parallelogram theorem itself (that vectors are added via a parallelogram is an analytic truth). Rather, the relativised a priori element is that the observed forces behave as vectors. The application of the theorem to our observations presupposes that the observed forces behave as vectors.

Reichenbach's flexible a priori is not a necessary element in knowledge *per se*, but in knowledge within a specific theoretical context. This implies that Reichenbach's a priori is not a priori in an empirical sense (ie it does not pertain to statements whose justification can be done independent of observation). Reichenbach's a priori is characteristic of statements whose justification is that they are part of a preestablished scientific context.

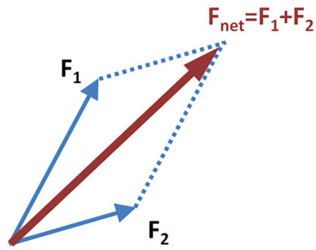


**Figure 2.1:** The parallelogram of forces is a theorem that tells us how two forces should be added. (image taken from public domain)

Kant's synthetic a priori is factual. Reichenbach's more flexible a priori depends on theoretical frameworks and is therefore only factual insofar as the relevant scientific theories can be considered factual.

Reichenbach's ideas about the role of the a priori in science have been analysed over the past decades by Michael Friedman. Friedman describes Kant's view on science as "antecedently presupposing a framework" inside which empirical laws can be formulated ([Friedman, 1999], p59 ff). It is this framework which Kant calls synthetic a priori in the sense we described above. The logical positivists, according to Friedman, took over the idea of the presupposed framework with a twist: the logical positivists did not side with Kant in believing the framework to be unchangeable. [Friedman, 1999] discusses in this context the approaches of Schlick, Carnap and Reichenbach. In this section we have discussed Reichenbach's idea of flexibising Kant's synthetic a priori.

Friedman states that, after a discussion with Schlick, Reichenbach dropped a crucial element in his approach: the idea that the flexibilised a priori is specific to a certain theoretical framework. We will discuss the difference between the approaches of Reichenbach and Schlick in a later chapter, but we may already state the result of our analysis. We dispute Friedman's claim that the context-specificity of Reichenbach's relativised a priori becomes lost. This conclusion partly overlaps with that of [Dieks, 2010], who argues that the 'conventions' with which Reichenbach (on Schlick's behest) replaces his relativised a priori bears a trace—context-specificity—of his earlier approach. This can be seen, according to Dieks, in [Reichenbach, 1928]. We go beyond [Dieks, 2010] in stating that the context-specificity can also be found in Reichenbach's 1935/49 work. We will see in part II of this dissertation that context-specificity plays an important role justifying (or vindicating) certain elements of Reichenbach's frequentist interpretation of probability.

[Stump, 2011] has compared Friedman's take on Reichenbach's relativised a priori with the *functional* a priori of Arthur Pap[5], about which Pap wrote no more than 25 years after the publication of Reichenbach's habilitation thesis. Stump writes that "Arthur Pap was not quite a Logical Empiricist" and that "Pap diverged most strongly from Logical Empiricism in his theory of a 'functional a priori' in which fundamental principles of science are hardened into definitions and act as criteria for further inquiry." If Stump's reading of Pap's functional a priori is correct, then the functional a priori does not set Pap apart from Reichenbach.

In this chapter we have seen that the a priori concept went through several phases. It changed from an aristotelian concept into a kantian one, with different faces: in Kant's philosophy the a priori can be either analytic or synthetic. After Kant's time it changed into a neokantian concept which, in the 20$^{\text{th}}$ century, was made part of logical positivist thought in the work of Reichenbach and Pap. Before we start our analysis of Reichenbach's work, we will investigate the role of the (relativised) a priori in the theory of probability before Reichenbach's time.

[5] Cf. [Pap, 1944].

# Chapter 3

# Classical Probability

**The contents of this chapter**

1. Pierre-Simon Laplace

2. Johannes von Kries

Chapter one suggested a relation between a mechanicist worldview and the emergence of the classical theory of probability and in chapter two we investigated the different guises that the concept of the a priori has had. In the current chapter we will combine the findings from the first two chapters: we will show what role Reichenbach's relativised a priori plays in the classical theory of probability.

In the 17$^{\text{th}}$ and 18$^{\text{th}}$ centuries there had been numerous early interpretations of the concept of probability[1]. From a concept associated with frivolous pastimes (gambling games; cf [David, 1990]) probability developed into a highly formalised mathematical discipline (as attested by the works of, for example, [Bernoulli, 1713] and [Bayes, 1763]).

In this chapter we will focus on Pierre-Simon Laplace's "Philosophical Essay on Probabilities" ("Essai philosophique sur les probabilités") which appeared as an introduction to the second edition of Laplace's "Analytical Theory of Probabilities" ("Theorie analytique des probabilités"; first published in 1812). The work of Laplace that is referred to in this section is the version of Laplace's *Essay* which was published separately in 1814 [Laplace, 1814].

In the second part of this chapter we shall analyse the probability interpretation of Johannes von Kries. Von Kries adopts Laplace's definition of

[1] cf [Hacking, 2003] for a vivid account of the emergence of probability. An older yet more detailed account of the development of the theory of probability can be found in [Todhunter, 1865].

probability but advocates a very specific interpretation of the definition: the definition should involve measurable quantities whose values reflect physically existing structure.

## 3.1 Pierre-Simon Laplace

In order to show how his concept of probability should be placed within mechanicist philosophy[2], Laplace starts his *Essay* with the following words:

> "An intelligence which, for one given instant, would know all the forces by which nature is animated and the respective situation of the entities which compose it, if besides it were sufficiently vast to submit all these data to mathematical analysis, would encompass in the same formula the movements of the largest bodies in the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes" ([Laplace, 1814], pp.34)

**Figure 3.1:** Pierre-Simon Laplace (1749-1827).

Everything in the universe is guided by a 'principle of sufficient reason'. According to this principle—which Laplace calls 'evident'—nothing can happen without a cause.[3]

### 3.1.1 Probability

Within this deterministic context there is room for probabilistic considerations. In the context of a throw with a die Laplace writes the following:

> "The theory of chance consists in reducing all the events of the same kind to a certain number of cases equally possible, that is to say, to such as we may be equally undecided about in regard to their existence, and in determining the number of cases favorable to the event whose probability is sought. The ratio of this number to that of all the cases possible is the measure of this probability,

[2] [van Strien, 2014] argues that it was not Laplace's intention to provide his readers with a foundation for his views on determinism, but that Laplace's specific view on determinism is a 'collateral effect' of his attempt to characterise probability.

[3] "Present events are connected with preceding ones by a tie based upon the evident principle that a thing cannot occur without a cause which produces it. This axiom, known by the name of the principle of sufficient reason..." (p.3)

> which is thus simply a fraction whose numerator is the number of favorable cases and whose denominator is the number of all the cases possible." ([Laplace, 1814], p.7[4])

In relation to this quote of Laplace we consider the following questions:

1. what are 'events of the same kind'?

2. what makes cases 'favourable'?

3. what does 'equally possible' mean?

**1** To be able to speak about the probability of events we must start by determining what 'events' are. We must precisely specify spatial and temporal limits that serve to define events (while those limits may seem obvious in the case of a die-throw, it is less straightforward what should count as an event in a particle accelerator[5]). The sameness of events should be understood in terms of this specification: events are the same if they are specified in the same way.

**2** The second question can be answered in a straightforward way: it is the probabilistic question that makes certain cases favourable with respect to the sought-for probability. For example, we throw a six-sided die and ask for the probability of an even number. We know that the number of favourable cases is three because we asked for the probability of an even number and there are three even numbers below seven[6].

**3** Thirdly, Laplace mentions cases that are equally possible[7]. Laplace states that cases are equipossible when we are equally undecided about their occurrence.

[4]The French original reads as follows: "La théorie des hasards consiste à réduire tous les évènemens du même genre, à un certain nombre de cas également possibles , c'est-à-dire tels que nous soyons également indécis sur leur existence, et à déterminer le nombre de cas favorables à l'événement dont on cherche la probabilité. Le rapport de ce nombre à celui de tous les cas possibles, est la mesure de cette probabilité qui n'est ainsi qu'une fraction dont le numérateur est le nombre des cas favorables, et dont le dénominateur est le nombre de tous les cas possibles."

[5]Or, less anachronistically, in considerations about the weather.

[6]It might seem obvious that it is a characteristic of our number-system that there are three even numbers below seven. Be that as it may, it entails that the relation between probabilistic favourability and the probabilistic question is not a wholly subjective affair. And therefore it limits the degree to which Laplace's probability can be said to be objective in a very real sense.

[7]Ian Hacking has made a detailed investigation of the history of the concept of equipossibility in [Hacking, 1971]. According to Hacking the concept does not originate with Laplace, but was already commonplace in the time of Leibniz (more than a century earlier).

It is a well-known point of criticism against Laplace's classical definition that it is circular because in the definition it is assumed that equipossible cases are equally probable[8]. The criticism is usually formulated along the following lines.

If we want to use Laplace's approach to understand/interpret the concept of probability we want to be able to justify the statement that certain cases are equipossible independently of the concept of probability. Basing probability judgements on judgements of equipossibility is circular if we assume that equipossible cases are equally probable (because then we define probability in terms of equal probability). A crucial point in this criticism of Laplace's approach is the idea that Laplace does not justify equipossibility independently of probability.

This criticism of Laplace's approach seems to be oversimplified as Laplace defines equipossibility in terms of our indifference and not in terms of probability. Laplace uses our indifference to define equipossible cases, and from this defines 'equally probable'. The point that Laplace's probability interpretation is not circular is argued for by [Uffink, 2011][9].

Laplace's characterization of equipossible cases as equiprobable, in the way just explained, defines a specific conceptual framework—a paradigm—for probability theory, which in some respects functions as Reichenbach's relativised a priori.

Consider a die-throw and suppose that we have specified that the outcomes of the throw are the events of interest. Laplace's approach requires that we identify events such that we have no reason to expect the occurrence of one of these events rather than another. Laplace infers the equiprobability of these events from our indifference: he considers events with regard to which we are indifferent to be equally probable. This step from indifference to equiprobability is essential to Laplace's probabilistic framework[10].

Any probability statement within Laplace's view presupposes the inference from indifference to equiprobability. This implies that within Laplace's view on probability nothing can contradict the use of this inference. Within

[8]See, for example [Mill, 1843] and [von Mises, 1928]. See also [Hájek, 2011] for a contemporary analysis of this issue.

[9]Uffink states that "The very point of the classical interpretation of probability is that a probability assignment should represent an epistemic judgement about a set of possible events. What the principle demands, therefore, is that if there happens to be a symmetry in our judgement, it should be reflected by a symmetry in the probability assignment. The principle [which justifies Laplace's inference from indifference to equiprobability] is therefore not circular at all."

[10]Authors later than Laplace say that the inference from indifference to equiprobability rests on a principle (of insufficient reason/indifference). We do not follow this convention as Laplace's inference from indifference to equiprobability is best seen as part of a definition and should therefore not be called a principle.

Laplace's view the inference's validity is a priori valid in the empirically a priori (analytic) sense of a priori. Of course it is possible that a probability statement that we derive later may contradict an earlier probability statement (eg when a die which we believed to be fair turns out to be loaded) but then we conclude that the indifference-inference was made in the wrong way (ie applied to the wrong events); not that the indifference-inference should not be made. The inference from indifference to equiprobability is (analytically) a priori within the limits of Laplace's view.

Put differently, the inference from indifference to equiprobability is a defining characteristic of Laplace's probability concept. The inference is empirically a priori (analytic) as it is presupposed by all Laplace's probability statements.

The indifference-inference is only empirically a priori (analytic) within Laplace's approach to probability (or other similar subjective interpretations of probability). In this respect there are similarities with Reichenbach's relativised a priori. For example, we saw that the parallelogram theorem for the addition of vectors is an analytic truth. However, when the theorem is applied to observed forces it becomes conditional on the truth of the relativised a priori statement that observed forces behave as vectors.

The issues about probability, causation, and apriority that we have touched upon here and in the previous chapters will be revisited in a more comprehensive manner in our investigation of Reichenbach's work. However, before we move on to Reichenbach's work we will analyse the probability interpretation of Johannes von Kries who in some respects may be thought of as one of the last followers of Laplace's classical interpretation. We will see that in von Kries' work, too, there is an element of the relativised a priori.

## 3.2 Johannes von Kries

### The contents of this section

1. Event Spaces
2. Elementary Event Spaces as Relativised A Priori

In the first section of this chapter we studied Laplace's classical interpretation of probability. In the 19$^{\text{th}}$ century there were various kinds of responses to the work of Laplace. The authors of most of these responses concurred with Laplace that probability should be defined in terms of a ratio among cases but they differ in their appraisal of the equipossibility or equiprobability of these cases. For example, J S Mill in his early writings advocated the stance that equipossible cases are equiprobable and that such equiprobability must manifest itself as equal relative frequencies of these cases in our observations ([Mill, 1843], p63). Other authors, such as Carl Stumpf ([Stumpf, 1892a] and [Stumpf, 1892b]), emphasised that Laplace's definition does not rely on an objective basis in physical reality [11]. Stumpf's subjectivist approach to probability follows the main lines of Laplace's approach. Stumpf bases probability judgements as much as possible on knowledge about causal relations and uses, just as did Laplace, our indifference to infer equiprobability from that.

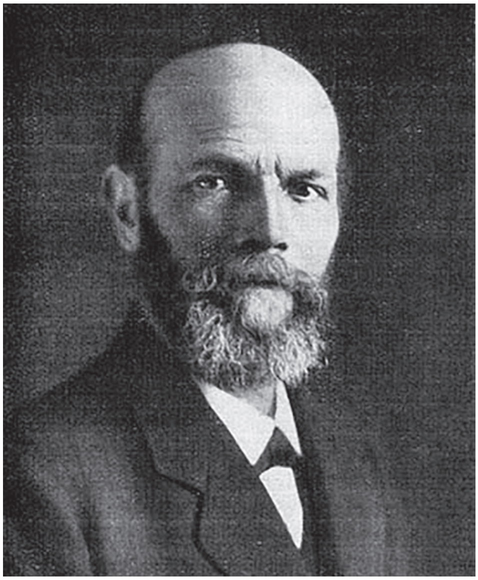

**Figure 3.2:** Johannes von Kries (1853-1928).

In this section we shall be concerned, however, with a fervent defender of an interpretation of probability in terms of objectively existing physical structures: the German physiologist Johannes von Kries[12]. If we interpret

[11] Cf [Benedictus, 2015] for an account of objectivity in Stumpf's writings.

[12] See [Buldt, 2016] for a description of how von Kries' views on probability tie in with his physiology. For our description of von Kries' views we have focussed on the aspects that are important for the arguments in this dissertation. For a discussion of other elements of von Kries' views see [Rosenthal, 2016]

Laplace's classical definition of probability as von Kries does (in terms of physical structures) then a statement of probability is to be regarded as a statement about the existence of a causal/physical structure of the process that occurs in nature. For example, the statement that throwing a six with a fair die is $\frac{1}{6}$ entails (in the approach of von Kries) that the die plus the experimental set-up in which it finds itself possesses a physical symmetry. The experiment has a physical structure such that the part of phase space connected with the outcome six fills up one-sixth of the total available volume.

In 1886 von Kries wrote an account of his interpretation with the title "Principles of the Calculus of Probabilities—a Logical Investigation"[13]. In this work von Kries invokes the notion of *event spaces* and a rule to assign measures to them. In this section we will first discuss von Kries' notion of event spaces and subsequently use this analysis to determine what assumptions von Kries must make to make his account of probability coherent.

On more than one occasion the account of von Kries has been referred to as a *logical* interpretation of probability (See, for example, [Kamlah, 1987] or [Heidelberger, 2001]. The application of the label logical to von Kries' view is understandable, because the subtitle of von Kries' work on probability tells us that this work is a "logical investigation". However, if we adopt the terminology current in modern philosophy of science, calling von Kries' account a logical interpretation is specious. Of course, the rules of logic guide von Kries in determining degrees of probability, and in that sense von Kries' account must be called logical. But in modern philosophy of science the term 'logical interpretation of probability' has become associated with a very specific interpretation of probability according to which probability is defined as a logical relation between sentences within a language. In the logical interpretation of probability this logical relation is taken to be primitive—it is not, as it is in von Kries' account, the result of a pre-existing causal structure.

### 3.2.1 Event Spaces

Von Kries' interpretation of Laplace's definition in terms of event spaces rests on the assumption that degrees of probability represent an existing physical structure. In von Kries' approach the probability of some experimental outcome is defined in terms of measurable equalities in the experiment's physical structure. To illustrate von Kries' ideas we consider the example of the roll of a die. There are six different outcomes constituting a 'universe of events'—the *event space*. In this event space every possible outcome has its part with which it can be associated. But these parts can be split up: each outcome can be realized by very many microscopic configurations that all manifest themselves

[13] "Principien der Wahrscheinlichkeitsrechnung—Eine logische Untersuchung" ([von Kries, 1886]).

macroscopically in the same way. We can then assign a measure to this space of more fundamental microscopic events. This thought led von Kries to define the probability of a specific outcome as the ratio of the size of its specific part in the universe of events to the size of the total universe of events, corresponding to all possible outcomes. It is then possible to say which outcomes are equiprobable—namely the outcomes whose intervals in the universe of events are of equal size. Given the symmetries of the situation, this procedure plausibly leads to assigning equal probabilities to all six possible results of the throw with a fair die in a situation of undirected tosses: one in six (see fig3.3).

But what if our die is biased, loaded in such a way that the sides are not equiprobable? In that case the volumes in the universe of events that are associated with the different outcomes are not equally sized. Von Kries argues that in such cases we should look at a more detailed description of the physical structure of the situation. The parts of phase space associated with the outcomes are unequal in size (because the die is non-homogeneous) but according to von Kries can be split up into smaller equally-sized intervals at a more detailed level of description.

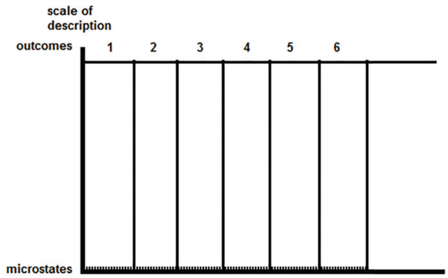


**Figure 3.3:** A throw with a six-sided die. It is a fair die so all 6 possible outcomes can be associated with equally sized event space intervals. These intervals each correspond to an equal number of microstates.

To illustrate the idea, suppose we increase the detail of our description of the die. Given the asymmetries in the physical situation—the die is loaded—we shall find that the number of microstates in the beginning of the experiment that will manifest themselves after the toss as one macroscopic outcome will not be the same for all outcomes. If we go to an even more detailed level of description then these microstates will split up even further. However, if we go to ever more detailed levels of description, this splitting up will from a certain level onwards not happen in an asymmetrical way (or at least that is assumed in von Kries' approach). If we have reached this situation von Kries dubs the events within the universe of events 'elementary' (see fig3.4), and the event space at this level of description is called an elementary event space. These elementary events, which do not split up asymmetrically upon further refinement of the description, are taken to be equiprobable.

To be more specific about the temporal sequence of events we must recognise in physical processes an initial state and an endstate. According to von Kries we are entitled to assume that there exists a one-one correspondence

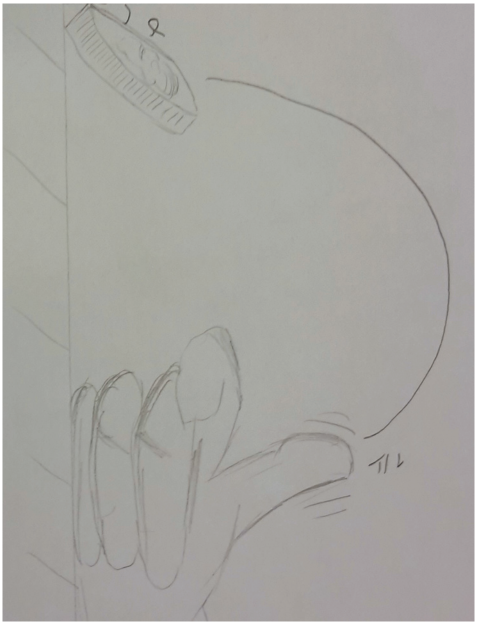

**Figure 3.5:** A cointoss. The initial and end states are exhaustively described by $\alpha$ and $F$ (drawing by Jesper Oldenburger).

between initial and endstates. By retracing the causal nexus between initial and endstates we can therefore find out which parts of the initial event space correspond to parts of the endstate-space. Knowing the equiprobable elementary events in the initial space, we can now indicate which end-states of the die are equiprobable.

The basic idea in von Kries's method is to take every elementary event (every microstate or set of microstates which together constitute an elementary even) as equally probable. The idea here is that we should continue our analysis of the physical situation until we find 'simple, non-composite' causes; the elementary event spaces we thus ultimately arrive at will perhaps be descriptions in terms of the states of the individual atoms of which the die consists, plus a detailed description of the tossing procedure.[14] In von Kries' approach the probability of an event is defined at one instant of time as the ratio of the number of elementary events with which it can be associated to the total number of elementary events which characterise the event's physical context.

To give a practical example of von Kries's method we may consider a toss with a symmetrical coin. For our present purpose it suffices to give a highly idealised description of this situation. We shall use one variable to describe the

[14]the atomic concept had not yet gained general acceptance in von Kries's time, but 'small quantities of matter' could serve a similar explanatory role.

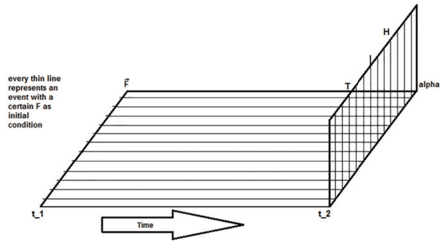


**Figure 3.6:** The development of a situation in which a coin is tossed.

initial state of the toss: the force ($F$) with which the coin is tossed. Likewise, there is only one variable that describes the endstate of the toss: the angle $\alpha$ between the coin and the normal vector of the plane upon which it falls (see fig3.5). We assume that the two partial domains of $\alpha$ (0°-90°and 90°-180°) correspond to the two outcomes of the coin toss (Heads and Tails).

The resulting process is depicted in fig3.6. On the left in the figure there is a range of initial states, each of which is characterised by a different value of $F$; on the right there is a range of endstates which is represented on two scales. Firstly, every possible endstate is characterised by a different value of $\alpha$; and, secondly, the different values of $\alpha$ can be grouped according to the outcome which they represent (H or T).

Von Kries' approach is based on the assumption that in every probabilistic process there are elementary events, but not every possible outcome needs to have the same number of elementary events associated with it. If we ask for the probability of H, we should determine the total number of possible microscopic configurations ($N$) of the physical system under consideration at a given point in time. After that we must determine the number of these that manifest themselves as H ($n_H$). The sought-for probability ($p$) is then the ratio $\frac{n_H}{N}$. This ratio can be translated between the different stages of the process. In the case of the coin that we described $p$ can be expressed in terms of $F$ or in terms of $\alpha$.

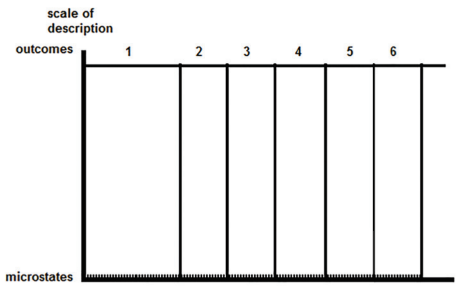


**Figure 3.4:** Again a throw with a six-sided die. This time the die is biased. Nevertheless, the 6 possible outcomes can be traced back to equally sized intervals on a more detailed level. Such intervals can be associated with elementary events (eg a single outcome of the die-throw).

### 3.2.2 Elementary Event Spaces as Relativised A Priori

How does von Kries justify his approach of probability in terms of event spaces? Von Kries discusses ([von Kries, 1886], p170) Adolf Fick's identification of probability judgements as synthetic a priori judgements. Fick had argued that the synthetic a priori justification of such judgements is suggested by the combination of their syntheticity and their unempirical nature ([Fick, 1883], p46). In his 1883 article "Philosophical Investigation of Probability"[15], Fick suggests that perhaps judgements about probability are the most plausible candidates to be synthetic a priori judgements.

Von Kries is not impressed by Fick's reference to probability statements as synthetic a priori statements. He calls the application of the term synthetic a priori in this context *arbitrary*[16]—a mere name with no explanatory value. In the account of von Kries the equiprobability of the elementary event spaces is a *primitive* notion. That means that its existence cannot be derived from the existence of some more fundamental entity (its existence cannot be deduced)—the existence of equiprobable, elementary event spaces is an *assumption*.

Von Kries assumes as fully unproblematic that physical events can be identified that are connected via deterministic laws; in his view, this is guaranteed by the principle of sufficient reason. It does not follow, however, that there are *elementary* event spaces. To see this we take another look at fig 3.4. The event spaces at the level of the outcomes are clearly asymmetrically distributed. Von Kries' method consists in probing the situation under consideration on ever more detailed scales until we find event spaces which do not split up in an asymmetrical way. But it might also be the case that no such event spaces exist. It might be that no matter how much we zoom in to determine a distribution of event spaces, further zooming in will yield differently distributed event spaces. In fig 3.4 this would mean that we would see asymmetrical splitting all the way down, or even that the distribution of event spaces always changes if we go to a more detailed level of description. The emerging situation may be rather counterintuitive, but it is not impossible. However, von Kries must assume that elementary event spaces always do exist in order to apply his definition of probability.

What is the nature of this assumption of von Kries? Since the definition or characterisation of event spaces does not entail the existence of elementary event spaces, the premiss that elementary event spaces exist is not analytic. However, the possibility of judgements of probability—in von Kries' view—depends vitally on exactly this existence of elementary event spaces (judgements of probability are conditional on their existence). We may conclude that the assumption that elementary event spaces exist is an a priori assumption

[15] "Philosophischer Versuch über die Wahrscheinlichkeiten".

[16] "Willkürlich".

within the context of von Kries's specific chosen conceptual framework—it may be seen as a relativised a priori.

In part II we will see similar assumptions as von Kries' relativised a priori return in different guises in the work of Reichenbach and von Mises (as Reichenbach's posits and von Mises' randomness as defining property of probability statements).

# Part II

# Probability & Reichenbach's Epistemology

# Chapter 4

# Reichenbach's dissertation (1916)

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   (b) The 'subjectivists'
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   (b) Equiprobability & the Probability Function
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   (b) Rational Expectation
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Hans Reichenbach was born in 1891. After finishing high school in his birthplace, Hamburg, he decided to follow the family tradition and become an engineer. However, after only two semesters at the Technische Hochschule in Stuttgart, Hans tired of civil engineering and turned his attention to the exact sciences. As he himself writes 25 years later, his interest in philosophy had not

**Figure 4.1:** Hans Reichenbach (1891—1953)

been too great at that time: he felt a certain disdain for its inexactness ([Reichenbach and Cohen, 1978]; Vol I, p1). Furthermore, with the exception of the writings of Kant there seemed to be no clear connection between philosophy and the natural sciences. This was unfortunate, because it was at this time that Reichenbach became interested in the philosophical foundations of the kinetic theory of gases, in which the concept of probability plays an important role. He chose this concept as the topic of his PhD-dissertation: "The concept of probability in the mathematical representation of reality" [Reichenbach, 1916][1].

In this chapter we will see how in Reichenbach's interpretation probability emerges in a universe where all events are related through causal laws. In the first two sections of the chapter we give a description of Reichenbach's views. Our own commentary in these sections is restricted to the notes. Our focus changes in the concluding section in which we use the background of the issues as described in the previous parts of this dissertation to evaluate Reichenbach's claims.

## 4.1 The Opposition

### Statement of the Problem

Reichenbach starts his PhD thesis with an appraisal of the philosophy of his time. He states that the discussion of foundational issues has split the community of philosophers in two. The discovery that in all knowledge there are traces of subjectivity has led many to believe that objective knowledge is an

[1] 'Der Begriff der Wahrscheinlichkeit für die mathematische Darstellung der Wirklichkeit'.

utter impossibility. Others have seen these traces as a reason to change the very aim of philosophy; those philosophers wish to determine which elements of our knowledge are objectively true. It is this context in which Reichenbach introduces the concept of probability. The irregularity of daily life makes the layman's imprecise notion of this concept understandable, but its great importance for the exact sciences demands a more rigorous formulation. Reichenbach claims that due to the strict determinism that is universally accepted in his days, probability is often misinterpreted as being merely a measure of our subjective expectation. About the paradoxical relation between causation and determinism on the one hand and probability on the other Reichenbach says the following:

> "Indeed, this paradox has led many philosophers to believe that the concept of probability only represents our subjective expectation, which does not have any connection to the real world. Similarly, the opposite view has been developed; but it is noteworthy that this view has not completely eliminated the subjective element from the concept of probability, either." ([Reichenbach, 1916], p41)

Reichenbach in his dissertation intends to answer two related questions: 1) what role is there in a deterministic worldview for the concept of probability and 2) how can the scientist provide an objective foundation for statements of probability?

### The 'subjectivists'

In order to delineate the subjectivist's account of probability, Reichenbach turns to the work of Carl Stumpf ([Reichenbach, 1916], p42 ff). Stumpf basically adopts Laplace's definition of probability. Reichenbach begins his evaluation of the subjectivist's account by quoting this definition given by Stumpf:

> "We say that any event is $\frac{n}{N}$ probable if we can conceive of it as one of $n$ elements (favourable instances) among a total of $N$ elements (possible instances), of which we know that exactly one is true, but we do not know which."

To guarantee that Stumpf's definition agrees with what we intuitively understand as probability, it is necessary that the different cases figuring in it are equally probable. In Stumpf's view equiprobability follows from a principle of insufficient reason: if we do not have any reason to believe in the occurrence of any one of the cases rather than another then we are justified in believing that all cases are equally probable. Because such a characterisation of equiprobable cases essentially depends on the (subjective) degree of knowledge of the person making the characterisation, Reichenbach critisises Stumpf for not providing objective criteria to determine which cases are equally probable. According to Reichenbach the insufficient-reason-principle makes Stumpf's probability into a mere description of our subjective judgement. Nothing is wrong with description per se, Reichenbach admits — Stumpf's interpretation is consistent. Reichenbach's objection is that Stumpf's probability statements do not provide us with what we expect from them: a basis for rational expectation[2].

Reichenbach illustrates this deficit in Stumpf's account with an example. Suppose we know that there is a comet in a stable orbit around the sun. We ask for the probability that the comet, of which we know nothing else than that it is in a stable orbit, has an elliptically shaped orbit. If we know that the orbit is either a hyperbola, a parabola, an ellipse or a circle, then Stumpf would judge that the aforementioned probability equals $\frac{1}{4}$. However, if we later realise that a circle is actually a certain kind of ellipse (with an eccentricity equal to zero) then the probability becomes $\frac{1}{3}$. It seems that the extent of our knowledge determines which cases are equiprobable.

Indeed, Reichenbach says, it is not inconsistent to have different beliefs at different times about the degree of probability of some state of affairs. But because Stumpf's view does not demand that there is a physical difference corresponding to the difference in beliefs, Stumpf's probability concept cannot yield a basis for rational expectation.

The arbitrariness of probability robs the probability judgement of any predictive value (any value that was not already contained in the statement that the comet is in a stable orbit around the sun). It is not the task of the theory of probability to describe what our expectations *are* (to express in a different way what we already know), Reichenbach says, but to tell us what our expectations *ought to be*.

Remarkably, in addition to this criticism of Stumpf's interpretation, Reichenbach claims that Stumpf's reference to the work of Laplace is unjustified. Reichenbach is willing to admit that Laplace has provided us with an inadequate characterisation of equiprobable events, but argues (citing from Laplace's *Essay Philosophique*, which we discussed in sect 3.1) that Laplace was not an outright subjectivist and was well aware of the delicacy of the matter.

### The 'objectivists'

Reichenbach takes Johannes von Kries ([von Kries, 1886]) as an advocate of a better-founded scientific conception of probability. In Reichenbach's eyes, von Kries' approach represents a significant advance towards an *objective* interpretation of probability, even though it does not lead us far enough. We

[2] "Mass der vernünftigen Erwartung" [Reichenbach, 1916], p46.

will proceed to show how von Kries' account falls short of Reichenbach's own aim of objectivity.

According to von Kries the principal shortcoming of the subjectivist's approach to the concept of probability is the absence of a clear justification of equiprobability assignments. Von Kries wishes to avoid the use of the principle upon which the subjectivist's equiprobability rests; the principle of insufficient reason. We have already seen (in sect 3.2) how he uses his idea of *event spaces* in an attempt to provide an objective basis for statements of probability.

However, Reichenbach judges that the principle of event spaces is insufficiently rigorous. Von Kries' belief that event spaces exist which are equally sized, probabilistically indifferent, and elementary (which we discussed on p39) should be made part of a mathematically rigorous theoretical scheme, in order to make the concept of probability valuable for science. Furthermore, Reichenbach objects that von Kries does not deduce his principle of event spaces within an overarching philosophical framework—he merely postulates it. In von Kries' proposal equal volumes in the elementary event space are considered as equiprobable, apparently because there is no plausible reason to distinguish between them. But this smacks of Stumpf's principle of insufficient reason, which Reichenbach rejects as unscientific. So it seems that even von Kries' approach at its very basis contains a hidden element of subjectivism, which disqualifies it as an objectivist approach.

In conclusion to the first chapter of his dissertation, Reichenbach discusses several other authors who have attempted to provide a consistent interpretation of the theory of probability. These authors vary in their appraisal of equiprobability. According to Reichenbach none of these earlier authors' interpretations is sufficiently objective to serve as a basis for rational expectation. The author that comes closest to rinsing off subjectivity is Adolf Fick. In his writing on the topic 'Philosophical Essay on Probability' ('Philosophischer Versuch über die Wahrscheinlichkeiten'; 1883) Fick argues that statements of probability should be interpreted in a kantian sense as synthetic a priori statements, but he forgoes any rigorous deduction of the kantian kind. We have seen (above; p42) that the possibility of assigning to statements of probability (or at least the principle upon which they are based; the principle of event spaces) the status of synthetic a priori had been criticised by von Kries, but Reichenbach refers to Fick's article without noting von Kries' criticism[3].

Another interesting point in Fick's writing requires our attention as it

[3]Reichenbach addresses von Kries' criticism implicitly when he writes "If it is possible to show that a principle forms the basic assumption for wide areas of knowledge, then a great deal has already been done towards the empirical proof of its validity. Since then it may make no lesser claim to its validity than these areas of knowledge make for themselves [...]" [Reichenbach, 1916], p105

foreshadows Reichenbach's position in his dissertation. It is perhaps best to state it in the words of Fick himself:

> "The fact that there appears to be an asymptotic relationship between probability and reality poses a problem for metaphysics which might be solvable." (as quoted in [Reichenbach, 1916], p57)

We will see later that it is precisely this metaphysical problem—that there appears to be an asymptotic relation between probability and reality—that Reichenbach claims to have solved with his own interpretation of probability. To present this interpretation Reichenbach introduces a hypothetical probability-machine—the transparency of the operation of this machine will make sure that anyone considering it would agree that it is subject to the laws of probability. Therefore an analysis of the workings of the machine will allow Reichenbach to lay bare the (tacit) assumptions made by anyone using these laws.

## 4.2 Reichenbach's Early Probability

### Probability & Causation

Reichenbach in his dissertation attempts to give a description in mathematical terms of the role that probability plays in science. An essential element in science, according to Reichenbach, is the attempt to coordinate mathematical concepts to entities in reality. If there are regularities in reality (typically, when particular types of events always follow each other) then we can describe these regularities mathematically by using (what Reichenbach calls) a "principle of lawful connection". Such lawful connection would describe which types of events follow each other. We may consider the example of a falling stone in an ideal situation in which there are no external influences (ie the falling stone is causally independent from its physical surroundings). Suppose that, in such an ideal situation, we could drop a stone from height $h$. The assumption that all events are connected by Newton's laws allows us to coordinate the path of the uniformly accelerated stone with a curve using the following function of $t$: $(x(t) = h - \frac{1}{2}gt^2)$.

Reichenbach argues that in actual science the representation of a causal law by a precise mathematical function is not be confused with the presupposition that everything in our universe actually follows strict causal regularities. Suppose that in our experiment with the stone we drop the idealisation assumption (and thus allow for external influences). It is almost certain that our stone will deviate from the path that is prescribed by Newton's laws. In fact, *any* causal law we assume to hold in an ideal experimental setup (where there are no external influences) will almost certainly be contradicted by observations of a real situation.

Reichenbach's view on causation involves the assumption that there are no closed systems (causally independent regions) in the universe. Therefore, in any physical situation there are external factors which potentially disturb lawlike behaviour. Suppose that we know about some developing physical situation 1) that the initial conditions are such that an event of type $A$ occurs, and 2) that we live in a universe in which physical laws connect events of type $A$ to events of type $B$. The above-mentioned lack of closure entails that the combination of 1) and 2) is insufficient to enable us to say with certainty that an event of type $B$ will follow the event of type $A$.

Only in ideal scientific experiments lawful connection suffices to link the observed outcomes to a mathematical description of the experiment. Such a connection would describe the causal nexus from the experimental setup to the only possible outcome. However, to be able to regard observations as the result of *deviations* from a causal law we need something more than the principle of lawful connection: we need a principle of lawful distribution. To use Reichenbach's words, the principle of lawful distribution connects events 'orthogonally'.

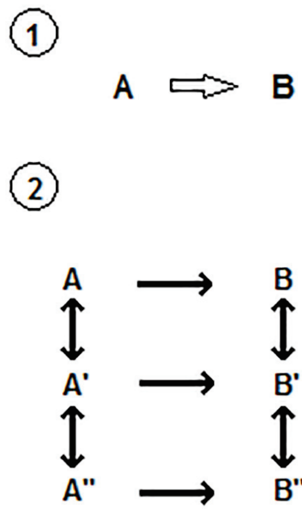


**Figure 4.2:** Suppose we live in a universe in which A is lawfully connected to B and the initial state of some experiment is A. In Reichenbach's view the uncertainty regarding the process results in a distribution over possible outcomes (B, B' and B"). Causation (represented by the horizontal arrows) and probability (vertical arrows) connect events in orthogonal directions.

This orthogonal connection is illustrated in fig4.2. Schematically, the causal sequences ($A \to B,\ A' \to B',\ A'' \to B''$) in some way must be connected in an orthogonal direction to make it possible that we regard the same mathematical equations as a representation of a stable reality governed by causal laws in the face of fluctuations that we observe in experiments (so that we can have strict laws connecting the events $A$ and $B$ even if we do not observe the associated strict sequence of events).[4]

[4]It seems plausible that von Kries would have argued that the orthogonal connection

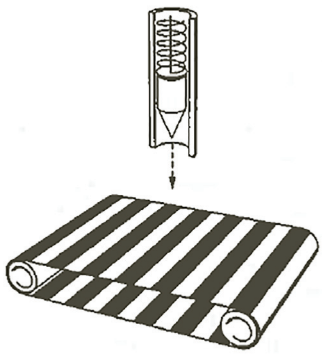

**Figure 4.3:** Reichenbach's probability machine ([Reichenbach, 1916], p7).

## Equiprobability & The Probability Function ($\phi$)

In our discussion of the views of Stumpf and von Kries (and earlier that of Laplace) we discovered that the justification of equiprobability plays an important role in interpreting probability. To investigate equiprobability Reichenbach devises a 'probability machine'. The machine serves to determine precisely which characteristics of any scientific experiment guarantee that any rational observer would believe that certain of the experiment's outcomes are equally probable.

The machine is a device shooting a projectile at a moving band of paper[5] covered with stripes of equal width. The stripes on the band are alternatingly black or white (see fig 4.3). In every separate run of the experiment the projectile is shot downwards and hits the band. The band is of finite length and the machine is set up in such a way that in every run of the machine the band completes one full lap around its track. Also, the machine is set up so that in every run of the machine the projectile is shot with the same force.

Therefore—in an ideal deterministic setting—there would be equal time-intervals in between the shots and so the projectile would hit either a white or a black stripe consistently. But, as we have discussed in the previous section, nature is not that monotonous. There are always myriads of minute perturbing factors. For example, it might be the case that variations in the temperature

between the events $A$ and $A'$ relates to an (approximate) symmetry in the material/causal structure of the events $A$ and $A'$. In case $A$ and $A'$ represent possible initial states of some probabilistic process (say, throws with a symmetrical die differing only in the force $\vec{F}$ with which the die is thrown) and $B$ and $B'$ represent possible outcomes of that process (say, 1 and 2) then the orthogonal connection between $A$ and $A'$ represents a relation between the material/causal structure of the two different initial states of the process (the relation between the different values of $\vec{F}$ in $A$ and $A'$).

[5]For Reichenbach's argument it is not essential that the band is moving.

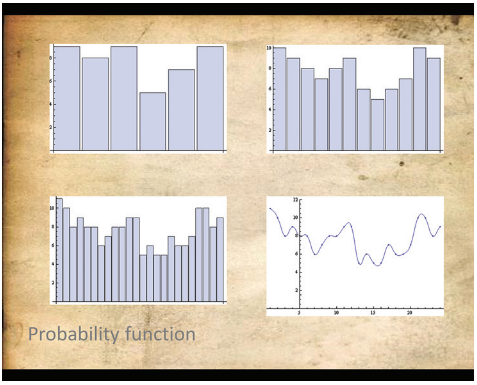


**Figure 4.4:** Reichenbach's 'probability-function' is an unobservable property of his probability machine (and any other probabilistic process). As the stripes become increasingly narrow, the tops of the bars in this bar-chart approximate $\phi$.

of the air around the machine influence the speed of the projectile; passing traffic might cause the probability machine to quake a little bit; we might even take into account the disturbing gravitational effect of a passing comet. Because of all this, the interval between the shots will never be exactly the same.

If the stripes are narrow enough the shootings will result in a non-trivial distribution over the black and white stripes (if the stripes are not narrow the fluctuations will never cause the result to deviate from the result predicted in ideal deterministic circumstances). Suppose that we represent the observed results of the probability machine in a bar-chart in the following way. We divide up the horizontal axis in the chart in segments each of which represents one of our machine's passing stripes. The vertical axis shows the relative frequency with which each stripe is hit. Any finite number of runs with the probability machine would result in a stepwise bar-chart. We may list the characteristics that the experiment with the probability machine must have in order to ensure that any rational observer would expect either a white stripe or a black stripe being hit with equal probability:

1. No one would expect the outcomes to be hit equally often after only a few shootings. The machine has to be run often[6].

2. The processes whose coincidence forms the events that we are interested in are assumed to be independent. In our machine two of such processes are the shooting of the projectile and the way the striped band is con-

[6]The applicability of probability laws to single instances was—and still is—an issue of debate. Fick and von Kries believe probability theory is applicable in those cases, and this idea gives rise to what later became known as the propensity-interpretation.

stituted and how it is moving. If, for example, the projectile were made out of metal while only the white stripes were covered with small magnets, the white stripes would be hit more often. No one would expect an equiprobability distribution of hits of black and white stripes in such a situation[7].

3. The processes need to produce some common effect to make the coincidences visible.

4. It must be possible to order the coincidences to enable us to count the relative frequencies of the different types of coincidences (in our case those coincidences are the black and white stripes being hit).

5. We want a convergence of the observed distribution to a definite distribution with certain characteristics within a finite number of repetitions.

### The Probability Function ($\phi$)

Reichenbach introduces the notion of a 'probability-function', $\phi$, in the context of the workings of his probability machine. $\phi$ allows Reichenbach to express the characteristics listed above in mathematical terms.

Reichenbach introduces $\phi$ in the context of an idealising condition for the width of the stripes in his machine when the machine has operated an infinite amount of time: *$\phi$ is the concatenation of the tops in the bar-chart introduced above in the limit of the width of the stripes approaching zero and the number of runs of the machine going to infinity* (see fig 4.4). Reichenbach's $\phi$ represents an (unobservable) extrapolation of the observed relative frequencies of the stripes being hit.

In order for $\phi$ to capture the characteristics of probability that were listed, Reichenbach demands that it is Riemann integrable and that the area it encloses is finite. In formal terms:

$$\int \phi(x)dx = finite \tag{4.1}$$

The characteristics in the list above are not only characteristics of Reichenbach's probability machine; they are shared by any physical system with which we associate degrees of probability. Reichenbach ([Reichenbach, 1916], p75) divides the characteristics into two different categories: 1) characteristics that

[7]Reichenbach notes that this independence will not be total (there are no closed systems in Reichenbach's universe) but approximate. It is not completely clear here how Reichenbach defines independence. Independence in terms of probability presupposes a concept of probability and is therefore problematic in the present context. Reichenbach apparently thinks of independence in a causal sense, but it is not self-evident what the connection is between this independence, equiprobability and relative frequencies.

specify which physical regularities are responsible for the observed relative frequencies (elements 1 and 5 in the list above); and 2) characteristics that serve to make the underlying physical regularities visible (elements 2, 3 and 4 in the list). Reichenbach argues that it can be decided on empirical grounds whether his machine has the characteristics of the second kind, and it is therefore the first kind of characteristics that he focuses on. Those characteristics (many repetitions and the convergence of $\phi$) can be translated to a single condition: for the scientist to be able to reason about probabilities it must be possible to extrapolate the bar-chart resulting from a finite number of repetitions of some probabilistic experiment to a probability function $\phi$ that is at least piecewise continuous and covers a finite area.

An important characteristic of Reichenbach's probability function is that it makes it possible to determine a class of equiprobable events; it thus yields a stepping stone for calculating other degrees of probability. To see how this works we return to our bar-chart (fig4.4). If the white stripes are hit exactly as often as the black stripes, then the total shaded area equals the total unshaded area. That an event of this kind has occurred up to now can of course be observed, but that such a regularity (equal relative frequencies) will persist in future experiments, so that we can draw conclusions pertaining to the probabilities, requires an argument that transcends oservation—here the appeal to the existence of the distribution function comes in.

Reichenbach now shows, following an idea of Poincaré, how the piecewise continuity of $\phi$ can be used to give an argument about equiprobability. The essential assumption here is that for every white stripe there is a black stripe immediately adjacent to it. And because of $\phi$'s continuity, the values that $\phi$ assigns to these adjacent stripes are approximately equal. If the number of shootings of the projectile is large, we would therefore expect that the black and white stripes will be hit approximately equally often. In the limit of the number of runs going to infinity, the areas of any two immediately adjacent black and white bars in the bar chart will become precisely equal, and this implies the equiprobability of black and white. *If* $\phi$ with the mentioned mathematical properties exists, and the geometry of the stripes is as indicated,ie with black and white everywhere near to each other, *then* the equiprobability of black and white follows by mere logic, as an analytical result.

Reichenbach's $\phi$ is not given in experience. Nevertheless, the proposition that $\phi$ exists is a statement about reality. In Kant's terms we would say that the proposition that $\phi$ exists is synthetic, as its truth-status cannot be determined on the basis of solely an analysis of the terms used in the proposition (in the spirit of Kant we might say that "appreciating the truth of the proposition would seem to require some kind of active synthesis of the mind uniting the different constituent thoughts" ([Rey, 2013], par1.1)).

Because it can be made independently of observation *and* is synthetic, in Reichenbach's analysis the proposition that $\phi$ exists is synthetic a priori. We have encountered the synthetic a priori in an earlier chapter (p26). We saw that Kant justified his thesis that there are synthetic a priori truths by arguing that they correspond to principles that are necessary prerequisites for the very possibility of knowledge. Kant called arguments aimed at showing the indispensability of specific a priori principles for the obtainability of knowledge 'transcendental deductions' of these principles. In the next section we will explore Reichenbach's version of a transcendental deduction for our case, the existence of $\phi$.

Before we move on to our exploration of Reichenbach's transcendental deduction we note that Reichenbach's 1916 view on probability is an early version of what has become known as the frequentist interpretation of probability. Degrees of probability in Reichenbach's 1916 view are characteristic of an infinitely often repeated experiment. In an infinite sequence of outcomes of such an experiment, degrees of probability correspond to limiting relative frequencies. In Reichenbach's dissertation a probability proposition about some experiment is true when there is a convergence of the observed distribution of outcomes to a distribution $\phi$ as the number of repetitions of the experiment grows. Reichenbach does not make it completely clear, however, how the mathematical statement about convergence and the existence of a limit are to be interpreted in experimental practice—for example, how many repetitions $N$ should we consider before we can be satisfied that there is a definite limiting frequency ([Reichenbach, 1916], p137)?

It is instructive at this point to quickly compare Reichenbach's interpretation with that of von Kries. Von Kries, whose probability interpretation we discussed earlier (sect3.2), judged that—from an empiricist's point of view—a definition of probability in terms of frequencies is inferior to his own proposal. The reason is that equating degrees of probability with limiting relative frequencies makes degrees of probability unobservable, so that we never could specify an exact value of a probability by basing ourselves on measured frequencies. In von Kries' own approach limiting relative frequencies play no role in the definition of probabilities: for him, degrees of probability are representative of the causal/physical structure of the experimental set-up as reflected in properties of the event space. A theoretical investigation of this structure at different levels of detail will yield well-defined and precise degrees of probability. The practical determination of these probabilities therefore transcends direct observation and involves theoretical considerations; this is to some extent comparable to the problem of directly measuring probabilities that we identified in Reichenbach's approach.

Von Kries' probabilistic framework, like Reichenbach's 1916 dissertation, involved the assumption that in principle there is a fully causal context (all phenomena are caused and identical causes have identical effects). When we have completed the current survey of Reichenbach's 1916 views on probability we will discuss the *frequentist* approach to probability in greater detail in the next chapter. We will find that the frequentist approaches of von Mises and the later Reichenbach are independent from the assumption of determinism.

### Reichenbach's Transcendental deduction

Reichenbach's idea in 1916 of the synthetic a priori differs somewhat from Kant's original idea. Whereas Kant investigated synthetic a priori truths as an essential ingredient of knowledge in a general sense, Reichenbach restricts his attention to the synthetic a priori as an essential ingredient in *scientific* knowledge. An essential element in scientific knowledge, according to Reichenbach, is the coordination of mathematical concepts to regularities in reality. We have seen that in Reichenbach's 1916 view the existence of natural regularities means that events follow causal laws—they follow a law of connection.

In Kant's transcendental logic the idea that all events obey causal laws received a synthetic a priori status. Although beyond empirical justification, the very possibility of physical knowledge requires the validity of a principle of lawful connection. We have seen that Reichenbach argues that only in ideal experiments lawful connection suffices but that in all real experiments an additional principle is needed to connect the events: a principle of lawful distribution.

The principle of lawful distribution bridges the gap between observed outcomes and causal laws. Without Reichenbach's probabilistic considerations, speaking about causal laws makes no sense in terms of the empirical. Therefore, we may say that these probabilistic considerations—and therefore $\phi$—play a conditional role for our experience of causal laws. Without probability there would be no link between observations and causal laws; and because without such a link science would become impossible, the existence of $\phi$ is synthetic a priori in Reichenbach's sense.

So Reichenbach argues that the existence of $\phi$ plays the role of a necessary condition: if scientific knowledge is to be possible then $\phi$ must exist.

We see now why Reichenbach considered the metaphysical problem of the asymptotic relation between reality and the laws of probability that was discerned by Fick (above; p52) to be soluble. The problem Fick discerned is that probabilistic statements cannot be experimentally confirmed and yet the apparent asymptotic relation between observations and probabilistic laws suggests that the laws of probability tell us something about reality.

Reichenbach's account shows us that probabilistic statements—although strictly speaking experimentally unverifiable—can nevertheless be made and to some extent be justified on the basis of neokantian principles (Fick's "reality" is reality as it comes to us in our observations). The asymptotic relationship between probabilities and relative frequencies is warranted by the a priori of the existence of the distribution function.

## 4.3 Assessment

In this last section on Reichenbach's dissertation we drop our neutrality and present our analysis of Reichenbach's views. The section is divided into three subsections.

In the first of these we will investigate the relation between von Kries' event spaces and Reichenbach's relative frequencies. After that we will scrutinise the role of objectivity and rationality in the views of Stumpf, von Kries, and Reichenbach. In the third and last subsection we will take a closer look at Reichenbach's transcendental deduction.

### 4.3.1 Event spaces & Relative Frequencies

*- recapitulation von Kries - relative frequencies & elementary event spaces - Reichenbach's claim is weaker than that of von Kries -*

One of the tasks that Reichenbach sets himself in his dissertation is to find an objective interpretation of probability. He starts out from a causal, deterministic worldview (just as von Kries does). In this context Reichenbach creates room for probabilistic considerations by considering systems that are not completely closed, so that there are unknown disturbances.

Let us start by briefly restating von Kries' view. We will do so by considering the example of a coin-toss. In von Kries' approach we first determine the number of macroscopically distinct possible outcomes, which is two in our example (H and T). We can describe the causal structure of any developing physical situation (such as the coin-toss) at different levels of detail. Suppose that we describe the macroscopically distinct outcomes H and T also at a microscopic scale (including details of the experimental set-up). It will be the case that many microscopic descriptions correspond to the same macroscopic state. However, the number of microstates associated with H need not be the same as the number of microstates associated with T. If we repeat the method and describe the experiment at ever more detailed scales the number of microstates associated with H and T may vary as the macrostates split up further and further.

Von Kries' approach rests on the assumption that there is a level of description on which the parts of the event space associated with different outcomes do not split up asymmetrically anymore when the level of description becomes more detailed. Von Kries calls this level the level of 'elementary events'. If we return to the scale of the visibly distinct outcomes (H and T), then we see that every elementary event manifests itself as one of these outcomes. Therefore, each of the visibly distinct outcomes has a number of elementary events associated with it. Von Kries defines the probability of each of the visibly distinct outcomes as the ratio between the number of elementary events with which the outcome is associated to the total number of elementary events associated with all possible outcomes.

Our investigations of von Kries' and Reichenbach's approaches to probability allow us to connect Reichenbach's relative frequencies to von Kries' elementary event spaces.

Suppose that von Kries were to investigate the physical structure of some probabilistic process empirically, by repeating the process often and by looking at the relative frequencies of the different outcomes of the process. A detailed investigation of the physical structure (ie of the process' event space) would require a large number of repetitions.

If after a large number of repetitions the relative frequencies of the outcomes do not fluctuate appreciably anymore, von Kries could possibly conclude that he has stumbled upon approximate probabilities, and could draw further conslusions about the elementary event spaces.

Reichenbach concludes that at this point the relative frequencies in von Kries' experiment approximate his $\phi$, and he may extrapolate its values. We see that Reichenbach's approach to probability entails a weaker claim than the approach of von Kries. Both authors assume the existence of probabilities, but von Kries assumes a physical background in a deterministic framework, whereas Reichenbach need not assume this. He can content himself with the existence of limiting relative frequencies without inquiring into their dynamical background.

### 4.3.2 Rational Expectation

*- objectivity & rationality - Bertrand's paradox shows that the principle of insufficient reason is problematic - objectivity & rationality in Stumpf - ...in von Kries - ...in Reichenbach - Reichenbach confuses objectivity & rationality -*

Reichenbach claims that Stumpf's concept of probability is not rational. In this subsection we will evaluate this claim. We know that in Stumpf's account an important role is played by the principle of insufficient reason. This principle serves to determine which events are equiprobable, and, via this, the principle serves to determine the numbers that go into Stumpf's definition (see above; p49). Reichenbach argues that the use of the principle of insufficient reason renders Stumpf's account of probability of little value for science; Stumpf's probability interpretation cannot yield a basis for rational expectation. We will argue that Reichenbach confuses rationality and objectivity. Reichenbach would be right if he had claimed that Stumpf's account is not fully objective. However, we will see that Reichenbach's actual claim—that Stumpf's account is not rational—is unjustified.

Before we can investigate the role of objectivity and rationality we must be clear about exactly what we mean by objectivity and rationality. In the literature on the topic there are differences of opinion on the right characterization of objectivity and rationality, but for our present purposes it suffices to adopt the following definitions in the context of interpretations of the theory of probability:

*An interpretation of probability theory is **objective** iff it renders the truth-value of probabilistic statements independent of subjective knowledge.*

*An interpretation of probability theory is **rational** iff it enables an agent to make probability statements in such a way that Dutch Books are avoided.*

Reichenbach's qualm with the principle of insufficient reason is that it leads to arbitrariness in the assignment of probability and thus cannot be used in an objective description of reality. Dependent on the way we categorize the possibilities, different verdicts of equiprobability may result, as we have seen in the example of the possible forms of orbits of the comet. Another notorious example is Bertand's paradox: depending on the choice of the independent variable with which we describe the position and orientation of a chord in a circle, we arrive at different answers: the various possible choices do not map linearly to each other, and equal intervals in one choice do not translate to equal intervals in another. This shows that the application of the principle of insufficient reason brings with it an element of subjectivity. It follows that probabilistic knowledge in the account of Stumpf (or Laplace, for that matter) is not objective.

Although the above considerations show that probabilistic knowledge in Stumpf's account is not objective knowledge, they do not problematise a coherent assignment of degrees of probability. It certainly is possible to assign values of probabilities in accordance with Kolmogorov's axioms of probability theory, even though these probabilities are subjective—the values of the probabilities and their coherence is a matter that is completely different from the question of subjectivity versus objectivity. Therefore, Stumpf can avoid Dutch books. If we adopt the modern understanding of rationality then we may conclude that Reichenbach's claim that Stumpf's account is not rational is unjustified.

In the interpretation of probability of von Kries there is also an element of non-objective choice. In von Kries' approach the identification of equally probable events corresponds to a division of phase space in equal volumes, according to a certain choice of the coordinates. This introduces a subjective element, since other choices are possible.

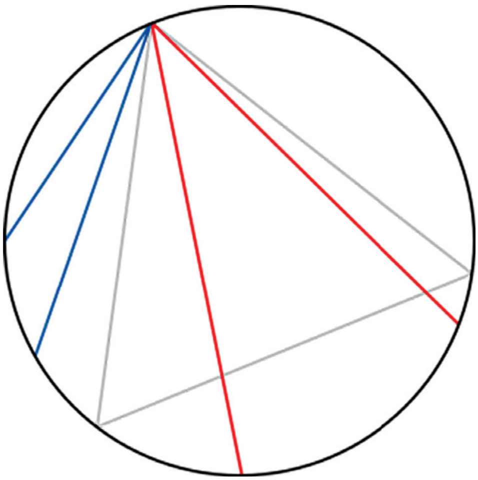

**Figure 4.5:** Suppose we randomly pick a chord which connects two points on the circumference of a circle. The probability that the chord that we pick is longer than one of the sides of a equilateral triangle inscribed in the circle depends on the way in which we make our random pick.

While Reichenbach considers von Kries' account to be an improvement over that of Stumpf, the accounts of von Kries and Stumpf are on a par as regards rationality. Moreover, the above considerations show that von Kries' claim that his approach is incontestably objective is wrong.

In Reichenbach's account the element of choice is minimal, because the values of probabilities are directly coupled to objective relative frequencies that occur in well-defined sequences. There is only the remaining leeway associated with the extrapolation from observed relative frequencies to a definite $\phi$—Reichenbach's general account does not specify the precise form of $\phi$, but only stipulates that *some* $\phi$, with certain mathematical characteristics, *exists*. However, the associated margin of arbitrariness, if any, is of a completely different kind than what we encountered in Bertrand's paradox.

Reichenbach's stance as regards the rationality of Stumpf's account is at odds with that of Stumpf himself. We can see that in the following quote of Stumpf:

> "mathematical probability is only a measure for rational expectation insofar as it is determined by reason alone." ([Stumpf, 1892a], p57)[8].

Stumpf does not say that probability can by itself serve as a basis for rational expectation, but says that probability is a basis for/measure of rational expectation only to the extent to which rational expectation is determined by reason by itself, without any other input.

[8] "[Ebendarum] wird die mathematische Wahrscheinlichkeit nur als Maß der vernünftigen Erwartung bezeichnet, d. h. der Erwartung, soweit sie von der bloßen Vernunft bestimmt ist oder wäre".

We might wonder whether there are no situations in which Reichenbach's account, instead of Stumpf's, does not easily yield a basis for expectation. If we find ourselves in a situation in which hardly any experiments have been done, then Reichenbach's approach does not tell us clearly what probability judgement to make. Stumpf's account has no problems, however. True, Stumpf's account does not yield an objective probability judgement, but it yields a probability judgement nonetheless, which could guide us in our actions.

Imagine a game of chance in which a die is thrown. Before the throw all players must attempt to predict the throw's outcome—not making a prediction leads to certain loss of the game. Is a player adhering to Stumpf's account of probability not better off in practice than an adherent of Reichenbach's? Surely, *any* prediction is better than certain loss. Might we not argue that, while in Reichenbach's account there are no subjective statements, it may be more rational to follow that of Stumpf in some cases?

### 4.3.3 The Quality of the Neokantian Solution

*- why is Reichenbach's approach an improvement of that of Stumpf and von Kries? - Reichenbach's belief in an all-encompassing philosophy of science -*

In this subsection we want to make the point that Reichenbach's valuation of the accounts of Stumpf and von Kries can only be understood in the light of the neokantian goal of his philosophy. Reichenbach's dissertation should be regarded as an attempt to describe the role of probability in providing an all-encompassing philosophy of science.

There are several elements to Reichenbach's neokantian view that might puzzle the modern reader. Firstly, why should we call it an improvement to replace Stumpf's a priori assumption that the principle of insufficient reason is valid with the assumption that Reichenbach's $\phi$ is synthetic a priori? Why is Reichenbach's approach in terms of an unobservable $\phi$ any more convincing than von Kries' approach in terms of a principle of event spaces? It is important to understand that Reichenbach considered it the greatest merit of his dissertation that he was able to deduce $\phi$'s necessity for science within an overarching kantian philosophical system—the central point being his qualification of the existence of $\phi$ as a synthetic a priori, necessary for the possibility of successful scientific reasoning. This is remarkable, especially when one considers the repeated claim that Reichenbach is 'perhaps the greatest empiricist of the 20$^{\text{th}}$ century' ([Glymour and Eberhardt, 2016], [Galavotti, 2009]).

How does Stumpf fare as regards such an all-encompassing philosophy of science? In Stumpf's account the relation between the concept of probability and objective scientific knowledge is not clear. Stumpf speaks about dice and balls in urns as physical systems in which the laws of probability manifest themselves, but also emphasises that probability does not presuppose the validity of a principle of lawful connection (like that of Reichenbach). In Stumpf's view it is not clear how—or even *that*—physical reality manifests itself in probability statements.

It is in the context of the search for an all-encompassing system that von Kries' view is clearly an improvement on that of Stumpf. The event spaces of von Kries are a natural element in a causal picture of the universe in which events can be identified. By embracing a principle of lawful connection, which Stumpf considered to be unessential for probabilistic considerations, von Kries has adopted a specific, encompassing philosophy of science in which event spaces have a natural place. There is, however, no such natural place for von Kries' *principle* of event spaces (which states that equally sized event spaces lead to equiprobability). Because this principle bears no relation to the principle of lawful connection we must regard it as an ad hoc-principle. This makes it clear why Reichenbach believed von Kries' account to be inferior to his own. Where Reichenbach regards his own approach as an advance towards a comprehensive philosophy of science, von Kries' principle of event spaces is an ad hoc principle and is therefore a step in the wrong direction.

Reichenbach's conclusion is as follows. Von Kries's account is an improvement over that of Stumpf in the context of the search for an all-encompassing system, but with the ad hoc adoption of the principle of event spaces von Kries commits the same error as did Stumpf (with his principle of insufficient reason). By contrast, Reichenbach's own approach is a firm step in the direction of a coherent and all-encompassing philosophy of science—a neo-kantian one.

In spite of this kantian framework with its synthetic a priori, Reichenbach's theory of probability is predominantly empiricist. He has no a priori principle of equiprobability, and all numerical probability statements come from extrapolation from observations. The requirement that there is a continuous distribution function $\phi$ (a synthetic a priori according to Reichenbach) does play a role in the explanation of equal probabilities: it explains that possibilities which are 'very close to each other' (like white and black in the probability machine) occur with (almost) equal probability.

# Chapter 5

# Varieties of Frequentism

**The contents of this chapter**

1. Reichenbach
   (a) 1916
   (b) 1920/1925
   (c) 1935/1949
2. von Mises
   (a) "First the Collective, then Probability"
   (b) Axioms
3. Comparison

We witnessed in part I the ascent of an interpretation of probability in terms of the frequencies with which events are observed. Inchoate versions of this interpretation—called *frequentism*— can be found in the works of Bernoulli and Laplace. In the 19$^{\text{th}}$ century different varieties of frequentism were around in the works of Mill, Ellis and Venn (an appreciation of these differences can be found in [Verburgt, 2014]). In the early 20$^{\text{th}}$ century frequentism was further developed into a sophisticated relative frequency interpretation of probability, bearing a logical positivist stamp. Or so the standard-story goes[1].

We want to show in this chapter that the received view on the development of frequentism should be amended in an important respect. The fully-fledged frequentism of the early 20$^{\text{th}}$ century is not as unified as the received view

[1] In this introduction we will encounter the writings of Russell in which he defends this account [Russell, 1948]. See, for a more recent statement of the same point, [Gillies, 2000].

has it. We will analyse the work of two scholars from the early 20$^{\text{th}}$ century who share the classification frequentist and we will find that these views differ subtly but significantly.

## 5.1 Reichenbach

### 5.1.1 1916

Let us begin by briefly reiterating the central points of Reichenbach's 1916 interpretation of probability. We saw that, according to the early Reichenbach, all the events in the universe are ordered according to causal laws. We also saw that this does not necessarily lead to predictability: the assumption that everything strictly follows causal laws does not imply that we can make perfectly accurate predictions. Even if we accurately knew the initial conditions of some developing physical situation, this knowledge would not allow us to predict its factual development. Such uncertainty comes about, says Reichenbach, because there are no closed systems (ie causally independent regions) in the universe. Because there are no closed systems there are always infinitely many factors in any physical situation that potentially influence the way in which it develops.

The irregularities causing this epistemological uncertainty manifest themselves when we make repeated measurements. Whenever a scientist repeats a measurement of the same physical variable many times, the distribution of the measurement-results is not trivial (as it would be in a universe in which everything is perfectly regular because then we would get the same result over and over). Probability, in Reichenbach's view, is an idealised description of this non-trivial distribution. The relation between probability and the observed, non-trivial distribution is the following: from the perturbed causal structure of any developing physical situation emerge relative frequencies for each of the possible outcomes of the situation. The probability that a scientist's measurement on such a physical system will have a certain result corresponds to the relative frequency of this result in a sequence of measurement-results that would come about if the number of repetitions of the measurement were infinite. This relative frequency, pertaining to an infinite sequence, cannot be observed and is therefore hypothetical. According to Reichenbach, the relative frequencies which are actually observed are approximations of the hypothetical, limiting relative frequencies resulting from infinitely many repetitions of a measurement.

The unobservable, ideal distribution of these limiting relative frequencies is described by what Reichenbach calls the probability function ($\phi$). According to Reichenbach, accepting that this probability function exists is a synthetic a priori in the kantian sense: it is necessary to assume this to get a probabilistic approach to physical reality off the ground. The scientist's standard practice of taking actual, observed relative frequencies as representative of probabilities, presupposing that such an extrapolation can be done in principle, is thus justified.

Where earlier probability interpretations made use of a principle of insufficient reason Reichenbach rejects this principle and replaces it with a condition on the distribution function: relative frequencies of measurement-outcomes in scientific experiments must approximate a Riemann integrable probability function ([Reichenbach, 1916], p83).

We end this subsection by noting that there are *two* assumptions in Reichenbach's early view on probability which he (in 1916) takes as synthetic a priori in a kantian sense. Both these two assumptions, Reichenbach reasons, are a necessary ingredient of scientific knowledge. It must be assumed that the following two principles are valid:

1. the principle of lawful connection
2. the principle of lawful distribution

The functioning of these principles is illustrated in fig4.2 (p53). The principle of lawful connection connects causes with their effects (A to B, A' to B' etc) and the principle of lawful distribution expresses a connection between either various possible initial states (A, A' and A") or various possible endstates (B, B' and B").

### 5.1.2 1920/1925

In 1920 Reichenbach's work 'The Theory of Relativity and A Priori Knowledge' [Reichenbach, 1920] is published. In this work Reichenbach attempts to reconciliate kantianism with Einstein's relativity. We will revisit this work of Reichenbach in a later chapter and here only mention his revision of the kantian synthetic a priori without explaining its relation with the theme of Reichenbach's 1920 book. Reichenbach proposes to adjust his earlier neokantian approach in two respects. Most importantly, Reichenbach's a priori becomes relative to a specific scientific context (see p28).

Reichenbach lists a number of principles that he regards as a priori in this relativised sense. This list contains several of the principles that Kant regarded as synthetic a priori—such as the existence of space, time, and a principle of lawful connection. Reichenbach specifically mentions his 1916 work as a reference for another principle that is on the list of relativised a priori principles: the principle of lawful distribution[2].

[2]Reichenbach argues that it is essential for science to coordinate mathematical concepts to

In 1925 Reichenbach writes the article "The Causal Structure of the World" [Reichenbach, 1925] in which he gives an analysis of the role of the concept of causation. He there realises that for any sequence of events that can be described with the aid of a principle of lawful distribution it is not necessary to assume the existence of causal relations in order to be able to describe observed correlations—it is not necessary to assume the validity of a principle of lawful connection in order to make scientific knowledge possible. The principle of lawful distribution suffices to describe any sequence of events in terms of mathematical concepts. Laws involving such concepts need not consist in a one-to-one deterministic correspondence relation between events, but may be stochastic in nature.

The result of Reichenbach's 1925 analysis that is most relevant for us is its conclusion that a valid physics description may be given either in terms of causation and probability or in terms of probability alone[3].

### 5.1.3 1935/1949

In 1935 Reichenbach's most comprehensive work on probability is published. It was originally published in German under the title "Wahrscheinlichkeitslehre" and translated into English in 1949, resulting in "The Theory of Probability" [Reichenbach, 1935]. In this work Reichenbach's approach to the concept of probability seems very different from his 1916 approach, but in the course of this subsection we will see that there is nevertheless an important similarity between Reichenbach's early and later approaches.

#### Formal Definition

The first deviation from his 1916 probability concept is that Reichenbach starts his analysis of probability not by recognising probability in a causal context—as he did in his dissertation—but by proposing a more general scheme in

physical objects. It is therefore the task of the philosopher of science to attempt to discover the axioms which lie at the basis of this coordination. In this context Reichenbach states that "for a particular area of physics, for the theory of probability, such an analysis [the quest for coordinative axioms] has already been carried out by the present author. It has led to the discovery of an axiom that has a foundational meaning for physical knowledge, and as a principle of distribution it can be put besides the law of causality as a principle of coordination." ("Für ein Spezialgebiet der Physik, für die Wahrscheinlichkeitsrechnung, könnte eine derartige Analyse vom Verfasser bereits durchgeführt werden. Sie führte zur Aufdeckung eines Axioms, das grundsätzliche Bedeutung für die physikalische Erkenntnis besitzt, und als Prinzip der Verteilung neben das Kausalitätsgesetz, als Prinzip der Verknüpfung gesetzt wurde.") ([Reichenbach, 1920], p75; p72 in the German original)

[3]In his later writings, Reichenbach repeatedly stresses the fact that his conclusion that mathematical models of mechanics may be stochastic even at a microscopic level predates Heisenberg's uncertainty principle by two years.

which degrees of probability are descriptive of relations between events that are members of different classes. Reichenbach's 1935 approach starts with a certain type of event ($x$). We are interested in the probability ($p$) that some other event ($y$) is of the same or of a particular different type.

We must recognise a class $A$ (the reference class) to which events of type $x$ belong. We must also identify a class $B$ (the attribute class). Reichenbach introduces a kind of logical implication ($\stackrel{p}{\rightarrow}$) that is different from the generic logical implication ($\rightarrow$) in that it brings with it a degree of expectation that is not certainty ([Reichenbach, 1935], p45[4]). A statement of probability can be formulated as follows:

$$x \epsilon A \stackrel{p}{\rightarrow} y \epsilon B \tag{5.1}$$

A probability statement in this sense is an implication between statements concerning a class membership of the elements of certain given classes: membership of $A$ implies membership of $B$ with a probability $p$. However, in this formulation there is not yet information about the relation between the events. We can express this information by adding indices to the events $x$ and $y$. Reichenbach's probabilistic implication[5] holds only between events that have the same index[6].

Generally, statements of Reichenbach's probability express a relation between a sequence of pairs of events ($\{x_i, y_i\}$) and two classes $A$ and $B$. We can formulate an improved statement of Reichenbach's probability:

$$\forall i : x_i \epsilon A \stackrel{p}{\rightarrow} y_i \epsilon B. \tag{5.2}$$

The above expression can be abbreviated as

$$A \stackrel{p}{\rightarrow} B \tag{5.3}$$

As an example we may consider a throw with a six-sided die. Let $A$ represent the class of throws with a cubical die; $x$ a particular throw, $y$ the event of the die's landing, and $B$ the class of outcomes 1. Formula 5.2 then says the following: if $x$ is a throw with a cubical die then $y$ belongs, with a certain probability, to the outcome-is-1-class.

[4] Reichenbach writes that the probability statement about the die-throw "has the form of a relation. It is not asserted unconditionally that face 1 will appear with the probability $\frac{1}{6}$; the assertion, rather, is subject to the condition that the die be thrown. If it is thrown, the occurrence of face 1 is to be expected with the probability $\frac{1}{6}$; this is the form in which the probability statement is asserted."

[5] Vide *non* licet: "Reichenbach's probability".

[6] There exists a 1-1 correspondence between the $x$'s and $y$'s because we look at pairs of events. The indices reflect a certain order in the events. This order may be (but need not be) temporal.

It may also be the case that $x_i$ and $y_i$ refer not to attributes of different events, but to different attributes of the same event. An example of such a case is the investigation of the probability that a neonate is a boy. Now both $x_i$ and $y_i$ refer to a newborn, but only $y_i$ refers to newborns being a boy. In this situation Reichenbach calls the probabilistic implication an internal probability implication ([Reichenbach, 1935], p48; see fig5.1). It should be noted that Reichenbach's concept of probability corresponds to what in modern parlance is called *conditional* probability.

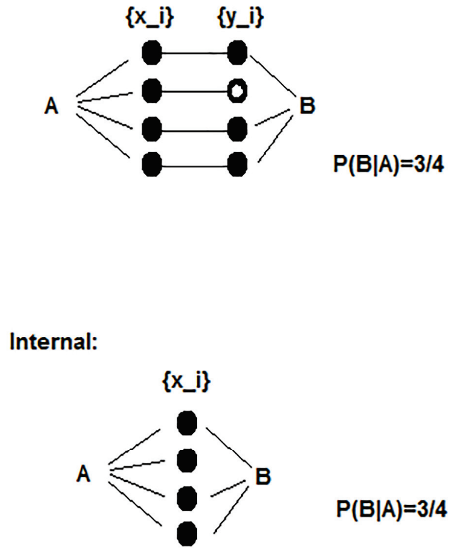


**Figure 5.1:** Reichenbach's 1935/49 probability illustrated: Probability describes a relation within an infinite sequence of pairs of events ($\{x_i, y_i\}$). Each of these events can be a member of one of two classes ($A$ and $B$). If the events under consideration belong to a sequence of individual events (and not a sequence of pairs of events) we consider the probabilistic relation as a special case of the former—an *internal* probability relation.

### Frequency Interpretation

After providing his readers with an axiomatic formal symbolic framework[7] Reichenbach proposes an empirical interpretation of probability: a frequency interpretation. Central to this move of Reichenbach's is that he interprets probability as "the limit of a frequency within an infinite sequence" ([Reichenbach, 1935], p68).

A statement of probability, in Reichenbach's 1949 approach, is an implication between statements concerning class membership of the elements of certain given classes. We consider a sequence of $n$ pairs {x,y}: $N^n(A)$ is the relative frequency with which an $x$ occurs that is in class A; $N^n(B)$ is the relative frequency with which a $y$ occurs that is in class B. The frequency of $y \epsilon B$ relative to $x \epsilon A$ ($N^n(A, B)$) can therefore be defined as follows ($N^n(A, B)$ expresses the number of times that in a sequence of $n$ pairs ($\{x_i, y_i\}$) an $x$ that is an element in class $A$ is coordinated to a $y$ that is an element in $B$):

[7] [Reichenbach, 1935], p54: The axioms are I. Univocality; II. Normalization; p58: III. Addition; p62: IV. Multiplication.

$$N^n(A,B) = \frac{N^n(A \wedge B)}{N^n(A)} \tag{5.4}$$

This frequency interpretation provides us with an explicit[8] meaning of the symbol $\xrightarrow{p}$. It does so by equating $N^n(A,B)$, in the limit of $n$ going to infinity, to the probability ($P(B|A)$) that $y$ co-occurs with $x$ in an infinite sequence of pairs of events:

$$P(B|A) = \lim_{n\to\infty} N^n(A,B)^9 \tag{5.5}$$

"Probability", Reichenbach states, "must be regarded as a three-term relation between two classes and a sequence of pairs".

Reichenbach shows that "all [probabilistic] axioms are satisfied tautologically and are strictly, not only approximately, valid even before transition to the limit" ([Reichenbach, 1935], p76).

As an example of an application of Reichenbach's probability concept we consider a coin-toss. We can apply Reichenbach's probability concept to the coin-toss in at least three different ways. Say we ask for the probability of throwing heads in the following situation:

1. We toss $n$ times with the same coin and get the following result:

$$\underbrace{tHtTtTtHtTtTtHtHtHtTtTtHtH...}_{n\text{ tosses}}$$

   t=toss
   H=outcome is heads
   T=outcome is tails

   We observe that t is followed by H 7 out of 13 times. Reichenbach's method can be applied by defining A as the class of throws (comprising all instances of t) and B as the class of outcomes H. Then $N^n(A,B) = \frac{N^n(A\wedge B)}{N^n(A)} = \frac{7}{13}$. We can then say that the probability of finding H given that the coin has been tossed is $\lim_{n\to\infty} N^n(A,B)$.

2. Alternatively, it may be the case that we perform every separate toss with one of several different coins (in this case only two) so that $t$ is

[8] Cf [Reichenbach, 1928], p93 for a description of the difference between implicit and explicit definitions. The difference was introduced by Hilbert and elaborated upon by Carnap.

[9] This gives us the probability that some event is a $B$ on condition that it is paired with an $A$-event.

either $t_1$ or $t_2$:

$$\underbrace{t_1Ht_1Tt_2Tt_1Ht_2Tt_2Tt_2Ht_1Ht_1Ht_2Tt_2Tt_2Ht_2H...}_{n \text{ tosses}}$$

Again we see that 7 out of 13 outcomes are H. But now that the $t's$ are no longer the same, Reichenbach's method allows us to refine our probability statements. We no longer ask for the probability of finding H given that the coin has been tossed without further specification, but rather we ask for the probability of finding H given that a particular coin has been tossed ($t_1$ or $t_2$). In this case the class A consists only of instances $t_1$ or of instances $t_2$.

For example, we may use Reichenbach's probabilistic approach to calculate the probability of the outcome H when coin $t_2$ is tossed[10]:

$$P(H|t_2) = \lim_{n\to\infty} N^n(H|t_2)$$

3. Yet another possibility is that we see $n$ coins lying on a table with either heads (H) or tails (T) facing upwards:

$$\underbrace{HTTHTTHHHTTHH...}_{n \text{ coins}}$$

In this situation we can apply Reichenbach's method by recognising that every outcome (every instance of H or T) is preceded by an event of the recognition of a coin (call it $r$). We thus end up with the following:

$$\underbrace{rHrTrTrHrTrTrHrHrHrTrTrHrH...}_{n \text{ coins}}$$

the resulting situation is similar to those related earlier, depending on whether all coins (all instances of $r$) belong to the same class.

[10] If we assume that the relative frequency observed so far is representative of a limiting relative frequency within a hypothetical infinite sequence of observations then we may say that $P(H|t_2) = \frac{3}{8}$.

If we compare this list with fig5.1 then we may recognise that in the first and second situation listed above probability expresses a relation between two classes and a sequence of pairs (every pair combines a throw with an outcome state), whereas the third situation expresses what Reichenbach would call ‘internal probability’.

In this context Reichenbach distinguishes two different limit-interpretations ([Reichenbach, 1935], p344). Firstly, we may take the limit in the definition of probability as an instance of ‘weak convergence’:

$$\forall\delta : \forall n_0 : \exists n : (n > n_0) \wedge (|f^n - p| < \delta), \tag{5.6}$$

where $f^n$ is defined with respect to the classes $A$ and $B$. If $n$ members of class $A$ are inspected, then $f^n$ is the number of these $n$ elements that are also a member of $B$. Translated into words this first interpretation means: however small we choose $\delta$ and however large we choose $n_0$, there is an element $f^n$ beyond $f^{n_0}$ that is situated within the interval $p \pm \delta$.

The second limit-interpretation defines probability in terms of ‘strong convergence’:

$$\forall\delta : \exists n_0 : \forall n : [(n > n_0) \Rightarrow (|f^n - p| < \delta)], \tag{5.7}$$

for every $\delta$, however small, there is an $n_0$ such that, from $f^{n_0}$ on, all $f^n$ remain between the limits $p \pm \delta$. $f^n$ not only reaches a value between certain limits, it also stays within these limits. Reichenbach states explicitly (p344) that his early (1916) approach was a version of the weak limit interpretation[11]. He distances himself from that interpretation and states that he has come to realise that probability is better defined in terms of the strong limit (formula 5.7).

Reichenbach addresses the issue of which interpretation to choose (either in terms of weak or strong convergence; respectively involving a weak or a strict limit) with a number of arguments. Firstly, one of the foremost arguments against choosing an interpretation in terms of strong convergence (the interpretation Reichenbach now wishes to defend) was that infinite sequences of events cannot be observed. However, Reichenbach remarks that weak-limit statements cannot be empirically verified either; the non-verifiability argument should therefore not be a ground for preference of an interpretation in terms of a weak limit over an interpretation in terms of a strict limit. Formally, Reichenbach's argument rests on the fact that any weak limit is logically equivalent to the negation of a strict limit:

[11] Reichenbach writes about a situation in which N runs of an experiment have yielded a certain distribution of relative frequencies: “...Was mit weiter wachsendem N geschieht, ob die Abweichung wieder größer wird oder nicht, darüber können wir nichts aussagen...” ([Reichenbach, 1916], p136).

$$\begin{aligned} \neg[\forall\delta : \forall n_0 : \exists n : (n > n_0) \wedge (|f^n - p| \leq \delta))] \equiv \\ \exists\delta : \exists n_0 : \forall n : [(n > n_0) \Rightarrow \neg(|f^n - p| \leq \delta)]. \end{aligned} \tag{5.8}$$

This equivalence of a weak limit to the negation of some other strict limit expresses the fact that for $f^n$ to take on a particular value within certain limits, say, $p \pm \delta$, it must not be the case that $f^n$ keeps on differing more than $\delta$ from $p$. As Reichenbach explains, the intimate relation between weak and strict limits demonstrates that the interpretation of probability in terms of the weak limit does not have advantages with respect to the possibilities of empirical verification and falsification (p346)[12].

### Posits

In a discussion of the problem of single-case probabilities (single-case probabilities of events are probabilities that refer to individual events [Reichenbach, 1935], p366) Reichenbach connects it with another fundamental problem within the philosophy of science—the problem of induction. Inductive inferences (in natural science) consist in an extrapolation of observed regularities into universal regularities. As this extrapolation proceeds from a finite sample of physical systems to an infinitely sized set of physical systems, the inference cannot be logically justified on the basis of observations. The same issue of justification on the basis of observations plays a role in all statements of probability. In fact, any probabilistic statement in the frequency-interpretation *is* an inductive statement.

Reichenbach proposes to solve three problems concerning his concept of probability (limiting relative frequencies being unobservable; the nature of single-case probabilities and the induction problem) in one fell swoop: he proposes to interpret a statement regarding the probability of some event as a *posit* regarding the relative frequency of this event within a particular class of similar events.

Reichenbach likens making such a posit to placing a bet in a horse-race ([Reichenbach, 1935], p373). We do not actually know that the horse on which we bet will win with a certain frequency but we behave as if we do know that. About the justification of making such posits Reichenbach argues: "The rule of induction is justified as an instrument of positing because it is a method of which we know that if it is possible to make statements about the future

[12] In this context, Reichenbach mentions the measure theory of Kolmogorov ([Reichenbach, 1935], p121). In the original German edition Reichenbach treats Kolmogorov's work somewhat more extensively when he compares Kolmogorov's axiomatisation of probability theory with his own. There Reichenbach says that what he himself calls probability, Kolmogorov calls relative probability.

we shall find them by means of this method" ([Reichenbach, 1935], p475). The inductive inference that is involved in Reichenbach's posits is therefore conditional: $a \Rightarrow (b \Rightarrow c)$—if (universal) natural regularities exist ($a$) then the use of induction ($b$) will lead us to their discovery ($c$). So we act as if it were certain that there are universal regularities. Without this assumption, Reichenbach says, it would not be possible at all to make scientific predictions.

## 5.2 Richard von Mises

### First the Collective, then Probability

Not long after Reichenbach had published his dissertation the Austrian mathematician Richard von Mises published his version of frequentism. Von Mises' first publication on the topic, an article with the title "Grundlagen der Wahrscheinlichkeitsrechnung" ("Foundations of Probability Theory"), appeared in 1919. The work for which von Mises has become well-known is his "Wahrscheinlichkeit, Statistik und Wahrheit", which appeared in 1928 and was translated into English in 1939 as "Probability, Statistics and Truth". Von Mises published a mathematical elaboration of his view in 1964 under the title "Mathematical Theory of Probability and Statistics" [von Mises, 1964].

Von Mises' account of probability can be characterised in one short sentence: "first the collective, then probability"[13]. This sentence shows us that von Mises' interpretation begins with what von Mises calls 'the collective' — a sequence of 'aggregate phenomena' or 'repetitive events' ([von Mises, 1964], p1). In von Mises' account degrees of probability are defined as relative frequencies within collectives. According to von Mises the theory of probability is a physical theory about sequences of outcomes of a finitely repeated but in principle infinitely repeatable process.

**Figure 5.2:** Richard von Mises (1883—1953). Image taken from the public domain.

### Axioms

To illustrate von Mises' collectives we consider the simple example of an experiment in which a coin is tossed many times. What characteristics must the sequence of results have in order for us to be justified in believing that the sequence is one of von Mises' collectives?

With respect to the sequence of results two classes are relevant; every element in the collective either belongs to class H or it belongs to class T. For such a sequence of elements to be called a collective the distribution within the sequence of elements which belong to the relevant classes must obey the following two conditions:

**I)** The relative frequency of both H and T must converge to a certain constant

[13] "Erst das Kollektiv, dann die Wahscheinlichkeit." ([von Mises, 1928], p21)

value as the sequence becomes very long.

**II)** For any subsequence, arrived at by a suitable place selection (a place selection is the selection of an infinite subset of elements that appeared in the original sequence), the relative frequencies of H and T are the same as that in the original sequence.

The above conditions are not limited to the example of the coin-toss. In fact, any sequence to which von Mises wishes to apply his theory of probability must obey these conditions. The number of relevant classes may vary (eg in the case a cubical die is thrown this number is six).

The axioms can be stated formally as follows. Let $K$ be a collective consisting of $n$ elements some of which have the property $\xi$ and let $K'$ be a suitable subsequence of $K$:

**I)** $\lim_{n\to\infty} relf_n^{\xi}(K)$ exists

**II)** $(K' \subset K) \to (\lim_{n\to\infty} relf_n^{\xi}(K') = \lim_{n\to\infty} relf_n^{\xi}(K))$

The question that naturally follows from this axiomatisation is how a suitable place selection should be characterised. Von Mises introduces for this a new principle: the principle of the impossibility of a gambling strategy. The idea here is as follows. A place selection specifies a sequence whose elements form an infinite subset of the elements in one of von Mises' collectives. The selection must be independent of the values of the elements themselves. Suppose now that the elements of this new sequence are the outcomes of some gambling game (eg a sequence of red and black segments in a game of roulette). If the new sequence is the result of a place selection that is 'suitable' then there would be no possible betting strategy for the game that would lead to net success in the long run (ie when the game is often repeated; the sequence becoming very long).[14]

The impossibility of a gambling strategy is not a directly observable property[15] and therefore von Mises has to assume at the outset that distributions *can* be such that a gambling strategy is impossible. The demand that relative frequencies should be taken in a sequence in which a gambling strategy is impossible is an additional demand.

[14]von Mises' randomness plays a role in von Mises' view on probability that seems to be similar to the role the principle of insufficient reason plays in the views of Stumpf and Laplace.

[15]This situation is the same for any universal statement in physics as any such statement rests on an inductive inference. For von Mises the inability to find a favourable betting strategy in a finite sequence is evidence from which, via induction, the impossibility of a gambling strategy can be inferred.

## 5.3 Comparison

### Letter to Russell

The content of the previous sections shows us that Reichenbach's view on probability differs from von Mises' view. In a letter that Reichenbach sent in 1949 to Bertrand Russell (reprinted in [Reichenbach and Cohen, 1978], pp405-411) Reichenbach comments on this difference. In this section we will analyse Reichenbach's comments. This analysis gives us the opportunity to revisit the key concepts within and identify the differences between the frequentist approaches of Reichenbach and von Mises and shows us which elements of the frequentist approach are most valued by Reichenbach. The letter is a response of Reichenbach to a book Russell had written the year before: "Human Knowledge" ([Russell, 1948]). In this book Russell speaks of 20$^{\text{th}}$ century frequentism as the 'Mises-Reichenbach theory'. Reichenbach responds to this as follows:

> "I have read with great pleasure your book on Human Knowledge. [...] I was surprised to find myself hyphenated to von Mises ... - as much surprised, presumably, as he. You even call my theory a development of that of von Mises. I do not think this is a correct statement. My first publication on probability [Reichenbach, 1916], which is earlier than Mises' publications, has already a frequency interpretation and a criticism of the principle of indifference, although later I abandoned the Kantian frame of this paper.
>
> [...] Mises' merit is to have shown that the strict-limit interpretation does not lead to contradictions and, further, to have provided a means for the characterization of random sequences. I then could show that my earlier frequency interpretation (which was weaker than a strict-limit interpretation) in combination with Bernoulli's theorem leads to the limit interpretation and thus took over this interpretation. But my mathematical theory is more comprehensive than Mises' theory, since it is not restricted to random sequences; furthermore, Mises does not connect his theory with the logical symbolism. And Mises has never had a theory of induction or of application of his theory to physical reality."[16]

In this letter a number of claims is made. We will analyse the following of these claims:

[16] Reprinted in Reichenbach 1978, vol II, p410.

1. Reichenbach's early interpretation is weaker than a strict-limit interpretation.

2. Mises does not connect his theory with the logical symbolism.

3. Reichenbach's mathematical theory is more comprehensive than that of von Mises.

4. Von Mises does not explain how his theory should be applied to physical reality.

[1] Reichenbach claims that his 1916 frequentist interpretation is weaker than the strict-limit interpretation. We can confirm the validity of this claim if we take another look at Reichenbach's 1916 view. We saw in the context of formulae 5.6 and 5.7 that Reichenbach in 1916 opts for the weaker variant of the limit statement that forgoes any claim as to the stability of convergence towards the limit (see p58). The interpretation is weaker because it involves a weaker claim about experimental results (in that it entails less of a restriction of those results).

[2] Reichenbach states that von Mises does not connect his theory with the logical symbolism. That is indeed an important characteristic of von Mises' approach which differentiates it from Reichenbach's approach. Von Mises says that his probability is a physical characteristic. Von Mises does not explain this any further or show how this physical characteristic fits in with his definition of degrees of probability as limiting relative frequencies.

The situation is very different as regards Reichenbach. To interpret the concept of probability Reichenbach introduces the logical symbol $\xrightarrow{p}$. Reichenbach's probabilistic axioms show how this new symbol behaves in a logical context. The connection of $\xrightarrow{p}$ to the observations of the scientist follows when Reichenbach interprets $\xrightarrow{p}$ in terms of relative frequencies of events.

[3] Reichenbach also claims that his mathematical theory is more comprehensive than that of von Mises. We have seen that Reichenbach's probability is not restricted to random sequences (see above; p73). Even in situations in which there is no randomness Reichenbach still speaks about probability. To put it differently, Reichenbach can calculate degrees of probability of attributes in periodic sequences (in which there are by definition attributes which have stable relative frequencies). This degree of freedom in Reichenbach's approach distinguishes it from von Mises' approach. It seems, then, that Reichenbach is right when he writes "my mathematical theory is more comprehensive". Von Mises' probability concept cannot be applied to non-random sequences whereas Reichenbach's concept can.

[4] Particularly the last claim Reichenbach makes, that "Mises has never had a theory of induction or of application of his theory to physical reality" is of interest to us. The question underlying Reichenbach's claim is the following: if randomness and probability are defined in terms of infinite sequences then how does probability relate to anything observable? Von Mises does not explain the relation between his probability and physical science. In Reichenbach's approach, on the other hand, the idea of the *posit* appears to allow us to extrapolate observed relative frequencies to limiting relative frequencies and as such to connect the concept of probability with reality as it is observed by the scientist.

This point, about the connection between Reichenbach's concept of probability and physical reality as it is observed by the scientist will be elaborated in the next chapter, as this connection plays an essential role in linking Reichenbach's early to his later work.

# Chapter 6

# Traces of Transcendentality

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6. More Transcendental Traces?

In contemporary philosophy of science there is a standard story[1] about the development of Reichenbach's philosophy of science. According to this standard story Reichenbach wrote his 1916 dissertation in a thoroughly neokantian

[1] Early versions of this story can be found in the preface of [Friedman, 1983] and in chapter three of [Friedman, 1999] (p63 ff). Ryckman endorses this story and speaks of Reichenbach as 'surrendering' kantian elements of his philosophy ([Ryckman, 2003], p179. See also [Ryckman, 2005], p39). [Eberhardt and Glymour, 2008] speak of "the end of [Rei-

spirit. In 1920, so the story continues, Reichenbach showed in his 'Relativity Theory and A Priori Knowledge' that Kantian philosophy cannot accommodate the then recently developed theory of relativity and that its central ideas, and in particular the notion of synthetic a priori had to be radically changed. Later that same year an exchange of letters with Schlick convinced Reichenbach that Kantianism had in fact been completely refuted, and that there was no longer a place for the kantian synthetic a priori at all. From then on, Reichenbach replaced references to synthetic a priori principles with references to 'conventions'. This change signalled Reichenbach's conversion to logical positivism. In his 'Probability Theory' (German original published in 1935) and his 'Experience and Prediction' (1938), the standard story concludes, we see the mature Reichenbach who clearly is an all-out logical positivist.

In this chapter[2] we will argue, however, that there is an underlying continuity in Reichenbach's philosophy to which the standard story does not do justice. To show this, we will start with a recapitulation of Reichenbach's views of 1916. After that we will investigate Reichenbach's 1920 analysis of the relation between relativity theory and a priori knowledge. We will see that Reichenbach, far from rejecting kantianism as obsolete, keeps on believing in the validity of its general ideas and only proposes to emend the kantian approach slightly so as to prevent it from contradicting relativity theory; in particular, Reichenbach sees an important role for the constitutive synthetic a priori. We will also investigate the correspondence between Reichenbach and Schlick. We will see that the 'conventions' with which Reichenbach replaced the synthetic a priori as a result of this exchange do not have the exact same status as those of Schlick: they retain the flavour of the constitutive a priori. After this we will focus on a note Reichenbach wrote in 1927 in which he gives us an assessment of his earlier ideas—particularly those in his 1916 dissertation. The content of this note lends further support to the idea that there is a significant continuity in Reichenbach's thought, and that it is incorrect to maintain that Reichenbach developed into a 'standard' logical positivist.

After the 1927 note we will discuss Reichenbach's 1935 'Theory of Probability'. In this work Reichenbach analyses the relation between probability and induction and comes up with the so-called straight rule of induction. We

chenbach's] Kantian views" after 1920. In recent scholarship we see a growing awareness of kantian traces in Reichenbach's later work (for example, [Stölzner, 2011] wonders whether Reichenbach's later probability interpretation is not "...at bottom transcendental" (p13) and [Glymour and Eberhardt, 2016] speak of a "remaining allegiance to Kant" (par2.2).). However, the standard story has not been laid to rest (as attested by, among others, [Padovani, 2011] and [Heis, 2013]). It will be our task in this chapter to pinpoint the exact locations of the kantian elements in Reichenbach's later work and show that these elements are traces of Reichenbach's earlier kantian views.

[2]The conclusions reached in this chapter have been put forward in different form in an article by this dissertation's author and his supervisor ([Benedictus and Dieks, 2014]).

will argue that also here traces of Reichenbach's kantian leanings can be identified. In particular, Reichenbach's characterisation of certain premises in his inductive inferences as *posits* are recognisable as traces of his earlier neokantian philosophy. Turning to Reichenbach's work 'Experience and Prediction' (1938), we will find that he uses a scheme that is reminiscent of inference to the best explanation (IBE) in order to justify the belief that theoretical entities in accepted scientific theories can be regarded as corresponding to objectively existing physical entities. We will compare this Reichenbachian argument to the transcendental deductions that he used earlier. We will conclude that Reichenbach's specific form of IBE can be seen as comparable to his earlier transcendental deductions, and as fulfilling the same functional role. Reichenbach's mature works can therefore be said to retain traces of the kantianism of his younger days as well.

In this chapter we limit ourselves to Reichenbach's work up until his 1949 translation of 'The Theory of Probability'—which is commonly regarded as containing his maturest views on probability. This limitation is not problematic because Reichenbach's work in this period is representative for his oeuvre, and suffices to make our point of an underlying continuity.

## 6.1 (1916) Early Frequentism

Let us briefly summarise the developments in Reichenbach's philosophy of science that we have so far encountered. Reichenbach in 1916 argued that in order to be able to cope in a scientific way with disturbances and errors, we have to assume that with any scientific experiment a Riemann integrable probability function is associated. Reichenbach showed that, once this assumption has been made, the equiprobability of certain outcomes of the experiment follows deductively (as had been argued earlier by Poincaré). With the latter deduction Reichenbach claimed to have finally provided a solution to a problem that had vexed interpreters of probability theory long before, and had become an explicit focus of attention since Laplace formulated his classical definition of probability—namely the problem of whether probability judgements can be given an objective basis.

At the time of Reichenbach's dissertation an essential role in most work on probability was played by the principle of insufficient reason. Reichenbach rejected this principle for the reason that it is subjective, but the concept with which Reichenbach replaced it has its own problematic implications. Reichenbach did not prove that his probability function exists in an ordinary physical or mathematical sense, but rather argued that the existence of this probability function must be accepted a priori as the assumption of its existence is a necessary precondition for the possibility of doing science; the argument thus consists in a kantian transcendental deduction. The details of this deduction have been discussed in a previous chapter. What is important here is that Reichenbach in 1916 argues that the existence of a probability function is a kantian synthetic a priori. In his dissertation Reichenbach extended traditional kantianism by adding the principle of the probability function to the principle of causality, and explicitly embraced the tenets of kantian philosophy.

## 6.2 (1920) Relativity Theory & A Priori Knowledge

It should therefore not surprise us that it was the validity of kantian transcendentalism in the context of then-current scientific developments that formed the subject-matter for Reichenbach's first major publication after his dissertation. The development of Einstein's theory of general relativity, in the first two decades of the 20$^{\text{th}}$ century, seemed to be a severe blow for kantianism. Indeed, general relativity showed that Kant's idea that it is a necessary (synthetic a priori) truth that the space we inhabit has a euclidean geometry is not correct. In his 1920 habilitation thesis Reichenbach therefore set out to explore in depth the seriousness of the conflict between relativity theory and kantian philosophy.

Now, thinking about physical geometry and the dynamics of material bodies soon shows that there is an intricate relationship between geometry, gravitational force, and observation. Suppose that observations (astronomical or otherwise) provide us with a body of evidence regarding the kinematics of massive bodies. General relativity explains these observations by postulating that spacetime is curved in a particular way. According to this theory massive objects that move through curved spacetime will follow the shortest spacetime path (they will follow geodesics). However, logic tells us that in principle there are other interpretations possible: instead of the just-mentioned general relativistic account, we might say that spacetime is not curved at all (or curved in a different way) and that the observational data are the results of certain gravitational forces that are at work. In this case the objects do not follow geodesics but are deflected from them by the action of a force. In other words, mere logic suggests that the observer is not absolutely bound by one single theoretical scheme: she can explain her observations by choosing either a curved geometry for the spacetime around her; she can postulate the existence of a gravitational force; or she might employ a mixture of these strategies (choose a particular geometry with complementary gravitational force). In all these cases scientifically bona fide theories can be formulated, some with a Euclidean geometric basis and some with a non-Euclidean geometry; and all making the same empirical predictions. Therefore, even if the universe's geometry is not supposed to be euclidean, science is still possible because observations can be interpreted in a systematic, lawlike way. This argument was more fully developed in Reichenbach's 1928 'Philosophy of Space and Time'.

### 6.2.1 The Synthetic A Priori Becomes Relativised

So there indeed is a problem with traditional kantianism: it is problematic to maintain euclidean geometry as a necessary precondition for the scientific study of space. Reichenbach argues, however, that general relativity does not invalidate the kantian approach in toto. General relativity does not render the transcendental question obsolete, but suggests that it has not been posed and answered in the right way. Moreover, general relativity shows us how to adjust the kantian approach so as to accommodate not only Einstein's theories of relativity, but also future developments in science that could yield a contradiction with kantian synthetic a prioris. According to Reichenbach it remains true, even in general relativity, that science can only move forward if it relies on a basis of already accepted notions; in this sense an a priori is indispensable. However, we do not have to suppose that this basis, which is constitutive of our fundamental concepts and patterns of reasoning, is unchangeable. Developments in science may show that with hindsight the concepts with which we started were not really necessary—that we can replace them with concepts that are better adapted to what we have learnt. Accordingly, what Reichenbach proposes instead of Kant's apodictic synthetic a priori is a *flexible* kind of a priori, which is relative to a stage in the history of science and is not unalterable—it is a *relativised* a priori.

Reichenbach's relativised a priori still has essential features in common with Kant's synthetic a priori: it is a priori in the sense that it precedes observation and that its availability is a precondition for doing science. The difference lies in the fact that Kant believed that his synthetic a priori truths were not only a priori, but also 'apodictic'—immutable. In Reichenbach's view statements can only be a priori true relative to a certain context of already accepted scientific theories and procedures; relativised a priori statements are therefore not immutable.

As examples of a priori truths Reichenbach lists concepts that are familiar from the kantian transcendental deductions. The concepts of space, time, and a ubiquitous causal relation are all fundamental ingredients of scientific knowledge according to Kant and must therefore be applicable to physical reality as it can be known to us—this is taken over by Reichenbach. An item on Reichenbach's list that we already have encountered and is not familiar from Kant's original writings is the concept of the Riemann integrable probability function. We have seen how and why Reichenbach introduced this function in 1916, namely in order to make it possible to have scientific knowledge of objects that are subject to the inevitable irregularities and errors that occur in scientific practice, for example in measurements. The existence of this probability function is thus a new synthetic a priori. Interestingly, as Reichenbach will note in 1925, the use of this new a priori makes the use of Kant's causality (determinism) as an a priori commitment superfluous; determinism thus will prove to be an a priori in a merely relativised sense, whose validity does not need to be assumed apodictically. This realization is evidently important in view of the development of probabilistic theories, in particular quantum mechanics, in the first half of the twentieth century.

The a priori that clearly comes out as relativised, in the 1920 publication, is that of euclidean geometry. Although euclidean geometry has a hold on our imagination and dominates visualisation, and thus constitutes a natural starting point for scientific investigations, the results of such investigations may show with hindsight that another geometrical scheme is preferable for their description. This is what in fact happened in the case of general relativity. Although we could not have started without the euclidean a priori, it has proven to be not apodictic.

### 6.2.2 The Relativised A Priori Becomes Convention

In 1920, right after the publication of "Relativity Theory & A Priori Knowledge", Reichenbach sent a copy of his work to Moritz Schlick, a colleague of his and one of the leading exponents of the Vienna Circle. In a subsequent exchange of letters Reichenbach is criticised by Schlick for using kantian terminology. Schlick, in his own writings[3], had regarded it as a result of Einstein's general theory of relativity that the geometry we use to describe the spacetime we inhabit has become a matter of *convention*. In his letters to Reichenbach he argues that Reichenbach's relativised a priori is no longer kantian and that he does not see the difference between Reichenbach's relativised a priori and his own conventions. Reichenbach should therefore, in the words of a modern scholar, "avoid misunderstandings about his alliances" ([Dieks, 2010], p325). Schlick believed that one should use the term 'convention', à la Poincaré, instead of using the term 'a priori constitutive principle'.

Reichenbach did indeed do away with kantian terminology after this exchange. However, we would like to argue that he did not fully align himself with Schlick's point of view. As we mentioned in chapter 2 in part I this is in contrast with, eg, how Friedman describes what happened (for scholars who follow Friedman in this, see note 1 on p83). In the course of their correspondence, Reichenbach and Schlick came to agree that their dispute was merely terminological. Reichenbach no longer spoke of principles that are in any way a priori and followed Schlick in speaking about conventions. Friedman argues that there is an important difference in the ideas about conventions

[3]Most notably his "Allgemeine Erkenntnislehre" [Schlick, 1918].

that Reichenbach and Schlick had: a difference which was not discussed by Reichenbach and Schlick. For Schlick conventionality had a very general epistemological connotation. For Reichenbach, on the other hand, conventionality is highly "theory-specific" ([Friedman, 1999], p68).

According to Friedman, when Reichenbach 'gave up' the relativised a priori after his correspondence with Schlick, he did away with a very valuable element in his approach. To appreciate the significance of the theory-specificity of Reichenbach's approach, Friedman says, we need only consider the example of the status of geometry within the theory of general relativity. Schlick says that geometry in general relativity is conventional, and that therefore experiment is not decisive in questions about geometry. According to Reichenbach's early approach, on the other hand, *within the context of the theory of general relativity* geometry is an empirical matter. Once it is agreed upon what is to count as a straight line, experiment is decisive in questions about geometry. In the current and the following sections, we will argue that Reichenbach's early theory-specificity remains as an aspect of the notion of convention that Reichenbach later adopts. We follow ([Dieks, 2010] in this, who argues that the theory-specificity survives as a trace of Reichenbach's early neokantian approach. However, the argument in this chapter goes further than that of Dieks: we argue that Reichenbach's early theory-specificity can be found not only in his later notion of convention, but also as an important element in his 1935/49 frequentism.

First we will investigate in greater detail the difference between Schlick's and Reichenbach's conventions. In order to do so, it is important to understand that 'conventionality' in science comes in different forms.

For example, assume that we want to model a given body of data about the movements of a particle by a continuous mathematical function. No matter how many observations we make, it will always be the case that an infinite number of different functions fit the data. Sometimes, the choice of one function out of these infinitely many alternatives is considered a convention[4]. This would be a very liberal understanding of the concept of convention, according to which we have complete freedom in the choice of the function mentioned above as long as we stay in accordance with the data. Any theory that fits the data would be a proper choice, according to this line of argumentation.

A more restrictive understanding of the concept of convention is that the freedom in the choice of the function in the above example is limited by more than only the given body of data about the movements of the particle. The idea here is that the chosen functions not only have to fit the data but must also meet other criteria, for example the requirement that they have the same form

[4] In modern literature on the topic it is said that the data in this example *underdetermine* the model.

as functions used elsewhere in the edifice of scientific theory. This context-dependent (or theory-specific) restrictiveness is illustrated by the reciprocal relation between geometry, gravitational force, and observation that we mentioned earlier. Given certain pre-existing ideas about geometric notions (how to measure a line, a distance, an angle, etc) and given a certain body of evidence, the choice of a particular geometry for the description of the spacetime is not free. Instead, given these restrictions that are already in place the geometry is fixed and can be determined by measurements. However, as our earlier brief discussion of this example illustrated, we can still maintain that the geometry is conventional here in the sense that the found geometrical structure is not completely sacred but can be modified if we allow changes elsewhere in the theory, for example in the description of the gravitational force.

### 6.2.3 Reichenbach's Conventions Are Not Schlick's

Schlick was not unequivocal about the nature of his conventions: he sometimes seemed to endorse a completely liberal understanding of conventionality and at other times advocated a more restrictive understanding. Obviously, we do not claim that Schlick would have denied that scientific conventions must form a coherent whole with both our observations and with already decided features of the theoretical structure. Still, we think that Schlick's ambiguity regarding this issue makes for an important difference between his conventions and those of Reichenbach. Reichenbach's conventions are *constitutive* in nature—a characteristic not only of Reichenbach's conventions, but also of his earlier relativised a priori.

Let us take a closer look at what Reichenbach wrote about conventions. In his work 'Philosophie der Raum-Zeit-Lehre' Reichenbach discusses the 'arbitrariness' of geometrical conventions:

> "From conventionalism the consequence was derived that it is impossible to make an objective statement about the geometry of physical space, and that we are dealing with subjective arbitrariness only; the concept of geometry of real space was called meaningless. This is a misunderstanding. Although the statement about the geometry is based upon certain arbitrary definitions, the statement itself does not become arbitrary: once the definitions have been formulated it is determined through objective reality alone which is the actual geometry." (The Philosophy of Space and Time; [Reichenbach, 1928], p36)

These words help us to better understand the constitutive character of Reichenbach's conventions. Without definitions for what we are going to understand by geometric notions like distance, line, and so on, it is difficult to see—if not downright impossible—to connect scientific theories about space with observation. The form of the geometry (euclidean or non-euclidean) is a direct consequence of the definitions that are employed. However, these 'definitions' of basic concepts are not completely free or arbitrary choices. They are linked up with the pre-scientific language that we use and we could not even begin our research without accepting some content for them. Nevertheless, we can change them if need be, as discussed before. These will not be arbitrary changes, but rather careful adaptations to the empirical results. These considerations show in what way Reichenbach's conventions contain necessary ingredients for the study of geometry and as such are constitutive principles for geometrical knowledge.

Underlying Reichenbach's more general philosophy of science (according to which science consists for a large part in the coordination of mathematics to natural phenomena) is the idea that the conventions of the kind mentioned above are a necessary ingredient, not only in scientific knowledge about geometry, but in all of science. This makes it clear how Reichenbach's conventions differ from an understanding of conventions as ad hoc additions to a formalism needed to make it into a theoretical model that fits some gathered data. We need to know what fundamental concepts like 'point', 'line' and 'distance' mean, and how they can be measured, before we can set out to make geometrical investigations. We could say that such a priori determinations are fundamental and constitutive, a far cry from being arbitrary in the sense of whimsical—although they are 'arbitrary' in the sense of mutable, subject to change in the face of new evidence.

We can summarise Reichenbach's arguments so far in the following way: Reichenbach in 1916 argued that the assumption that a Riemann integrable probability function, $\phi$, exists is a precondition for science and is therefore synthetic a priori. His view here is part of a traditional kantian framework in which also causality and euclidean geometry are a priori. In 1920 Reichenbach adjusts his philosophical stance in that the status of such preconditions of scientific knowledge become relativised (they are preconditions for a specific theoretical context). Both the original synthetic a priori and the new relativised a priori have a constitutive function, and play analogous roles in a (neo)kantian framework. After his exchange with Schlick, and in particular in his 1928 'The Philosophy of Space and Time', Reichenbach replaces the term relativised a priori with the term convention. However, in Reichenbach's use of the term there is practically no difference with his earlier relativised a priori: although Reichenbach occasionally pays lip service to the Schlick-like 'arbitrary' character of conventions, it is abundantly clear from the way he actually employs the notion that what he has in mind is not complete arbitrariness but rather adaptability under the pressure of empirical results.

## 6.3 (1927) Self-Assessment

In a note left in Reichenbach's papers, dated 6 August 1927 ([Benedictus and Dieks, 2014]), we find the following assessment.

> "Results of my 1914 book:
>
> 1) I demonstrated that the assumption of equal probabilities can be reduced to a continuity assumption, at least for a certain set of problems.
>
> 2) I demonstrated that this continuity assumption is not only presupposed in probability problems, but in all physical statements whatsoever; without it causality claims would be empty.
>
> 3) I attempted to base probability claims on statements that are certain. (There definitively is an N such that $\epsilon <$ given $\Delta\eta$; by contrast, it is not certain whether there exists an N such that $\epsilon > \eta$)
>
> 4) I attempted to demonstrate that the probability assumption is a precondition of knowledge, as a synthetic a priori judgement.
>
> To this I now say the following: 3 and 4 are failed attempts, 1 and 2 have succeeded.
>
> .......
>
> With respect to 4 I have to say that only one publication has convinced me of the impossibility of synthetic a priori judgements: namely my own (1920). For this reason I have already in that publication made the remark that I have to correct my probability book. Also in the symposium I have declared that W is not a synthetic a priori judgement.
>
> .....
>
> With respect to 2: This fact I have uncovered myself, even today I consider it the most important discovery that has been made concerning the problem of probability since Hume."

A remarkable point here is that Reichenbach refers to his assumption of the existence of a continuous distribution as a 'Voraussetzung' (literally, a *pre*supposition or precondition) and in 1927 still considers this idea from his dissertation as his most important contribution to the subject. It is true that in 1927 all a priori kantian certainty is lost (accordingly Reichenbach acknowledges that points three and four in his original programme have failed), but the structuring constitutive a priori appears not to have lost strength.

A second very striking point is that Reichenbach cites his own book from 1920 as his only reason to leave behind the notion of the kantian synthetic a priori. As we have seen, this book was meant and should be interpreted as a move in a neokantian programme, in which the synthetic a priori is weakened but not abandoned. Although the apodictic side of the kantian a priori was jettisoned in 1920, its constitutive role remained in force, and an important part of the book was devoted to an explanation of how this can be understood in a modernized kantian system that accords with science. Rather than leaving kantianism completely behind, Reichenbach had developed an improved form of it.

Another notable feature of Reichenbach's assessment is that he makes no mention of Schlick. In his work after 1920 Reichenbach gradually does away with neokantian terminology[5] and in his 1928 'Philosophy of Space and Time' ('Raum-Zeit-Lehre') speaks solely of conventions. But the conventions Reichenbach speaks of are not the same as those of Schlick, as we have seen. As noted, Schlick was ambiguous about the precise nature of conventions in his epistemology. Schlick regards his conventions as arbitrary, but is ambiguous as to what this arbitrariness means and whether it pertains to individual conventions or only to whole scientific systems. The spirit of Schlick's statements on this subject is that conventions represent completely free decisions, subject to the individual will of the researcher. By contrast, Reichenbach stresses more than once that his conventions are not arbitary in the usual sense and are of a holistic kind: conventions in Reichenbach's view are not completely free but must fit in with each other so as to yield a coherent scientific image which not only accurately describes our observations but also accords with pre-given definitions. Reichenbach's conventions have a constitutive flavour which they have in common with Reichenbach's earlier relativised a priori.

The way Reichenbach actually made use of the notion of a convention, for example in his "Philosophy of Space and Time" (to be distinguished from the lip service he occasionally pays to a Schlick-like arbitrariness in cases in which empirical results do not logically determine a unique theoretical scheme), in combination with the lack of any detailed reference to Schlick, supports the idea that Reichenbach's turn to logical positivism is not as fundamental as tradition has it.

[5] Cf [Dieks, 2010] for a detailed analysis of this.

## 6.4 (1935/1949) The Theory of Probability

We now turn our attention to Reichenbach's "Theory of Probability". Reichenbach finished this book in 1934 when he was a professor in Istanbul[6], and in 1935 it was published in German as 'Wahrscheinlichkeitslehre'. The English translation of the book was published in 1949[7]. The 'Theory of Probability' focusses on the mathematical theory of probability, but it also contains important philosophical parts.

In Reichenbach's 'Theory of Probability' the concept of probability is regarded as indispensable and fundamental for all of science. Actually, Reichenbach puts great stress on the immense importance of the concept of probability for the status of knowledge claims and the possibilities of knowledge acquisition in general. This importance of probability becomes especially clear when one thinks of its role in induction. Induction is essential for the extrapolation of any observed regularity (comprising a finite number of observations) to an unobserved (and unobservable) universal regularity, so almost all of science involves induction. The status of scientific knowledge, therefore, depends to a large extent on the question of whether induction can be justified.

The problem with inductive extrapolation, as Hume showed us, is that inductive inferences are not logically justifiable on the basis of observations, as they are ampliative—their conclusions lie beyond what can be logically deduced from observation. Reichenbach gives a new twist to the subject, by proposing not to give a direct justification of induction but by providing a *vindication* of induction. This vindication consists in showing the usefulness and success in a certain sense of an inductive rule—this should be contrasted with a proof of a factual proposition about the truth-conduciveness of the rule [8].

What we mean by the 'usefulness' of inductive rules depends on the specific context in which the inductive rule is applied. If we regard science as Reichenbach does (in terms of a descriptively simple coordination between observations and reality in which the relation between observations and reality is probabilistic) then we *must* make use of induction in order to have any knowledge at all. Reichenbach's view of science provides a context in which

[6] To where he had fled in 1933 to escape the Nazi's—cf [Irzik, 2011] for a description of Reichenbach's (1933-1938) stay in Istanbul.

[7] The 1949 version was Reichenbach's own translation (with which he was assisted by EH Hutten and his wife Maria Reichenbach) into English of his 1935 work. This translation differed substantially from the original and was hence styled as a 'new edition' of the work.

[8] Cf [Salmon, 1991] for a statement of what is meant by a vindication. Here we read: "One vindicates a rule by showing that its use is well suited to the achievement of some aim we have. The rules of propositional logic can be vindicated by showing them to be truth-preserving. Their use fulfils our desire to avoid deriving false conclusions from true premises."

certain inductive rules are more useful than others.

It is not our aim here to assess the merits of Reichenbach's vindication of induction in itself[9]. We discuss Reichenbach's approach to the problem of induction because, as we will see, it bears a notable resemblance to his 1920 neokantian approach to the same problem.

### 6.4.1 The Vindication of Induction

Before we proceed to discuss Reichenbach's approach to induction we must first take a closer look at what role inductive inferences play in his epistemological views. In Reichenbach's view, inductive inferences—as they are made in the natural sciences—pertain to sequences of observed events. Every event within such a sequence occurs with a certain relative frequency. Any inductive inference consists in inferring that the relative frequencies within the sequence of events so far observed is representative of relative frequencies within a continued infinite sequence of events (Reichenbach's 'straight rule of induction', which we will encounter later in this section, consists in taking the relative frequencies so far observed as *exactly* representative of relative frequencies within an infinite sequence). This characterisation of the inductive inference allows us to precisify the problem of induction: how can we justify choosing a finite sequence for the representation of something that might happen in an unobservable infinite sequence?

Reichenbach's treatment of induction proceeds in two steps. He first attempts to show that of all possible inferences there is a subclass of inferences—the inductive inferences—that are more suitable for finding objective regularities. The second step consists in choosing from this subclass of inductive inferences the particular inference (or inference-type) that satisfies the simplest description ([Reichenbach, 1935], p447).

For his first step Reichenbach starts from the seemingly trivial observation that there either are regularities in nature or there are not. The successful use of induction in science (in making predictions) depends of course on whether such regularities actually exist.

| | **Sequence has a limit** | **Sequence has no limit** |
|---|---|---|
| **Induction is used** | Success | Failure |
| **Induction is not used** | Success or failure | Failure |

**Figure 6.1:** Table showing the results of either using or not using induction when confronted with sequences of observations.

Our belief that physical regularities do exist is a basis for thinking that

[9] Reichenbach's attempt at vindication has been criticised; eg ([Salmon, 1991], p105) and ([Atkinson and Peijnenburg, 2008], p2).

observed relative frequencies within a sequence of observations converge to a limit if the sequence were continued indefinitely. Reichenbach now considers four possibilities with regard to the existence of such a limit. Suppose a scientist is confronted with a sequence of observations and is asked to make predictions about the elements of the sequence that will be observed next. The scientist either chooses to use induction or she does not. The choices available to the scientist are listed in table 6.1 (copied from [Salmon, 1991]). Reichenbach argues that if regularities actually exist then the use of induction will (after possibly an infinite number of observations) certainly lead us to them. There is even a possibility that induction will yield accurate predictions after a finite number of observations. The alternative is described by Salmon as follows:

> "Suppose, instead, that we do not use induction. This might happen in either of two ways. In the first place, we might simply refuse to make any inferences at all. This alternative obviously fails whether nature is uniform or not. Nothing ventured, nothing gained. In the second place, we might try some different method for making predictions, for example, making wild guesses, consulting a crystal gazer, or believing what is found in Chinese fortune cookies. If nature exhibits uniformities, any of these methods might work, but there is no guarantee of success. If nature is uniform, then, it seems clear that induction is the best method, for it is bound to work on the whole, whereas the others may or may not be successful." ([Salmon, 1991], p100)

These considerations serve to make a selection from all possible inferences (or rules which tell us which inference to make). The inference rules that are to be preferred are those involving induction. These inferences are equivalent in the sense that they are able to latch on to regularities, if these exist in nature, so that these rules will asymptotically close in on those regularities (consider, for example, an often repeated cointoss: any inductive inference is an extrapolation of observed relative frequencies to limiting relative frequencies. If these limiting relative frequencies are representative of a natural regularity then Bernoulli's law states that it becomes increasingly unlikely that the observed relative frequencies deviate much from the limiting relative frequencies as the number of tosses becomes larger). Because of this asymptotic approximation Reichenbach calls the preferred rules *asymptotic rules.* The use of these rules will be vindicated in the sense that if success can be achieved, these rules will achieve it in the long run. Obviously, this does not mean that we can prove that there actually are regularities in the long run; so we do not have a solution of the problem of induction in the form posed by Hume.

But how do we further justify or rather vindicate choosing from the class of asymptotic rules the inductive rule that is to be used in science? Reichenbach argues that we should choose the rule that displays the highest degree of descriptive simplicity ([Reichenbach, 1935], p447). The single inductive rule that remains is Reichenbach's 'straight rule of induction' ([Glymour and Eberhardt, 2016]): in any scientific experiment we should take the relative frequencies of events within observed sequences as representative of limiting relative frequencies. It is important to note that Reichenbach's appeal to descriptive simplicity is pragmatic and does not entail an ontological claim; Reichenbach is not arguing that the asymptotic rule which is simplest will bring us closest to a true description of nature (see [Reichenbach, 1935], p447 and [Reichenbach, 1938], p373 ff).

Reichenbach's stance towards asymptotic rules in 1949 is reminiscent of his 1916 stance towards $\phi$. We saw earlier that Reichenbach in 1916 states that we know nothing about the precise form or even objective existence of $\phi$, but that we have to act nevertheless as if we knew that some $\phi$ exists in order to make scientific research possible at all. The first step in Reichenbach's 1949 treatment of induction is similar: we do not know whether there are any real persistent regularities, but in order to make sense of science we must assume their existence. We do not pretend to know that asymptotic rules will reveal natural laws, but we know that if natural regularities exist as we have to assume in science, then asymptotic rules will help us to find them and the simplest rule will do so as well as others, as far as we know.

### 6.4.2 Posits Redux

The inductive extrapolation of observational data, in accordance with the above philosophy, leads to the making of what Reichenbach calls *posits*. Reichenbach compares these posits with what we do when we place a bet in a horse-race: we do not express, by placing a bet, that we are sure that the horse will win with a certain frequency, but we act *as if* this were true by staking money on it. Analogously, an inductive posit is a statement about a limit frequency which we treat as being true, although its truth-value is actually unknown.

The posits of Reichenbach play a very important role in his frequentism. We have met Reichenbach's frequentism in an earlier chapter. The general idea of Reichenbach's early (1916) frequentism is that in any experimental setup wholly subject to causal laws degrees of probability nevertheless result from ever-present disturbing influences (which are themselves also subject to causal laws). Such influences have the result that the experimental outcomes, rather than being identical for identical experiments, are distributed in a non-trivial way. We have to posit that they conform to a continuous probability function.

The degree of probability of any particular outcome is the relative frequency to which this outcome is coordinated by the probability function—the existence of these well-defined probabilities, which inductive methods attempt to approximate, is a posit.

We should mention, as a side issue, that in 1935 Reichenbach stresses that the technical results of his earlier 1916 approach to probability can be retained in the new form he is now giving them. In particular, the use of the continuous probability function to define equiprobability can be carried over to the new conceptual framework. As he writes:

> "Even though [the] first among my papers referring to the problem of probability was written under the influence of Kant's epistemology, it seems to me that the result concerning the theory of probability can be stated independently of Kant's doctrine and incorporated in my present views." ([Reichenbach, 1935], p355)

Reichenbach's elaboration of this remark makes it clear that the 'result' he refers to is the emergence of equiprobability along the lines originally proposed by Poincaré, as we discussed earlier. In 1916 this result followed from the kantian a priori of the existence of a probability function; now it emerges from the *posit* that this same function exists.

In order to compare Reichenbach's 1935 frequentism with his 1916 views in terms of a concrete example, we return to his probability machine (see p54). We saw that the result of any finite number of runs of such a machine is a bar-chart whose tops, if linked together, form a discontinuous curve. Reichenbach's $\phi$ results from the idealisation of the number of runs going to infinity and the width of the bars approaching zero. In such an idealised situation the tops of the bars would form a Riemann integrable curve. This curve is representative of limiting relative frequencies and therefore probabilities. Reichenbach's 1916 statement that the existence of $\phi$ is a condition for science to be possible, we conclude, is equivalent to his 1935 view that we have to accept, as posits, the existence of limiting relative frequencies. Reichenbach's 1916 statement that the possibility of science depends on the a priori assumption that $\phi$ exists plays the same functional role as the statement in Reichenbach's 1935 work that we have to posit the existence of limit frequencies that can be approximated by relative frequencies in finite sequences, in order to make sense of inductive practices at all. The use of inductive rules, in particular the straight rule, is vindicated by these posits.

The above considerations highlight a clear trace of Reichenbach's early neokantian approach—that of *context-specificity*. In 1916 Reichenbach had the idea that $\phi$ must exist if probability statements are to be possible. In 1928 Reichenbach had the idea that conventions must be theory-specific in order to make geometrical knowledge possible. In 1935, we have seen in this section, it is the context of science as a coordination which is optimally useful for making predictions and at the same time descriptively simple that vindicates us in making posits.

## 6.5 (1938) Experience & Prediction

In 1938 "Experience and Prediction" ([Reichenbach, 1938]) appears, also written in Istanbul. In this book, with the subtitle *An Analysis of the Foundations and the Structure of Knowledge*, Reichenbach presents a comprehensive epistemological system in which probability figures as the central concept. Again the problem of induction receives ample attention, and is discussed along the lines already set out in *Wahrscheinlichkeitslehre*.

### 6.5.1 Reduction & Projection

Reichenbach begins explaining his view on science by showing how it differs from a positivist outlook. The positivist, according to Reichenbach, reasons as follows ([Reichenbach, 1938], p101). The relation between propositions in scientific theories and propositions about objects in external reality is one of *reduction*. Reichenbach compares this relation to that between a wall and the bricks it consists of. Propositions about the wall are reducible to propositions about the bricks; any proposition about the wall is equivalent to one or more propositions about the bricks in it. It is an essential feature of such a reduction that it is not 'existence-preserving' (or 'ontologically symmetrical'): the proposition that bricks exist does not imply the proposition that a wall exists.

This reductionist doctrine entails a theory of meaning which Reichenbach diagnoses as highly problematic, and which he replaces with a probabilistic variant. According to the positivist's theory of truth and meaning a proposition has meaning iff it is verifiable as true or false by checking the truth status of the elementary building blocks to which it can be reduced ([Reichenbach, 1938], p30), whereas according to Reichenbach's probability theory of meaning a proposition has meaning iff it can be associated with a well-determined degree of probability ([Reichenbach, 1938], p54). Reichenbach argues that this different theory of meaning nullifies the positivist agnostic argument: instead of saying that the only thing we can know is what is literally contained in the observations, we can consider statements that go beyond the observable, while withholding our full consent.

As a consequence of adopting this different theory of meaning Reichenbach regards the relation between terms in scientific theories and propositions about objects in external reality not as one of reduction but of (what Reichenbach calls) projection (see, for an example of such a projection, figure

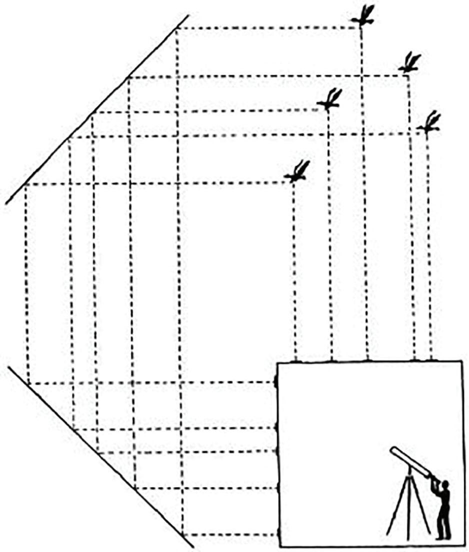

**Figure 6.2:** Reichenbach illustrates his idea of a projection by considering observers inside a tent seeing some overflying birds. The observers cannot directly see the birds but only the shadows the birds (and their mirror-images) cast on the walls of the tent—each bird corresponds to a set of shadows ([Reichenbach, 1938],p 117).

6.2). Both a reduction and a projection consists in the coordination of a set of elements to a 'complex'. The difference lies in the fact that the relation between a reductive complex and its elements is an equivalence relation, whereas the relation between a projective complex and its elements is a probabilistic relation. This difference changes the implicatory asymmetry that is a characteristic of a reduction into a probabilistically implicatory symmetry. The proposition that a certain complex exists, according to the positivist doctrine implies the proposition that reductive elements exist, but not the other way around: the proposition that reductive elements exist does not imply the proposition that there is an associated complex (cp the wall and its bricks). The projective relation does not have this characteristic of asymmetry. The proposition that a certain projective complex exists implies the proposition that its corresponding elements exist with a certain probability. Likewise, the proposition that observational elements exist implies the proposition that their projective complex exists with a certain probability. The crucial point is that the implicatory asymmetry of the reductive relation allows room for the rational belief in ontological asymmetry, whereas a projective relation makes the belief in ontological symmetry rational.

Reichenbach argues that the positivist's idea that the relation between our impressions and the outside world is one of reduction is not correct: "the relation between direct and indirect sentences is only a probability connection, not an equivalence. Thus the main idea of the positivistic reduction is not tenable." ([Reichenbach, 1938],p 104). In this quote "direct sentences" refers to impressions and "indirect sentences" refers to propositions about the outside world—as we have already noted, Reichenbach calls the relation between impressions and the outside world a projection rather than a reduction. It is part and parcel of Reichenbach's approach that there is uncertainty about the relation between our impressions and the outside world. This conforms to scientific practice: there is uncertainty involved in all scientific measurements, so a given body of data (eg observations of shadows on a wall) yields only probabilistic support to a model explaining the data (eg birds flying over a tent with mirrors next to it).

Due to the probabilistic connection we are not justified in believing that the relation between propositions about the shadows and propositions about the birds—to stay with Reichenbach's own example— is (ontologically) asymmetrical: propositions about the existence of the shadow-observations imply propositions about the existence of the birds probabilistically and, inversely, propositions about the existence of the birds imply propositions about the existence of shadow-observations probabilistically. Reichenbach's argument is that since the relation between propositions about the shadows and the birds is not asymmetrical there are no good grounds for believing that there is a difference in ontological status between the sets of shadows and the birds. This, then, is a step in the direction of scientific realism. Reichenbach does not at all reject the existence of an unobservable external reality: rather, he embraces it. If we posit the existence of an unobservable physical world, this world can be made reponsible for our impressions and the very idea of its existence gives content to scientific research. This posit of a not directly observable reality does not mean that we can be sure that this reality actually exists. The relation between what we observe and what we assume to exist is not one of certainty—but we have to make this assumption of an external reality nevertheless in order to make a coherent whole of our scientific activities.

### 6.5.2 Reichenbach's Realism

The notion of an external reality is defended by Reichenbach by using an argument whose form is similar to what in modern philosophy of science has become known as *inference to the best explanation* (IBE), frequently used by present-day philosophers of science to defend scientific realism [10]. The precise meaning of the word realism can be debated, but for our purposes it suffices to understand scientific realism as the philosophy that has as its basic

[10] Cf [Psillos, 2011], p30 for a comparison of Reichenbach's argument with the realist argument in modern philosophy of science.

premiss that the terms that figure in established scientific theories correspond to entities (objects) that exist independent of observation[11]. The belief in the existence of external objects is justified, according to the realist, because the existence of an outside world which is (approximately) as described by the scientific theories we accept is the best way we can explain our observations and the success of science[12].

The IBE has a structure that is typically conditional in nature. Let $e(O)$ denote the proposition that external objects exist and let $o(O)$ stand for the proposition that objects are observed. Then the IBE from the previous paragraph is structured as follows:

$$[[e(O) \Rightarrow o(O)] \wedge o(O)] \text{ therefore } e(O) \tag{6.1}$$

In words: "if certain external objects exist then they can explain certain observations. These observations do in fact occur; therefore the objects in question exist". The assumption that a stable world of (sub)microscopic objects exists is the 'best explanation' for our observations. The relation with Reichenbach's argument is clear. For Reichenbach the external world is a 'projection' that has to be posited to make sense of the way we deal with our scientific observations—it is not something that we can be sure of, but it plays a fundamental constitutive role nevertheless.

It is these posits, playing both the role of 'best explanation' and 'precondition for the meaningfulness of science' that play a dominant role in Reichenbach's philosophy of science, his theory of probability and his vindication of induction. To paraphrase Reichenbach himself:

> "As blind men we face the world. We feel no path but we know that only if we believe that a path exists we might find that path ."[13]

### 6.5.3 Summary

We are now in a position to combine and compare several of the notions that we have encountered in our description of Reichenbach's work. In 1916 Reichenbach argued that he had transcendentally deduced the existence of $\phi$ in the kantian sense, as the possibility of science depends on the existence of

[11] Which is not to say that these entities/objects are necessarily wholly independent of observation.

[12] This is a variation on the no-miracles-argument; cf [Laudan, 1981] for a criticism of this argument. Laudan argues that the no-miracles-argument (NMA) is 'viciously circular'.

[13] Reichenbach ([Reichenbach, 1935],p 482) writes: "As blind men we face the future but we feel a path. And we know: if we can find a way through the future it is by feeling our way along this path."

this continuous probability function. In 1920 Reichenbach modified his earlier kantian synthetic a priori and moulded it into his more flexible relativised a priori. This new a priori is flexible in the sense that it can change over time, and relativised in that it holds relative to a specific scientific context and accepted background knowledge. The new a priori, just as the original kantian and Reichenbach's earlier a priori, has a constitutive nature—it provides a basis for scientific knowledge and justifies research procedures. If science is to be possible then some such basis is necessary and constitutive elements therefore have to be in place.

Between 1920 and 1928 Reichenbach replaces the concept of the relativised a priori with the term convention. The way in which Reichenbach uses this term does not completely coincide with the way his contemporaries use it, though: Reichenbach's conventions are linked up with constitutive principles. In 1935 we see that Reichenbach attempts to vindicate the use of induction in science and argues that inductive inferences should be made on the basis of *posits*. Reichenbach does not say that posits are true (ie latch on to actual physical states of affairs or regularities) but he considers their status as constitutive: if science is to be possible and meaningful at all then we must make inductive posits. In 1938 Reichenbach argues that the relation between terms in scientific theories and objects in external reality is not one of reduction but of projection. In combination with an argument similar to inference to the best explanation the idea of the projection helps Reichenbach defend a realist stance. The IBE in his case plays a constitutive role: the best explanation for science being successful is the assumption that scientific terms refer to entities that exist in an external world, and if we want to make sense of our observations at all, we have to assume that they are manifestations of some underlying world as well, even though in both cases we cannot be sure about the characteristics of this world.

During the gradual development of Reichenbach's philosophy the importance of the concept of probability keeps increasing. In 1916 Reichenbach argued that a probability function exists, alongside a causal principle (a law of connection), to make it possible for scientists to deal with measurement errors. Reichenbach in 1916 assumed that the statement that $\phi$ exists is a synthetic a priori statement in the kantian sense. Although Reichenbach in 1920 gave up this terminology, in 1925 he argued that $\phi$ not only augments but might even replace the principle of causality. Reichenbach's 1935 posits are degrees of probability; strictly deterministic laws no longer are necessary to make sense of science. In 1938 Reichenbach uses the concept of probability to make a distinction between a reduction and a projection—a distinction that plays a crucial role in Reichenbach's argument for the posit of an external world.

We may conclude that the standard account of Reichenbach's philosophy—which sees the early, neokantian, Reichenbach convert to a diametrically opposed logical positivist stance—is only partly justified by Reichenbach's writings. A convincing case can be made that traces of transcendental philosophy remain of vital importance in Reichenbach's later writings. More specifically, we have tried to show that Friedman's remark that "...the most important element in [Reichenbach's] conception of the relativized a priori is actually lost" ([Friedman, 1999], p64) should be put in perspective. It may be argued that this 'most important element' (viz the theory-specificity of Reichenbach's relativised a priori) can be found in Reichenbach's later work: the possibility of posits as relativised a priori in the context of probability theory.

## 6.6 More Transcendental Traces?

The past chapters and in particular the present one may raise the question whether there are also traces of Reichenbach's early transcendental philosophy in his writings after 1949—publications later than his "Theory of Probability". In this dissertation we have limited ourselves to the period up until 1949, in which a clear continuity is visible. We will leave Reichenbach's publications between 1949 and his death in 1953 for further research, but we will make one remark about his "The Rise of Scientific Philosophy"; a popular book on general philosophy of science published in 1951 [Reichenbach, 1951].

In "The Rise of Scientific Philosophy" Reichenbach states ([Reichenbach, 1951], p252) that the logical empiricist replaces a transcendental conception of scientific knowledge, according to which human knowledge can transcend the boundaries of the (either directly or indirectly) observable, with a functional conception which "regards knowledge as an instrument of prediction and for which sense observation is the only admissible criterion for nonempty truth".

At first sight these words of Reichenbach fit in very well with the story of Reichenbach's conversion (cf. [Friedman, 1999], pp62 ff) from a neokantian beginning to a logical positivist position. However, in the current and the past chapters we have shown that we must be careful: although Reichenbach did not leave all transcendental reasoning behind, he moulded it into schemes and formulations befitting the empiricist, as we have seen. Reichenbach's ideas about the relativised a priori and his vindication of induction do not conflict with the empiricist ideal to make the empirical basis decisive for all scientific knowledge claims, and so it is easy to give Reichenbach's views the appearance of completely fitting in with logical positivism—of which Reichenbach is often regarded as one of the most distinguished exponents. It is only when we look at the details of his probability theory, his theory of induction and his defence of his own brand of realism that we see that Reichenbach's philosophy is an attempt at formulating an answer to the neokantian transcendental question: "*how is scientific knowledge possible?*"

# Chapter 7

# Epilogue

## **Reichenbach's Logical Positivism** - A Methodological Apology

**The contents of this chapter**

1. 'Positivismusstreit'
2. The Subtle Empirical Cycle

In the last chapter of this dissertation we will argue that the development of Reichenbach's work as analysed in the previous chapters problematises an opinion that is widespread among both scientists and philosophers. This opinion is that Reichenbach's ideas, as belonging to or being similar to logical positivism, are a past and passed station. This standard view conflicts with the fact that many of Reichenbach's ideas are more subtle than often recognised and anticipate many later post-positivist ideas. We argue our point by discussing how a number of present-day philosophical issues in the philosophy of science are treated if not solved in Reichenbach's work.

## 7.1 Positivismusstreit - The criticism of the logical positivists by the Frankfurt school

In discussing Reichenbach's changing philosophy of science we have spoken about 'logical positivism' or 'the logical positivist school' to denote a movement in the philosophy of science that was contemporary with Reichenbach philosophical activities (or at least the later stages of Reichenbach's work). There is no consensus in the scientific literature about the precise characterisation of this movement ([Creath, 2011]) or which scholars should be identified as logical positivists (Cf [Baldwin, 1998]). Notwithstanding the absence of a precise characterisation, logical positivism has been the target of fervent attacks by other philosophical schools. Perhaps most notable among these is the Frankfurt school[1].

The claims and accusations made by the Frankfurt school against the logical positivists extend over several decades[2]. The general claim of the adherents of the Frankfurt school (put very roughly) is as follows. Positivism and logical positivism continue to advocate the enlightenment ideal of reason and science bringing light into the darkness wrought by religion and other forms of superstition. The authors argue that the rigid rejection of metaphysics and the quantification and rationalisation that come along with a restriction of the methods of philosophy to those of science inevitably lead to dehumanised politics, preparing the ground for oppression and dictatorship (Horkheimer, in 'Dialectic of Enlightenment', expresses this as the "indissoluble alliance of reason and atrocity" ([Horkheimer and Adorno, 1944], p92)[3].

The accusations against the logical positivists were not limited to the political or sociological domain. The Frankfurt philosophers also accused the logical positivists of several methodological errors or naiveties . First of all, the logical positivists were accused of adopting the idea that science is about facts which are the object of straightforward observation. In this context Adorno writes about "the positivistic ideal of the sheer acceptance ("Hinnehmens") of irreducible facts" ([Adorno, 1956], p57). Marcuse emphasises the naivety of the logical positivists' approach in this respect when he says that "neo-positivism is not concerned with the great and general ambiguity and obscurity which is the established universe of experience" ([Marcuse, 1964], p187). Another respect in which Marcuse believed that the logical positivists falter is the logical positivist conviction that science progresses "toward the real core of reality" ([Marcuse, 1964], p154). The view that underlies this, which is often associated with logical positivism, is that scientific progress is cumulative. This view has also been criticised outside the Frankfurt school. In this context Kuhn's work 'The Structure of Scientific Revolutions' ([Kuhn, 1962]) plays a pivotal

[1] Members of the Frankfurt school directed most of their attacks against what they called 'positivism'. They used the term positivism to refer to different philosophical movements that they believed shared the same methodological basis (including the Vienna Circle, logical positivism, and logical atomism; cf 'The Essential Frankfurt School Reader' ([Arato and Gebhardt, 1985]).

[2] Beginning with Horkheimer's 1937 'The Latest Attack on Metaphysics' and extending well into the sixties (the sixties were the high tide of the 'positivism dispute' ('Positivismusstreit') between the adherents of the Frankfurt school (Marcuse and Adorno, among others) and exponents of positivism (Popper and Albert, who, by the way, did not consider themselves as positivists but as critical rationalists).

[3] The first edition of [Horkheimer and Adorno, 1944] was published by Social Studies Association, Inc., in New York.

role. A third characteristic for which logical positivism is often criticised is the logical positivist's alleged uncritical use of the notions of verification and justification. Marcuse accuses the logical positivists of a "rejection or devaluation of those elements of thought and speech which transcend the accepted system of validation ([Marcuse, 1964], pp188-89).

The details of the attacks and accusations by the Frankfurters—although interesting in themselves—need not concern us here (nor is it important for us to establish which arguments should be attributed to whom). What the above account illustrates is that logical positivism has acquired a negative connotation: in contemporary literature on the philosophy of science it is often considered as a sign of intellectual immaturity to express affinity with logical positivist ideas—even if this affinity only pertains to logical positivist ideas about the methodology of science[4]. We do not make any claims about the sociopolitical or historical aspect of the accusations made by Horkheimer and others, but if the methodological assumptions about science made by the logical positivists can be separated from the political connotations that logical positivism has, then we think that the point argued for in the next section can be upheld.

## 7.2 The Subtle Empirical Cycle

The core methodological assumptions that lie at the basis of the logical positivist approach to a large extent can be said to be basic to much of modern scientific methodology as embodied in a sophisticated version of the *empirical cycle*. Although many scientists nowadays doubt whether the chaotic and capricious process of scientific development can be described with the help of a few neatly ordered concepts, there can be little doubt that there are a number of concepts that play a central role in scientific research and scientific change. Present-day scientists and philosophers of science who explicitly distance themselves from logical positivism in really all its facets deny a great part of their own identity[5]. In Reichenbach's work the philosophical issues concerning the central concepts within the empirical cycle receive ample attention. As we will argue, Reichenbach's ideas are much more sophisticated than those that critics have often associated with logical positivism.

We will argue our point by first describing the empirical cycle itself and then show that several of the problems that according to critics of logical positivism beset the various elements of the empirical cycle are anticipated,

[4]Perhaps this is also what Salmon meant when he wrote that "logical positivism is passé but logical empiricism is not" (as quoted by [Stadler, 2011] (p152))

[5]We do not claim that the Frankfurters' criticism is necessarily in all respects careless or inaccurate. Whereas in [Horkheimer and Adorno, 1944] subtlety is sometimes sacrificed at the altar of dramatic effect, [Marcuse, 1964] shows (as we will see) ample attention to detail.

discussed or even solved in Reichenbach's work. It is precisely because of Reichenbach's specific approach, going back to his kantian background, that his philosophy of science is more sophisticated than what is often associated with logical positivism.[6]

The empirical cycle is the name of a methodological model of empirical research. Many and manifold are the different formulations of the empirical cycle. The original formulation of the empirical cycle was given by A D de Groot in 1961 (the model is here reproduced in figure 7.1).

In de Groot's model there are five components:

1. observation
2. induction
3. deduction
4. testing
5. evaluation

It stands to reason that the methodological models of empirical research that are in use nowadays are not exactly identical to the empirical cycle as it was described by de Groot. For example, A F Chalmers describes a model of empirical research which is more elaborate, as it includes laws, theories, prediction and explanation ([Chalmers, 2008], p54).

We will discuss the various items in the list above in the context of a concrete example. Suppose that in some scientific experiment we gather a body of data. We may think of an experiment in which we make several **observations** of a falling stone in order to determine the path it follows. Say that we simply look at the stone at several subsequent points in time to determine its position. We can then apply **induction** to these observations to coordinate the path of the stone to a mathematical curve. We may call this coordination hypothesis $H$. After that we can use $H$ to **deduce** predictions about some future experiment (eg the path of the stone when we drop it from a different height). The outcome of the future experiment serves as a **test** of the hypothesis of the mathematical curve. In the **evaluation**-phase of the research we decide whether or not $H$ should be held on to.

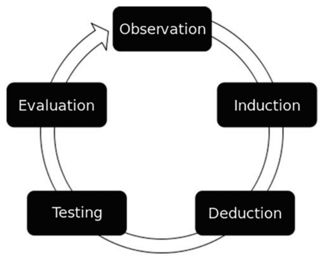


**Figure 7.1:** A D de Groot's model of empirical research.

[6]It seems not implausible that the standard picture of logical positivism that is a frequent target of attack is inaccurate in general. However, we here focus on Reichenbach's specific position and will not argue the more general case.

### [1] Observation

Let us first look at item 1: *observation*. First of all, we must realise that the set of sense impressions that is the direct result of conducting an experiment is not the same as what the scientist usually calls an observation. Observations, such as that of the falling stone in the example, presuppose a theoretical context in terms of which sense impressions can be interpreted. In the above example we assume that there is a definite way to determine from some set of sense impressions that the stone has a certain position. Typically this means that we assume that the geometry of space-time is fixed and that light travels (approximately[7]) in straight lines.

The idea that the results of scientific observations presuppose a theoretical context (and thus should not be regarded as 'irreducible facts') has become known as the *theory-ladenness of observation*, a concept usually attributed to Popper. However, Popper's idea is by no means overlooked by Reichenbach—neither in his early nor in his later work. More specifically, the constitutive aspect of the kantian synthetic a priori (and of Reichenbach's own relativised a priori) plays a role in Reichenbach's philosophy, from its very beginnings, that is very similar to what Popper called theory-ladenness. According to Reichenbach scientific statements can only be asserted as a part of a certain conceptual context: In his 1916 dissertation this is still the fixed background of kantian philosophy, but from 1920 on Reichenbach relativises the conceptual background to what is contained in already accepted scientific theories. In both cases, the central idea is that we can only make scientific sense of sense impressions if we coordinate them to scientific theories, and this can only be done if we already have concepts in place—observation without concepts is blind, to paraphrase Kant. The theoretical context plays a constitutive role for the statements made within this context. Clearly, this is closely related to the 'post-positivist' idea that every scientific statement is laden with theoretical content: also here every scientific statement presupposes a theoretical context.

Early on, Reichenbach emphasized that it is the central task of science to coordinate empirical findings to theoretical schemes. The recognition that this can be done in various ways, even though it is true that we can only proceed if we use one specific framework to start with, was one of the central insights of his 1920 study of the relativised a priori. Concretely, we cannot do any physics of space and time at all, if we do not accept that certain sense impressions have a meaning in terms of concepts like angle, line, length, etc, which obey euclidean relations. However, this is no obstacle of principle to the possibility of 'putting on other glasses' later, and look at empirical reality through them. This is why it proves possible to interpret the same sense impressions one

[7]Measurement limitations make the assumption that light travels in straight lines methodologically equivalent to the assumption that light travels only approximately in straight lines.

time in terms of euclidean geometry, and another in terms of relativistic non-euclidean relations. The whole possibility of scientific progress depends on this recognition of theory-ladenness of observation, in Reichenbach's philosophy.

As a further concrete example, we may look at a statement that assigns to a certain event a certain degree of probability. The idea of theory-ladenness entails that the meaning of a probability statement depends on the probability theory (or rather, interpretation of that theory) within which the statement is made. Within Laplace's theory of probability a probability statement asserts the existence of a state of belief, often justified by certain physical symmetries. By contrast, in frequentist theories of probability a probability statement asserts something completely different, namely something about an infinite sequence in which the type of event in question occurs with a definite relative frequency.

Reichenbach is fully aware of this difference in his dissertation of 1916, in which he discusses several interpretations of probability. He also notes the important role played by conceptual frameworks in this context. Reichenbach saw it as a central part of his own conception of probability that one should look at the outcomes of experiments assuming that they approximate a probability function $\phi$ whose existence must be assumed a priori and which plays a constitutive role in giving meaning to probability statements. Statements of probability cannot be made without $\phi$ in place. Saying that $\phi$ serves a constitutive function in probability statements is tantamount to saying that probability statements are laden with theoretical content.

We see that Reichenbach is not at all guilty of overlooking the subtleties involved in scientific observation, and does not regard the results of scientific observation as 'irreducible facts'.

### [2] Induction

The second item in the list is *induction.* The problem of induction has been extensively discussed in earlier chapters. Reichenbach considers the problem of induction in the form posed by Hume to be unsolvable; this already implies that he is aware of the problems surrounding the subject. Reichenbach acknowledges that inductive statements cannot be logically derived from observations, because such statements are founded on inferences which are ampliative—Reichenbach acknowledges the spectre of induction that ever since Hume's writings has haunted philosophers. Although the impossibility of logically deriving inductive statements is more or less unquestioned, this problem of induction is still current in modern literature on the philosophy of science[8].

As we have seen, Reichenbach not only comments on this old problem and sees its seriousness, but also presents an analysis that gives a new and

[8]See, for examples, [Howson, 2000] and [Artemov and Fitting, 2011].

sophisticated twist to it. Instead of looking for a logical justification he proposes that induction can be *vindicated*. A vindication does not involve the derivation (logical or otherwise) of some statement, but rather justifies the application of a rule in practice. Far from rejecting "those elements of thought and speech which transcend the accepted system of validation" (Marcuse; see above), Reichenbach proposes a new philosophical concept which is not a part of the accepted system of validation. In this aspect, too, Reichenbach was not short-sighted.

In addition, we should note that Reichenbach's solution, relying as it does on posits that replace his earlier a priori assumptions—as we have seen in earlier chapters—perfectly fits in with the just-mentioned idea that pre-existing conceptual frameworks are indispensable to structure scientific research and even to make sense of it. This anticipates, and at the same time obviates, much of the criticism that portrays logical positivism as a naive attempts to simply see the world as it really is. At least in the work of Reichenbach this criticism does not hit the mark.

**[3] Deduction**

Thirdly, there is the concept of deduction. Underlying much of the criticism of the logical positivist rejection of metaphysics is the idea that the logical positivists' belief in the primacy of logic (and, hence, deduction) is misguided. This belief is clearly born out by passages such as Adorno's "critique of logical absolutism" ([Adorno, 1956], p41). The belief in the validity of deduction (the belief that deduction leads from true premises to true conclusions) is a crucial premise of the empirical cycle. For a methodology built around the empirical cycle the conviction that deduction is valid is essential.

The idea that deduction is not an infallible tool leading to absolute truth was playfully illustrated by Lewis Carroll in an article he wrote in 1895: 'What the Tortoise Said to Achilles' [Carroll, 1895]. Carroll's puzzle is much older than the positivism dispute related in the previous section, but the puzzle's solution illustrates one of the pillars of logical positivism: the idea that logic does not possess empirical content and that its theorems are true by definition.

Carroll's puzzle aims to show that convincing someone that deduction leads from true premises to true conclusions is impossible if that someone does not already believe that deduction is logically valid. In Carroll's 1895 article Achilles is challenged by his antagonist, the tortoise, to convince him that a certain deductive argument is logically valid. Achilles finds out, in the course of the dialogue, that even if an infinite number of assumptions is added to the premises of a deductive argument (as it is formulated by Carroll) a sceptic of deduction may remain sceptical.

Achilles' argument is as follows:

**Figure 7.2:** Charles Lutwidge Dodgson (1832-1898). Under the pseudonym 'Lewis Carroll' Dodgson wrote the article 'What the Tortoise Said to Achilles' (1895) there is a dialogue between Achilles and the tortoise in which the tortoise makes Achilles see that even an infinite number of assumptions need not be enough to convince a sceptic that deduction is valid.

**A** Things that are equal to the same are equal to each other.

**B** The two sides of this Triangle are things that are equal to the same.

**Z** The two sides of this Triangle are equal to each other.

But according to Carroll's tortoise it is not impossible to believe that A and B are true without believing that Z is true. Achilles therefore adds premise C: "If A and B are true, then Z must be true", and argues that A, B and C lead to Z. Carroll's tortoise is still not convinced and counters that it is not impossible to believe that A, B and C are true without believing that Z is true. Achilles is forced to add assumption D: "If A, B and C are true, then Z must be true". But still there is no end to the tortoise's doubt...*ad nauseam.*

In [Carroll, 1895] there is no discussion of the philosophical implications of the arguments above. The message, however, is clear: deduction cannot be logically justified (it cannot be proved within logic that true premises lead to a true conclusion). That seems paradoxical as logic and science rest on deduction.

A solution to Carroll's puzzle can be found in Russell's "Principles of Mathematics" ([Russell, 1903], par38) in which Russell by-passes Carroll's paradox by distinguishing logical implication from logical inference[9].

The distinction that Russell makes is that in a deductive argument one of whose premises is an inference the premises are actually asserted ($p$ is true, *therefore* $q$ is true), whereas in a deductive argument one of whose premises is an implication the premises are only hypothetical (*if* $p$ is true, *then* $q$ is true). If we apply Russell's approach to the arguments in Carroll's dialogue

[9]Russell himself calls his approach to Carroll's paradox merely a "the first step in answering Lewis Carroll's puzzle".

we see that the statement 'A and B imply Z' does not assert that either A, B or Z is true. Rather than stating 'A and B are true *therefore* Z is true' a deductive argument need only assert that '*if* A and B are true *then* Z is true'. In this form the deductive argument does not involve making existential assertions and therefore is not in need of justification because there is nothing to be justified ($[p \wedge (p \rightarrow q)] \rightarrow q$ asserts nothing (no *knowledge*) if we take $\rightarrow$ to mean 'implies'). The validity of deduction rests on an assumption, and as such we may be certain that deductive implication leads from true premises to true conclusions.[10]

Typically, logical positivists share Russell's view on the logic of deduction in that they argue that logic in itself does not involve any existential assertions (as opposed to implications)[11]. This view on deduction also underlies Reichenbach's philosophy of science, as attested by his repeated remark that 'logic is empty'. In this respect Reichenbach does not so much distinguish himself from his contemporaries. However, our analysis shows that it would be unfair to criticise the logical positivists for being naive and unthoughtful about the status of logic; this comment certainly also applies to Reichenbach's work.

### [4] Testing

The fourth step in de Groot's empirical cycle is that of *testing*. We understand testing here as the comparison of predictions deduced from a hypothesis with observations in an experiment. What our characterisation of testing immediately shows is that it cannot be separated from observation, induction and deduction. However, because the latter three concepts have already been discussed we focus here on a problem that is specifically related to the testing of hypotheses—the problem of verification. Verification has had a very specific role in the early stages of logical positivist philosophy; a role which manifests itself in the principle of verification. According to the principle of verification the meaning of a statement is the method of its verification. The generations of philosophers that succeeded that of logical positivism have criticised the verification-principle for a variety of reasons. An obvious point of concern for any empiricist philosopher—whose aim it is to rely as much as possible on observation for the justification of scientific statements—is that definitive empirical verification of any sufficiently general scientific statement is not pos-

[10] The reader might wonder how this solves the riddle. Cannot the validity of the deductive implication be doubted, just as the validity of the deductive inference? Yes, of course it can. But the validity of the implication is a matter of definition, whereas the validity of the inference is a matter of fact. The puzzle is solved by yielding insight into the nature of deduction, not by showing how deduction can be justified.

[11] This point is candidly made by Ayer ([Ayer, 1936], p75 ff). Cf [Feigl, 1950] for the statement that logical principles cannot be justified within logic itself.

sible at all as any finite number of observations can never serve to prove all empirical implications of a general statement.

Reichenbach's approach to verification shows that he realises full well that universal statements cannot be verified. As we saw in the previous chapter Reichenbach introduces the probability theory of meaning, according to which a proposition has meaning iff it can be associated with a well-determined degree of probability. He uses this theory of meaning to show that the relation between scientific observations and the outside world is one of projection and as a result any claim about the outside world has the nature of a posit. The concept of posit deals with the impossibility of verification. A posit is an assumption, a pre-existing conceptual stepping stone, which can be used to argue about the continuation of a regularity thus far observed. The concept of posit plays an important role in Reichenbach's general philosophy, his interpretation of probability, and is essential to his realist view. Reichenbach most certainly did not naively cling to a principle of verification: quite the opposite, avoiding the simple idea of immediate verification and observation is a vital part of Reichenbach's views.

### [5] Evaluation

The last step of the empirical cycle that we shall be concerned with here is that of *evaluation*. Before investigating the concept of evaluation we must answer the question what evaluation is and how it differs from testing. Evaluation is what the scientist must do after a comparison between predicted and observed values has been made. After testing her hypothesis the scientist must decide whether to adjust her hypothesis or to retain it in its present form.

A problem with the evaluation of hypotheses is that if the test of a hypothesis does not yield the predicted result, then we do not know which of the assumptions that were made in order to be able to carry out the test is responsible for this discrepancy. This uncertainty means that no hypothesis can be tested in isolation, which is why the philosopher and historian Pierre Duhem wrote that "A Crucial Experiment Is Impossible In Physics" (as quoted in [Gillies, 1998] on p273).[12]

Duhem's problem (nowadays considered by most as a manifestation of *underdetermination*) becomes much more pressing within Quine's holist view of science. In Quine's view there are no isolated or independent hypotheses; there is only one great web of belief [Quine and Ullian, 1978] in which all hypotheses to which a degree of belief can be assigned have a place and are more or less

[12] We note that Duhem's problem also works in the opposite direction. If the test of a hypothesis does yield the predicted result, then we do not know whether all of the assumptions were correct or that the effects of two or more erroneous assumptions cancel each other out so that the discrepancy disappears.

entangled with all the other hypotheses in the web.[13] As a consequence of the entanglement of hypotheses it is impossible to test a hypothesis in isolation: if the test of a hypothesis yields a discrepancy between the predicted and the observed value the discrepancy may be ascribed to the falsity of any of the hypotheses entangled with the hypothesis under scrutiny ([Quine and Ullian, 1978],p.11).

Let us look at an example. Imagine that we look at the falling stone in our example at several subsequent points in time and infer from our sense impressions the position that the stone has at the different points in time. To make the inference from sense impressions to positions of the stone we must assume that light travels a certain path between us and the stone. In classical, newtonian physics this path is taken to be straight, and in addition we assume that the spacetime through which the light travels has a euclidean geometry. Suppose that $z$ is a variable describing the height of the stone; $t$ a variable describing the time that elapses as the stone falls and $g$ represents the constant gravitational acceleration. Suppose also that we observe $z(t_1) = h$ and $z(t_2) = \frac{h}{2}$. The assumption that newtonian mechanics accurately describes reality entails the belief that there is a gravitational force at work such that $z(t) = \frac{1}{2}gt^2$. It follows from this equation that $t_2 = \sqrt{2}t_1$. We will conclude from the observations $z(t_1) = h$ and $z(t_2) = \frac{h}{2}$ that the ontology[14] is such that $z(t_1) = h$ and $z(t_2) = \frac{h}{2}$ (the stone really is at those positions at those times).

But what if we doubt that newtonian mechanics yields an accurate description of reality? What if we wish to determine whether newtonian physics is valid? Say we are doubtful about the hypothesis that newtonian mechanics describes the stone in our example—call this hypothesis $H_n$. We would perhaps consider the observations $z(t_1) = h$ and $z(t_2) = \frac{h}{2}$ to be a confirmation of $H_n$. It may instead be the case that we observe $z(t_1) = \frac{3}{4}h$ and $z(t_2) = \frac{h}{4}$; what should our conclusion be in this case? While it may be that our first impulse is to reject $H_n$, the experiment does not immediately justify that rejection. Any hypothesis used in the description of the experiment (eg that the position of the observer does not change, or that $g$ is constant during the fall) may be responsible for the discrepancy between values predicted on the basis of $H_e$ and the values that are observed (for example, we may retain $H_n$ even if we observe $z(t_1) = \frac{3}{4}h$ and $z(t_2) = \frac{h}{4}$ if we assume that the position of the observer shifts upwards during the experiment.

From these considerations we might draw the conclusion that the logical structure of the empirical cycle leaves no room for gradual or cumulative scientific progress (in terms of improved predictive accuracy). The test of a hypoth-

[13] Gillies doubts the ubiquity of this entanglement and therefore prefers Duhem's view over that of Quine.

[14] Or at least the *phenomenology*; depending on our stance towards scientific realism.

esis yields results that either correspond or do not correspond to predictions. In case of correspondence we do not know whether all of the assumptions were correct or that the effects of two or more erroneous assumptions cancel each other out. In case of non-correspondence we do not know which of the scientist's assumptions was/were incorrect. Whatever the outcome of our tests, any or all of the scientist's assumptions may have been wrong. What our example shows is that the underdetermination of theories/hypotheses makes it difficult to see how gradual scientific change can come about if the process of science is indeed similar to the process which is described by the empirical cycle.

In the remainder of this section we wish to make two points. Firstly, we want to show that Reichenbach's work contains a specific solution to (or at least treatment of) the holist qualms of Quine and Duhem about geometry in our example of the falling stone. Reichenbach's concept of 'universal forces' and his identification of the methodological rule to set them to zero allows us to address the problem of underdetermination. Secondly, we argue that Reichenbach's idea about the successive approximation ("Stetige Erweiterung"; [Reichenbach, 1920], p69) of science shows how progress of both scientific methodology and science itself is possible regardless of the logical issues that are inherent in empirical research. Reichenbach's concept of successive approximation refers to the assumptions underlying successive scientific theories: as science progresses the assumptions underlying successive theories should be regarded as expansions[15] of assumptions underlying their predecessor theories. Before we explain how that comes about we will focus on Reichenbach's specific solution.

In his 1928 'Philosophy of Space and Time' Reichenbach introduces the concept of 'universal forces'. Forces are universal if they have the following two properties ([Reichenbach, 1928], p13):

1. They affect all materials in the same way.

2. There are no insulating walls.

There will be no empirical difference for us between a universe with a euclidean geometry, E, in which a universal force $F$ works and a universe with a geometry G, as long as $G = E + F$ (see figure 7.3).

With his concept of the universal forces in mind Reichenbach proposes the methodological rule to formulate hypotheses as if universal forces do not

[15] Reichenbach's "Erweiterung" can be translated as either "approximation" or "expansion". In Maria Reichenbach's 1969 translation of [Reichenbach, 1920] we read "approximation". We believe that the term 'approximation' falsely suggests that there is some ultimate description which is the goal of Reichenbach's philosophy.

exist (ie the scientist should assign the value zero to universal forces; [Reichenbach, 1928], p22). We may illustrate Reichenbach's reasoning in this regard by returning to the example in figure 7.3—to a situation in which different geometries cause no empirical difference due to the existence of a universal force.

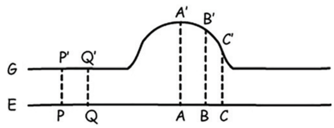


**Figure 7.3:** A universe with a euclidean geometry, E, in which a universal force, F, works would be indistinguishable from a universe with a geometry G as long as F is a universal force and $G = E + F$.

Reichenbach's rule does not solve or bypass underdetermination, but it shows us that Reichenbach is fully aware of the problem of underdetermination and the possibility of different theoretical schemes. Moreover, it represents an attempt to determine what remains of objective knowledge in the face of underdetermination.

It is not our aim here to discuss the merits or deficits of Reichenbach's approach to underdetermination and his rule about universal forces. We want to point out with our analysis that in this respect, too, Reichenbach's thought is far too refined to be the object of the criticism of naivety usually levelled against logical positivism.

Now that we have discussed Reichenbach's specific solution to the problem of underdetermination in the context of spacetime geometry, let us move on to a more general matter. The issue at stake, we remind ourselves, is nothing less than the possibility of gradual scientific change in light of the logical structure of empirical research as embodied by the empirical cycle.

Reichenbach considers the following question: how can measurements guide the scientist towards changes of the very principles that help to define what is a measurement? To explain how this advance comes about, we return again to our example of the falling stone. Suppose we make a repeated measurement of $v_{end}$ (the terminal velocity of the stone) by measuring the depth of the impact-crater the stone creates. We extrapolate the results of our measurements into a continuous distribution that is approximately described by a normal distribution $\phi_1(v)$ with the average $v_{end}$ being $\mu_1$. Imagine that after elaborate further experimentation (perhaps over the course of many years) we have many more results and we again extrapolate our measurements into a normal distribution, $\phi_2(v)$. $\phi_2(v)$ has an average value of $\mu_2$.

In this simple example Reichenbach's successive approximation works as follows. It might seem as if measurements which yield a value of $\mu_2$ that is different from $\mu_1$ contradict the hypothesis that $\mu_1$ represents a physically existing entity. To see that that is not necessarily the case, we must realise that the values of $\mu_1$ and $\mu_2$ are only known up to a certain measurement error: say, $\mu_1 \pm \delta$ and $\mu_2 \pm \delta$. If we find, after further experimentation, a $\mu_2$ that differs

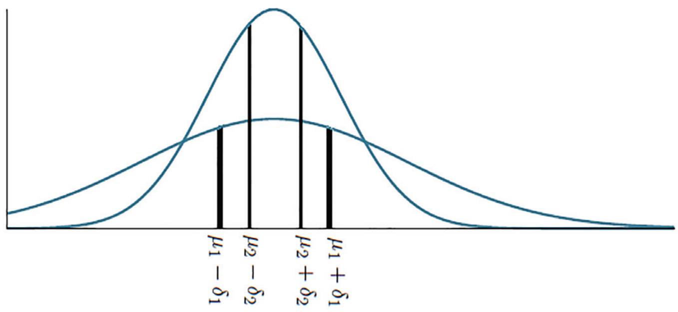


**Figure 7.4:** $\phi_1(v)$ and $\phi_2(v)$ superposed. The graph shows that the inequalities $(\mu_1 - \delta_1) < (\mu_2 - \delta_2) \wedge (\mu_1 + \delta_1) > (\mu_2 + \delta_2)$ leave room for $\phi_2$ to shift laterally.

from $\mu_1$ then we need not reject our hypothesis that $\mu_1$ represents a physically existing entity as long as $[(\mu_1 - \delta_1) < (\mu_2 - \delta_2)] \wedge [(\mu_1 + \delta_1) > (\mu_2 + \delta_2)]$. These inequalities leave room for $\mu_2$ to shift along the v-axis between $\mu_1 - \delta_2$ and $\mu_1 + \delta_2$. As long as the inequalities are obeyed we may say that the model of which $\mu_2$ is a truthful representation is a successive approximation of our earlier model (of which $\mu_1$ is a truthful representation).

In situations in which theory change is involved, similar reasoning can be applied. Think, for example, of the transition from classical mechanics to special relativity. The concept of inertial mass is different in the two theories: in classical mechanics it is a property of material bodies that is independent of velocity, whereas in relativity theory mass increases with velocity. Looking through the glasses of relativity, the scientist sees the world differently than before. However, the two views are compatible from a practical point of view by virtue of the principle of continuous expansion. The measurement results obtained within the old theory are not in conflict with the relativistic results, in the same way as the two sets of measurements discussed above were not conflicting.

We see that Reichenbach's approach answers the question "how can measurements guide the scientist towards changes of the very principles that help to define what is a measurement?" as follows: The principles that underlie measurements can be changed as long as the new principles yield a model that contains the model based on the older principles as an approximation. Reichenbach's approach shows us how gradual scientific change is possible even within a holist view of science.

## 7.3 Conclusion

In this chapter we have seen that Reichenbach in his work is very subtle in addressing various of the issues related to the several steps in the empirical cycle. What this shows is that Reichenbach's 'logical positivist views' are much less naive than critics have often assumed. The criticism from the members of the Frankfurt School and others at the address of logical positivist philosophy is not applicable to the work of Reichenbach. Importantly, our analysis shows that it is precisely the persistent influence of kantian philosophy, with its pre-existing 'paradigmatic' conceptual frameworks, that make Reichenbach's views occupy a corner of the logical positivist school that is remote from the classrooms usually targeted by criticism of logical positivism.[16]

[16] We also believe, but cannot further argue it here, that it is probable that upon investigation of other philosophers (perhaps more closely) associated with the logical positivist school, the claim that logical positivism is a passed station will turn out to be overly simplified.

# Samenvatting in het Nederlands

In dit proefschrift betoog ik dat er sporen van de filosofie van Kant te vinden zijn in de filosofische opvattingen van Hans Reichenbach, en dat deze sporen onder andere aan te treffen en te volgen zijn in Reichenbachs veranderende interpretatie van de waarschijnlijkheidsrekening. Ruwweg is het proefschrift onder te verdelen in twee delen: in de hoofdstukken een, twee en drie worden de concepten waarvan ik gebruik maak in mijn betoog geïntroduceerd; in het vierde tot en met het zevende hoofdstuk behandel ik de opvattingen van Reichenbach. Het tweede deel is opnieuw onder te verdelen: in de hoofdstukken vier, vijf en zes laat ik zien hoe en waar in het werk van Reichenbach de concepten uit het eerste deel aan bod komen, en in hoofdstuk zeven worden Reichenbachs bewerkingen van deze concepten op zo'n manier geordend dat de conclusie van dit proefschrift duidelijk naar voren komt: de sporen van kantiaanse filosofie in Reichenbachs werk zorgen ervoor dat diens opvattingen veel subtieler (en minder vatbaar voor standaard-kritiek) zijn dan doorgaans aangenomen wordt.

## H.1: Mechanicisme en Waarschijnlijkheid

Het eerste hoofdstuk van dit proefschrift heeft de vorm van een opmerking over de opkomst van de waarschijnlijkheidsrekening. Ik denk dat er een spanning is tussen de filosofie van het *mechanicisme* en denken in termen van *waarschijnlijkheid*, en daarom dat het geen toeval is dat tijdens de opkomst van de mechanistische filosofie (ongeveer in de 17$^{e}$ eeuw) ook de klassieke waarschijnlijkheidsrekening tot bloei kwam.

Volgens de mechanistisch filosoof is het een belangrijk doel van de natuurkunde om een mechanistische beschrijving[17] te vinden van de waargenomen werkelijkheid. Een dergelijke mechanistische beschrijving is er een in termen van oorzaak en gevolg; men was op zoek naar de mechanische oorzaak van waargenomen gebeurtenissen. Een filosofisch idee dat nauw verwant is aan de mechanistische filosofie is het idee van determinisme. Determinisme is

[17] [Dijksterhuis, 1950] merkt op dat het gebruik van het adjectief 'mechanistisch' niet consequent is met het gebruik van het substantief 'mechanicisme' (p1, n1).

het idee dat iedere gebeurtenis het noodzakelijke gevolg is van voorgaande gebeurtenissen in combinatie met de natuurwetten[18]. In een mechanistisch wereldbeeld bestaat er voor iedere waargenomen gebeurtenis een achterliggend mechanisme. Dit wereldbeeld zou deterministisch zijn als alle achterliggende mechanismen werken volgens uniek bepalende oorzaak/gevolg-relaties.

De tijd waarin het mechanicisme haar intrede deed, was ook de periode waarin de waarschijnlijkheidsrekening opkwam. Voor de opkomst van het mechanicisme werd de waarschijnlijkheidsrekening beschouwd als een niet al te serieuze tak van de wiskunde, gebruikt door wiskundigen om voordelige strategiën te ontwikkelen voor rijke dobbelaars. In de 17e eeuw ontwikkelde de waarschijnlijkheidsrekening zich tot een veelzijdig instrument (bijvoorbeeld in de rechtspraak) en verscheen er een formalisme ten tonele dat werd gedeeld door de wetenschappelijke wereldgemeenschap[19].

In het eerste hoofdstuk van mijn proefschrift probeer ik aannemelijk te maken dat de opkomst van de mechanistische filosofie en het daarmee gepaard gaande determinisme een vruchtbare voedingsbodem vormde voor de waarschijnlijkheidsrekening. In een wereld waarin alle natuurlijke processen deterministisch verlopen wordt het een prangende vraag wat concepten als kans en waarschijnlijkheid precies inhouden. Waar filosofen voor de 17e eeuw misschien hun toevlucht zochten tot bovennatuurlijke verklaringen voor het optreden van waarschijnlijkheden (waarom de dobbelsteen rolt zoals hij rolt), zochten mechanistisch filosofen naar het mechanisme dat schuilgaat achter waarschijnlijkheid. Ik denk dat de spanning tussen enerzijds het mechanicisme en het determinisme, en anderzijds het denken in termen van kansen en waarschijnlijkheden een grote rol speelt in de ontwikkeling van de waarschijnlijkheidsrekening (of in ieder geval van het formalisme dat daarbij hoort).

## H.2: A Priori

Hoofdstuk twee van dit proefschrift lijkt een nieuw startpunt. De opmerking over de relatie tussen waarschijnlijkheid en het mechanicisme is gemaakt en we gaan nu in op een schijnbaar heel ander onderwerp. Hoofdstuk twee gaat over de rol van het concept *a priori* in de wetenschap. Echter, ondanks de schijn van het tegendeel is dit hoofdstuk niet een nieuw startpunt. We zullen zien dat de inhoud van dit hoofdstuk samen met inhoud van het derde hoofdstuk een ideale opstap is voor onze bestudering van de 20e eeuwse waarschijnlijkheidsrekening.

[18] [Hoefer, 2016].

[19] Zie voor een toepassing van de waarschijnlijkheidsrekening in de rechtspraak Wilhelm Leibniz' 'Dissertatio De Arte Combinatoria' uit 1666. Het formalisme werd ook buiten Europa gedeeld; zie Mikami's 'Development of Mathematics in China and Japan' ([Mikami, 1913]).

## Kants A Priori

De ontwikkeling van het a priori-concept kent meerdere stadia. Een geschikt beginpunt voor onze analyse is de betekenis die het a priori kreeg in het werk van Kant (in de $18^{e}$ eeuw). Voor deze analyse bekijken we Kants *transcendentale filosofie*. Centraal hierin staat de transcendentale gedachte—het idee dat *als* kennis van de wereld mogelijk is de wereld zó in elkaar moet zitten dat deze kennis mogelijk is.

Al voor de tijd van Kant werd er een onderscheid gemaakt tussen uitspraken die *analytisch* zijn en uitspraken die *synthetisch* zijn. De waarheid van analytische uitspraken hangt af van de betekenis van de termen in de uitspraak (bv. 'vrijgezellen zijn ongetrouwd'). De waarheid van synthetische uitspraken hangt niet alleen af van de betekenis van de termen in de uitspraak maar ook van iets in de wereld buiten degene die de uitspraak doet (bv. 'de appel is rood').

Om dit verschil te beschrijven maakte Kant gebruik van de twee termen *a priori* en *a posteriori*. Kant gebruikte deze termen op een iets andere manier dan ze gebruikt werden door filosofen voor zijn tijd. Kant gebruikte de termen om een strikt onderscheid te maken tussen twee soorten uitspraken: uitspraken waarvan de waarheid onafhankelijk van waarnemingen vastgesteld kan worden noemde Kant a priori (vóór de waarneming kan de (on)waarheid vastgesteld worden), en uitspraken waarvoor waarnemingen gedaan moeten worden om te kunnen vaststellen of ze waar zijn noemde Kant a posteriori (pas na waarneming kan de waarheid worden vastgesteld).

Met deze twee onderverdelingen van uitspraken (analytisch/synthetisch en a priori/a posteriori) in gedachten kunnen we beschrijven wat Kants transcendentale gedachte inhoudt. Op het eerste gezicht lijkt het aannemelijk dat analytische uitspraken a priori zijn (voor een analyse van termen is immers geen waarneming nodig), en synthetische uitspraken a posteriori. Kant meende dat als kennis van de wereld mogelijk is er naast analytische a priori uitspraken en synthetische a posteriori uitspraken nog een derde categorie uitspraken moet bestaan: synthetische a priori uitspraken. Een synthetische a priori uitspraak zegt iets over de wereld buiten degene die de uitspraak doet, maar er is geen waarneming vereist om de uitspraak te kunnen toetsen.

Kants gedachte kan als volgt worden toegepast. Kant stelde dat de wereld (de natuur) de eigenschappen moet hebben die kennis van deze wereld mogelijk maken. Stel je bijvoorbeeld een vallende steen voor: je denkt dan waarschijnlijk aan een min of meer bolvormig klompje materie dat op verschillende momenten op verschillende plaatsen is. Het is duidelijk dat de kleur van het klompje materie, of de snelheid waarmee het valt, niet erg belangrijk is voor de mogelijkheid van de voorstelling (we kunnen ons best een steen met een andere kleur of met een andere snelheid voorstellen[20]).

Er zijn volgens Kant ook eigenschappen van de vallende steen die essentieel zijn voor de voorstelling. Eigenschappen die zo belangrijk zijn, dat zonder die eigenschappen je je simpelweg geen voorstelling kunt maken. Zo kun je je volgens Kant geen voorstelling maken van zoiets als een vallende steen of bijvoorbeeld botsende biljartballen zonder te denken aan objecten die ruimte innemen en veranderen in de tijd. Om fysische toestanden op verschillende momenten met elkaar in verband te brengen (bijvoorbeeld de posities en snelheden van vallende stenen of van botsende biljartballen) is het volgens Kant noodzakelijk om aan te nemen dat de fysische toestand van de wereld op ieder moment noodzakelijkerwijs wordt teweeggebracht door een combinatie van de voorgaande fysische toestand van de wereld met de natuurwetten. In de moderne wetenschapsfilosofie beschrijft men een dergelijke relatie tussen toestanden met een term die we al eerder zijn tegengekomen: determinisme. In het wereldbeeld van Kant komt een deterministische relatie voort uit een uitzonderingsloze opeenvolging van oorzaak en gevolg. Niet alleen is iedere gebeurtenis de oorzaak van een andere gebeurtenis, ook is het zo dat gelijke oorzaken altijd dezelfde gevolgen hebben.

## Kritiek op Kant

Kant was ervan overtuigd dat als kennis van de wereld mogelijk is, we moeten aannemen dat waarneembare gebeurtenissen geordend zijn in termen van tijd, ruimte en oorzakelijkheid. Kennis in de tijd van Kant is kennis binnen de newtoniaanse mechanica, die op haar beurt gestoeld is op de meetkunde van Euclides. De euclidische meetkunde zoals zij ten grondslag ligt aan de newtoniaanse mechanica beschrijft de ruimte als vlak. Het is daarom niet verwonderlijk dat een deel van Kants overtuiging erin bestond dat—als kennis mogelijk is—de natuurkunde gestoeld moet zijn op de euclidische meetkunde. In de kantiaanse filosofie leidt de transcendentale gedachte tot het inzicht dat de ruimte zoals wij haar waarnemen vlak is.

Kants ideeën over ruimte werden in de 18$^{\mathrm{e}}$ en 19$^{\mathrm{e}}$ eeuw bekritiseerd. Deze kritiek werd kracht bijgezet toen in de tweede helft van de 19$^{\mathrm{e}}$ eeuw werd ondekt dat euclidische meetkunde niet de enige consistente meetkunde is. De ontkenning van Euclides' vijfde postulaat (het zogenaamde parallellenpostulaat) leidde tot het idee dat er consistente, niet-euclidische meetkundes bestaan. Omdat deze zogenaamde riemanniaanse meetkundes kunnen worden gebruikt voor het beschrijven van een *gekromde* ruimte werd de twijfel over Kants aanname dat de ruimte niet gekromd is groter. Kunnen we gekromde ruimte wel a priori—onafhankelijk van waarnemingen—uitsluiten?

[20]Of zelfs zonder kleur (in het donker) of snelheid (we kunnen ons zelfs een steen voorstellen die stil lijkt te liggen maar in werkelijkheid valt).

De ontdekking dat niet-euclidische meetkunde een consistent onderdeel van de wiskunde is doet de vraag rijzen of gekromde ruimte misschien (in weerwil van Kants oorspronkelijke filosofie) een consistent onderdeel van de natuurkunde zou kunnen zijn. In de newtoniaanse mechanica is het een basisaanname dat de ruimte niet gekromd is, maar is die aanname essentieel voor de mechanica—welke vorm deze dan ook heeft?

Deze vraag werd ondubbelzinnig beantwoord toen in het begin van de 20$^{e}$ eeuw de relativiteitstheorieën werden ontdekt[21] door Einstein. In de algemene relativiteitstheorie is een vlakke ruimte niet langer een basisaanname en alle realistische modellen in deze theorie (waarin de ruimte niet leeg is) gaan zelfs uit van een gekromde ruimte.

## Reichenbachs A Priori

De fysicus en filosoof Hans Reichenbach schreef in 1920 een boek waarin hij poogde de kantiaanse transcendentale filosofie zodanig aan te passen dat zij de relativiteitstheorie niet langer tegenspreekt. In de inleiding van zijn boek stelt Reichenbach dat we Kants transcendentale gedachte tegemoet moeten komen, maar dat het overweldigende empirische bewijs voor de relativiteitstheorie niet genegeerd kan worden. Na een analyse van Kants transcendentale filosofie komt Reichenbach tot een aanpassing van Kants ideeën over het synthetische a priori.

Reichenbach ziet in dat Kants synthetische a priori twee verschillende aspecten heeft. Ten eerste heeft Kants synthetische a priori een *constitutief* karakter, en ten tweede is kants synthetische a priori *apodictisch*. Het constitutieve karakter bestaat erin dat het synthetische a priori de fysische voorstelling constitueert. Om een voorbeeld te geven keren we terug naar de vallende steen waar we het eerder over hadden. De functie van constitutieve concepten als ruimte en tijd is anders dan die van andere concepten (bijvoorbeeld kleur) omdat constitutieve concepten een zodanige ordening aanbrengen in gedane waarnemingen dat we zonder deze concepten niet van een voorstelling zouden kunnen spreken. Het andere aspect van Kants synthetische a priori dat Reichenbach ontwaarde was het apodictisch aspect. In de kantiaanse filosofie moeten ware synthetische a priori uitspraken altijd waar zijn, juist omdat ze constitutief zijn. Om de mogelijkheid van kennis te allen tijde te kunnen waarborgen moet Kants synthetische a priori onveranderlijk (apodictisch) zijn.

Reichenbach staat positief tegenover Kants transcendentale gedachte. Om kennis van de wereld mogelijk te maken moet de wereld de eigenschappen hebben die deze kennis mogelijk maken. Maar omdat we onze kennis van de wereld kunnen herzien, zo stelt Reichenbach, moeten we ook van tijd tot tijd onze ideeën over de kennis-mogelijk-makende (constitutieve) eigenschappen

[21] Kant zou misschien hebben gezegd dat de relativiteitstheorieën werden *uitgevonden*.

kunnen herzien. De empirische wetenschap vertelt ons welke eigenschappen constitutief zijn. Voortschrijdende wetenschap kan dus leiden tot herziening van de constitutieve eigenschappen (zoals de algemene relativiteitstheorie heeft laten zien dat een vlakke ruimte geen constitutieve eigenschap van alle mogelijke natuurkundige theoriën is).

Na de bovenstaande analyse betoogt Reichenbach dat we niet zouden moeten aannemen dat er een synthetisch a priori bestaat dat onveranderlijk is. In wat Kant synthetisch a priori noemde, ziet Reichenbach een constitutief maar veranderlijk a priori. Het idee van Reichenbach is in latere wetenschapsfilosofische literatuur bekend komen te staan als *gerelativeerd a priori*—omdat het slechts a priori is met betrekking tot een welbepaalde wetenschappelijke context.

We zagen in het lijstje van Kants synthetische a priori's de concepten ruimte, tijd en een deterministische oorzakelijkheid. Reichenbach vervangt Kants idee van deterministische oorzakelijkheid met het idee van een veranderlijk constitutief a priori: Reichenbachs *principe van de waarschijnlijkheidsfunctie*. In het vierde en vijfde hoofdstuk van mijn proefschrift beschrijf ik hoe in Reichenbachs werk een deterministische oorzakelijkheid stapsgewijs plaatsmaakt voor de waarschijnlijkheidsfunctie, maar om deze overgang nauwgezet te kunnen volgen zal ik eerst een hoofdstuk wijden aan wat bekend is komen te staan als de *klassieke waarschijnlijkheid*: de waarschijnlijkheidsrekening zoals zij werd beoefend in de 18$^{e}$ en 19$^{e}$ eeuw.

## H.3: Klassieke Waarschijnlijkheid

De *locus classicus* voor de klassieke waarschijnlijkheid is het werk van P.S. Laplace. Volgens de klassieke benadering van het kansbegrip zegt waarschijnlijkheid iets over de ontwikkeling van een fysisch proces. De waarschijnlijkheid van een bepaalde uitkomst van zo'n proces wordt gedefiniëerd als de verhouding tussen het aantal mogelijke uitkomsten dat als 'gunstig' wordt beschouwd en het totale aantal mogelijke uitkomsten. Als we bijvoorbeeld werpen met een dobbelsteen en vragen naar de kans op een even getal, dan zijn er drie gunstige gevallen (omdat er drie even getallen onder de zeven zijn) en zes mogelijke gevallen (ervan uitgaande dat de dobbelsteen zes zijden heeft). De kans waarnaar gevraagd wordt is dus gelijk aan $\frac{3}{6} = \frac{1}{2}$. De toepassing van deze definitie vooronderstelt dat alle mogelijke gevallen (en dus ook alle gunstige gevallen) even waarschijnlijk zijn—maar hoe kunnen we deze vooronderstelling rechtvaardigen?

In de klassieke benadering werd aangenomen dat twee uitkomsten even waarschijnlijk zijn als we geen enkele reden hebben om aan te nemen dat de waarschijnlijkheid van de één groter is dan die van de ander. Deze aanname (ook wel *het principe van indifferentie* genoemd) lijkt op het eerste gezicht aannemelijk: is dat niet wat we allemaal dagelijks doen? Als we over één of ander fysisch proces niets anders weten dan dat het zich op $n$ verschillende manieren kan ontwikkelen, dan gaan we er toch vanuit dat iedere ontwikkeling een waarschijnlijkheid van $\frac{1}{n}$ heeft?

Sommige theoretici in de tweede helft van de $19^{\mathrm{e}}$ eeuw stelden dat het kansbegrip alleen maar waardevol voor de wetenschap is als kansen objectief vastgesteld kunnen worden (zodat wetenschappers onder gelijke omstandigheden gelijke kansen zullen toekennen). Daarom werd gesteld dat het principe van indifferentie alleen zou moeten worden toegepast als er sprake is van een meetbare fysische symmetrie: pas als we kunnen meten dat de dobbelsteen die we werpen homogeen is en de afmetingen van de zijden niet van elkaar verschillen, mogen we aannemen dat alle mogelijke uitkomsten van de worp even waarschijnlijk zijn. Ik bespreek in dit verband de opvatting van Johannes von Kries.

Von Kries meende dat waarschijnlijkheden die wij gewoonlijk associëren met fysische toestanden kunnen worden uitgedrukt als verhoudingingen tussen aantallen mogelijke beschrijvingen van deze fysische toestanden op microniveau. Zo kenmerkt von Kries de kans op een zes bij een worp met een eerlijke dobbelsteen als volgt. De gezochte kans is gelijk aan de verhouding tussen 1) het aantal 'oorspronkelijke' en 'enkelvoudige' microtoestanden die ieder overeenkomen met een zes als uitkomst en 2) het totale aantal microtoestanden die ieder overeenkomen met een mogelijke uitkomst van de worp. Gezien het feit dat de dobbelsteen in kwestie eerlijk is, lijkt het redelijk om aan te nemen dat von Kries op een kans van $\frac{1}{6}$ zou uitkomen. Omdat von Kries redeneert binnen een deterministisch wereldbeeld is er tussen iedere microtoestand die een begintoestand voor een fysisch proces vormt een oorzakelijk verband met een microtoestand die een eindtoestand beschrijft. In het geval van de dobbelsteenworp kan iedere uitkomst dus niet alleen worden geassociëerd met een bereik aan micro-eindtoestanden, maar ook met een bereik aan micro-begintoestanden. Von Kries was van mening dat zijn karakterisering van waarschijnlijkheden in termen van (in principe meetbare) fysische microtoestanden het principe van indifferentie overbodig maakt.

In het werk van Laplace en von Kries vormt een deterministisch wereldbeeld het startpunt van bespiegelingen over kansrekening. Een hypothetische entiteit (god?) die de begintoestand van het hele universum kent zou in een deterministische gang van zaken alles wat er gebeurt kunnen voorspellen. Echter, wij kennen van een natuurlijk proces nooit de gehele begintoestand. Er zal daarom altijd onzekerheid bestaan over het daadwerkelijke verloop van een fysisch proces. Vergelijk dit met een dobbelsteenworp. Ook al zouden we alle natuurwetten en de toestand van de dobbelsteen zelf tot in detail kennen, dan nog kunnen we—zelfs als de wereld deterministisch is—niet met zekerheid zeggen wat de uitkomst van de worp zal zijn. De zojuist gekarakteriseerde waarschijnlijkheid levert een maat voor deze onzekerheid, en zorgt ervoor dat we ondanks onzekerheid wetenschappelijke uitspraken kunnen doen over het verloop van een proces.

## H.4: Reichenbachs Proefschrift

We keren nu terug naar Reichenbach. Een paar jaar voordat hij het boek schreef waarin hij probeerde de spanning tussen de relativiteitstheorie en de filosofie van Kant weg te nemen, schreef Reichenbach zijn proefschrift. Dit werk, dat in 1916 werd gepubliceerd, is geschreven geheel vanuit een kantiaans perspectief: Reichenbach zoekt in 1916 naar de eigenschappen die de wereld moet hebben opdat kennis mogelijk is. Eerder zagen we dat (volgens Kant) onze waarnemingen moeten worden geordend in termen van tijd, ruimte en determinisme om kennis mogelijk te maken. Ook zagen we dat een deterministische gang van zaken niet per se leidt tot voorspelbaarheid: doordat we nooit de gehele begintoestand van een fysisch systeem kennen, zullen er altijd verstoringen van het (door ons beschrevene) deterministisch verloop optreden. Waarschijnlijkheid beschrijft de resulterende onzekerheid over het verloop. Reichenbach redeneert daarom als volgt. Door de reeds genoemde verstoringen is iedere uitspraak over een meting in de natuurkunde een waarschijnlijkheidsuitspraak. Reichenbachs aanpassing van Kants transcendentale gedachte is daarom dat—als kennis mogelijk is—we moeten aannemen dat waarschijnlijkheidsuitspraken mogelijk zijn. Reichenbach analyseert daarom de waarschijnlijkheidsrekening.

Reichenbach onderzoekt von Kries' definitie van waarschijnlijkheid en komt tot een punt van kritiek. Hij oordeelt dat er twee problemen zijn met diens beschouwingen over microtoestanden. Ten eerste leiden von Kries' microtoestanden volgens Reichenbach tot het problematiseren van de rationele basis voor waarschijnlijkheidsuitspraken. Von Kries beweert wel dat hij een fysische, objectieve theorie heeft (en dus geen gebruik hoeft te maken van het principe van indifferentie), maar hij neemt aan dat al zijn 'oorspronkelijke' microtoestanden even waarschijnlijk zijn. Deze aanname over de microtoestanden vloeit (net als het principe van indifferentie) niet logisch voort uit de fysische beschrijving van de microtoestanden. De aanname is dus subjectief, en subjectieve kennis, zo oordeelt Reichenbach, is geen goede basis voor wetenschappelijk-rationele uitspraken.

Het tweede probleem dat Reichenbach heeft met von Kries' waarschijnlijkheid is dat diens aanname dat alle microtoestanden even waarschijnlijk zijn een *ad hoc*-aanname is: de aanname lijkt te zijn gedaan om waarschijnlijkheid te kunnen definiëren als een verhouding tussen aantallen microtoestanden en niet omdat de aanname op natuurlijke wijze past binnen een overkoepelend wetenschapsfilosofisch systeem waarvan de waarschijnlijkheidsrekening een onderdeel is.

Om deze beide problemen in een keer op te lossen stelt Reichenbach zijn eigen interpretatie van waarschijnlijkheid voor. We zullen zien dat Reichenbachs kantiaanse interpretatie van waarschijnlijkheid de aanname dat alle microtoestanden van von Kries even waarschijnlijk zijn overbodig maakt en waarschijnlijkheidsinschattingen tot een coherent onderdeel van een kantiaanse interpretatie maakt.

Reichenbach begint, net als de aanhangers van de klassieke waarschijnlijkheid, met de aanname dat alle fysische processen deterministisch verlopen. Veel wetenschappelijke uitspraken kunnen alleen worden gedaan in de vorm van waarschijnlijkheidsuitspraken, aldus Reichenbach, omdat er in het verloop van ieder fysisch proces altijd invloeden zijn van buiten de beschreven begintoestand die een deterministische beschrijving van het proces onbruikbaar maken[22]. Reichenbachs voorstel is om waarschijnlijkheid te definiëren in termen van de relatieve frequenties van waarnemingen. Stel dat we van een bepaalde gebeurtenis de waarschijnlijkheid willen bepalen en we bekijken $n$ situaties waarin de gebeurtenis mogelijkerwijs optreedt. De waarschijnlijkheid van de gebeurtenis is in de definitie van Reichenbach de limiet van de relatieve frequentie van deze gebeurtenis als $n \to \infty$. Een waarschijnlijkheidsuitspraak à la Reichenbach vooronderstelt het bestaan van limiet-frequenties; Reichenbachs waarschijnlijkheidsrekening moet dus van het bestaan van limiet-frequenties uitgaan. Deze limiet-frequenties spelen een *constitutieve* rol in waarschijnlijkheidsuitspraken.

In Reichenbachs benadering moet dus iedere beschrijving van een fysisch proces waarin waarschijnlijkheden een rol spelen worden geassocieerd met limiet-frequenties in oneindige rijen van herhalingen. Reichenbach noemt de aaneenschakeling van deze limiet-frequenties de waarschijnlijkheidsfunctie, $\phi$. We zullen zometeen zien dat als we aannemen dat $\phi$ een *continue* functie is, von Kries' aanname van gelijke waarschijnlijkheid van oorspronkelijke microtoestanden overbodig is. De mogelijkheid van waarschijnlijkheidsuitspraken (en dus van de meeste natuurkundige uitspraken) vereist alleen de aanname dat $\phi$ bestaat—deze aanname is volgens Reichenbach in 1916 daarom synthetisch a priori.

Reichenbach is ervan overtuigd dat hij met zijn kantiaanse benadering de problemen met von Kries' benadering van de waarschijnlijkheidsrekening kan oplossen. Als we waarschijnlijkheid definiëren zoals Reichenbach dat doet, is de waarschijnlijkheid van een gebeurtenis een extrapolatie van de relatieve

[22]Pas in zijn latere werk stelt Reichenbach de aanname ter discussie dat de resulterende niet-deterministische beschrijving een proces beschrijft dat op fundamenteel niveau deterministisch is.

frequentie van de gebeurtenis in onze waarnemingen. We kunnen deze opvattingen van Reichenbach nu vergelijken met von Kries' benadering van de waarschijnlijkheidsrekening.

Vertaald naar Reichenbachs benadering is von Kries' aanname dat alle oorspronkelijke microtoestanden van een proces even waarschijnlijk zijn equivalent aan de aanname dat wanneer het proces vaak wordt herhaald, de waargenomen relatieve frequenties kunnen worden geëxtrapoleerd naar precies bepaalde limiet-frequenties, waarin de oorspronkelijke gelijke kansen tot uitdrukking komen. Zoals we hebben gezien, stelt Reichenbach daarvoor een andere aanname in de plaats: als waarschijnlijkheidsuitspraken mogelijk zijn, bestaat er een continue waarschijnlijkheidsfunctie. Deze aanname over $\phi$ is zwakker dan von Kries' aanname. Als $\phi$ bestaat dan bestaan er limiet-frequenties, maar Reichenbachs benadering zegt niets over de specifieke waarden van deze limiet-frequenties. Wel stelt Reichenbach dat $\phi$ de vorm van een continue functie heeft. De continuïteit van $\phi$ impliceert dat kleine (infinitesimale) veranderingen in de uitkomsten zich vertalen naar kleine veranderingen in de kansen op die uitkomsten (de limiet-frequenties). Omdat $\phi$ ons vertelt dat dicht bij elkaar gelegen uitkomsten vrijwel gelijke waarschijnlijkheden bezitten, heeft Reichenbach von Kries' aanname over de kansen van oorspronkelijke microtoestanden niet langer nodig.

Het laten zien dat von Kries' aanname van gelijke aanvangswaarschijnlijkheden niet gedragen wordt door de fysische basis van waarschijnlijkheidsuitspraken is gelijk de eerste stap van Reichenbachs oplossing van het tweede probleem. Het rest hem alleen nog te laten zien dat het principe dat in plaats komt van von Kries' aanname wél op natuurlijke wijze past binnen een overkoepelende (filosofische) beschouwing over kansen. Door af te leiden dat de aanname dat $\phi$ bestaat synthetisch a priori is, laat Reichenbach zien dat $\phi$ past binnen een (neo)kantiaans wetenschapsfilosofisch systeem.

Ik betoog dat het eerste punt van Reichenbach leidt tot een probleem in de toepassing van zijn frequentistische interpretatie. Reichenbach beperkt zich tot de aanname dat $\phi$ bestaat, zonder iets te zeggen over de specifieke waarden die $\phi$ aanneemt. Deze beperking door Reichenbach ontdoet waarschijnlijkheid inderdaad van subjectiviteit: de willekeur besloten in het principe van indifferentie en von Kries' aanname van gelijke waarschijnlijkheid is niet langer onderdeel van de waarschijnlijkheidsrekening. Echter, met het onderdrukken van de subjectiviteit krijgt ook de mogelijkheid van systematische en gerechtvaardigde vorming van verwachtingen een klap. Voor iemand die een inschatting wil maken van de waarschijnlijkheid van een gebeurtenis is Reichenbachs benadering niet afdoende; Reichenbachs benadering zegt immers niets over hoe uit waarnemingen geëxtrapoleerd moet worden: het bestaan van een limiet zegt niets over de snelheid waarmee die limiet benaderd wordt. Reichenbachs benadering van de waarschijnlijkheid is weliswaar objectief, maar leidt tot een rechtvaardigingsprobleem voor kansuitspraken op basis van eindige hoeveelheden empirisch materiaal.

Reichenbach was niet de enige die waarschijnlijkheden definieerde in termen van limieten van relatieve frequenties. In het vijfde hoofdstuk van mijn proefschrift laat ik zien dat de interpretatie van waarschijnlijkheden in termen van limieten van relatieve frequenties op verschillende manieren kan gebeuren.

## H.5: Variaties op het Frequentisme

In de voorgaande hoofdstukken zijn we verschillende werken van Reichenbach tegengekomen (in niet-chronologische volgorde). In 1916 wordt zijn proefschrift gepubliceerd, waarin hij een volledig kantiaanse interpretatie van de waarschijnlijkheidsrekening uiteenzet; en in 1920 schrijft Reichenbach het boek waarin hij zijn algemene kantiaanse epistemologie omvormt tot een neokantiaanse—nu met een gerelativeerd a priori. In dit hoofdstuk zullen we de frequentistische interpretatie van waarschijnlijkheid bestuderen die resulteert als Reichenbach zijn neokantiaanse interpretatie verder verfijnt.

In 1925 schrijft Reichenbach een artikel waarin hij de conclusie trekt dat als we aannemen dat $\phi$ bestaat, de aanname dat fysische processen deterministisch verlopen niet meer nodig is voor het mogelijk maken van natuurwetenschappelijke kennis. In kantiaanse termen: Reichenbach realiseert zich dat determinisme niet een noodzakelijke constitutieve voorwaarde is voor de kennis die wij hebben. Zelfs in een indeterministische wereld is wetenschappelijke kennis mogelijk.

In 1935 introduceert Reichenbach een frequentistische interpretatie van waarschijnlijkheid die niet uitgaat van deterministische processen. De waarschijnlijkheid die Reichenbach definieert is een relatie tussen twee klassen en een oneindige rij van paren van elementen. Waarschijnlijkheid wordt als volgt gedefinieerd: de waarschijnlijkheid $P(B|A)$ is de verhouding tussen 1) het aantal paren in de rij waarvan het eerste element behoort tot een bepaalde klasse $A$ en het tweede element tot een klasse $B$ en 2) het totale aantal paren in de rij waarvan het eerste element behoort tot klasse $A$. Omdat Reichenbach waarschijnlijkheid op deze wijze kenmerkt als eigenschap van een oneindige rij van *paren* van elementen kunnen we zeggen dat zijn formele opbouw van de waarschijnlijkheidsrekening het begrip *conditionele waarschijnlijkheid* als fundamenteel neemt.

Reichenbachs frequentisme wordt zo formeel gekarakteriseerd als een theorie over elementen in verzamelingen. Deze theorie moet natuurlijk worden toegepast op fysische gebeurtenissen, maar een dergelijke toepassing is een extra stap die niet uit het formalisme volgt. Dit noopt Reichenbach ertoe uit te leggen hoe de relatie is tussen waarnemingen en waarschijnlijkheidsuitspraken. Waarschijnlijkheidsuitspraken zijn een 'extrapolatie' van waargenomen relatieve frequenties—ze zijn ampliatief en volgen niet deductief uit het waarnemingsmateriaal. Reichenbach beschouwt zulke uitspraken als *'posits'*, d.w.z. als proposities die we moeten behandelen als ware uitspraken ondanks dat we ze niet kunnen bewijzen—als we zulke posits niet zouden aannemen, zouden we geen basis hebben om verder te redeneren. Wederom zien we hier het kantiaanse spoor in Reichenbachs denken. Zijn waarschijnlijkheidsposits zijn een onlosmakelijk onderdeel van toepassingen van de waarschijnlijkheidsrekening: zij constitueren onze probabilistische behandeling van de werkelijkheid. Net als de waarschijnlijkheidsfunctie past ook het idee van de posit naadloos in een (neo)kantiaans raamwerk: het aannemen ervan is een condities waaraan moet zijn voldaan om wetenschappelijke kennis mogelijk maken.

Een andere frequentistische interpretatie is die van Richard von Mises. Volgens von Mises is de waarschijnlijkheidsrekening, anders dan bij Reichenbach, een inductieve fysische theorie over reeksen van gebeurtenissen. De spanning tussen 1) von Mises' idee dat de waarschijnlijkheidstheorie een fysische theorie is en 2) het idee dat waarschijnlijkheid een limietfrequentie is in oneindig lange (en dus onwaarneembare) reeksen wordt door von Mises afgedaan met de opmerking dat waarschijnlijkheidsuitspraken een hypothetische aard hebben. Dat is niet ongewoon voor een fysische theorie, aldus von Mises, maar vergelijkbaar met uitspraken in bijvoorbeeld de newtoniaanse mechanica. Een belangrijk verschil met Reichenbach is dat von Mises waarschijnlijkheden beschouwt als de uitkomsten van de toepassing van standaard inductieve methoden op de relative frequenties die worden gevonden in eindige reeksen. Dit is een puur empiristische benadering. Reichenbach daarentegen meent dat het onmogelijk is over waarschijnlijkheden te spreken zonder het maken van a priori veronderstellingen (die later eventueel kunnen worden aangepast op basis van feitenmateriaal).

Volgens von Mises is waarschijnlijkheid alleen gedefinieerd in reeksen gebeurtenissen als deze reeksen *kollektieven* zijn. Von Mises' voorwaarde voor het bestempelen van een reeks gebeurtenissen als een kollektief is dat de elementen in zo'n reeks *willekeurig* verdeeld zijn. Deze willekeur bestaat in het uitgesloten zijn van een spelstrategie: als ieder element in de reeks de uitkomst van een kansspel zou voorstellen bestaat er geen spelstrategie die gegarandeerd leidt tot winst van het spel.

In 1949 schrijft Reichenbach een brief aan Bertrand Russell. De brief is een reactie op een boek van Russell waarin deze het $20^{\mathrm{e}}$ eeuwse frequentisme benoemt als de 'Mises-Reichenbach-theorie'. Reichenbach ziet duidelijke verschillen tussen zijn interpretatie en die van von Mises en vindt het daarom ongepast dat Russell het heeft over de Mises-Reichenbach-theorie. Zoals we hebben gezien zijn er inderdaad verschillen tussen de benaderingen van Reichenbach en von Mises. Terwijl von Mises' frequentisme in beginsel een fysische theorie is, is Reichenbachs theorie een in de eerste plaats formele theorie die kan worden toegepast op fysische verschijnselen als aan bepaalde voorwaarden is voldaan. Deze toepassingsstap onderscheidt Reichenbachs interpretatie van die van von Mises.

## H.6: Sporen van Transcendentaliteit

In de twee voorgaande hoofdstukken is er een continuïteit te bespeuren in de opvattingen van Reichenbach. Sporen van zijn vroege kantiaanse benadering doen zijn latere frequentistische interpretatie afwijken van de interpretatie van von Mises. In hoofdstuk zes bespreek ik de kantiaanse sporen in het werk van Reichenbach explicieter.

In Reichenbachs frequentistische interpretatie in 1916 zijn zowel determinisme en het bestaan van een continue $\phi$ synthetisch a priori in de kantiaanse zin. In 1925 laat Reichenbach het determinisme achter zich en is het synthetisch a priori afgezwakt tot een gerelativeerd a priori. In een persoonlijke notitie uit 1927 schrijft Reichenbach een 'zelfbeoordeling' waarin hij stelt dat het bestaan van een continue $\phi$ wordt verondersteld in alle natuurkundige uitspraken. We kunnen opmerken dat dit spreken van vooronderstellingen die nodig zijn voor de mogelijkheid van uitspraken typerend is voor (neo)kantiaans denken.

Een spoor van kantianisme dat nog niet eerder aan bod is gekomen is Reichenbachs opvatting over *conventies*. Moritz Schlick was van mening dat datgene wat Reichenbach het synthetisch a priori noemde niets anders is dan een conventie en niets met kantianisme te maken heeft. Schlick verweet het Reichenbach daarom dat deze kantiaanse termen als constitutiviteit gebruikte. Reichenbach nam hierna de term conventie van Schlick over, maar bleef—ook in zijn latere werk (o.a. dat uit 1928)—de constitutieve rol van conventies in wetenschappelijke kennis benadrukken. Dit constitutieve karakter van Reichenbachs conventies illustreert opnieuw het kantiaanse spoor in Reichenbachs filosofie.

Nauw verbonden aan Reichenbachs idee van waarschijnlijkheidsuitspraken als posits, zoals we dat in het vorige hoofdstuk tegenkwamen, is Reichenbachs idee uit 1935 van de *vindicatie van inductie*. In Reichenbachs benadering zijn alle waarschijnlijkheidsuitspraken posits en niet deductief te rechtvaardigen. Desalniettemin kunnen we achteraf gelijk krijgen bij het redeneren op grond van deze posits, namelijk wanneer we tot succesvolle voorspellingen komen. Reichenbach stelt nu dat dat deze situatie essentieel dezelfde is als die we tegenkomen bij de rechtvaardiging van inductie. Als er ware uitspraken over de toekomst kunnen worden gedaan, dan zullen bepaalde inductieve posits tot deze uitspraken voeren—zonder het gebruik van zulke posits is er geen inductieve kennis mogelijk, volgens Reichenbach. In Reichenbachs benadering zijn inductieve posits constitutief voor kennis. We kunnen ze niet deductief rechtvaardigen, maar moeten ze aannemen om vooruit te kunnen komen in redeneringen over de toekomst. Achteraf kunnen onze posits worden bekrachtigd door vindicatie, namelijk wanneer ze leiden tot juist blijkende verwachtingen. Reichenbachs vervanging van rechtvaardiging door vindicatie—berustend op het idee van constitutiviteit en uitgaande van voorwaarden die kennis mogelijk maken—is geheel in de geest van de kantiaanse filosofie.

Het laatste—en misschien wel belangrijkste—spoor van kantianisme in Reichenbachs latere werk is zijn *realisme* (uit 1938). Reichenbachs realisme is zijn idee dat er een werkelijkheid bestaat die zich openbaart in onze waarnemingen en dat uitspraken over deze werkelijkheid een welgedefinieerde betekenis hebben. Reichenbach is het hierin dus niet eens met het deel van zijn tijdgenoten voor wie de metafysica anathema is. Reichenbach verdedigt zijn realistische visie door het concept *projectie* in te voeren, het uitgaan boven het empirisch materiaal door een posit aan te nemen die dit materiaal verklaart. De invoering van dit concept kan worden gezien als een stap binnen een neokantiaanse benadering. Zonder projectie, en dus de aanname van een grond voor onze waarnemingsgegevens, is natuurwetenschappelijke kennis over de realiteit niet mogelijk. De aanname van een realiteit buiten de waarneming is dus constitutief voor wetenschappelijke kennis.

In Reichenbachs latere opvattingen is de relatie tussen wetenschappelijke waarnemingen en werkelijkheid dus geen reductie, maar een projectie—het is een waarschijnlijkheidheidsrelatie en geen relatie van equivalentie. Wat waarnemingen zeggen over de werkelijkheid is in termen van waarschijnlijkheid, niet van zekerheid.

## H 7: Epiloog

In het laatste hoofdstuk van mijn proefschrift laat ik zien hoe de kantiaanse elementen in Reichenbachs filosofie ervoor zorgen dat zijn filosofie subtiel omgaat met veel van de problemen die ook nog in de moderne wetenschapsfilosofie een rol spelen.

De latere Reichenbach wordt vaak geschaard onder de aanhangers van het *logisch positivisme*. In de tweede helft van de $20^{\mathrm{e}}$ eeuw raakt het logisch positivisme verwikkeld in de 'positivismusstreit': de logisch positivisten worden bekritiseerd onder andere vanwege hun afwijzing van de metafysica en de grote waarde die ze hechten aan directe, onbevooroordeelde waarneming. Ik laat zien dat Reichenbachs filosofie verfijnder is dan vaak wordt aangenomen en dat de standaardkritiek uit de positivismusstreit hem niet treft.

Reichenbach schenkt veel aandacht aan de relatie tussen directe waarneming en wat theorieën over waarnemingen zeggen. We hebben gezien dat er in Reichenbachs benadering constitutieve elementen nodig zijn om van directe waarnemingen naar wetenschappelijke waarnemingen te komen. Reichenbachs erkenning van het bestaan van zulke constitutieve elementen lijkt dezelfde strekking te hebben als het idee van Karl Popper dat wetenschappelijke waarnemingen theoriegeladen zijn. Reichenbachs ideeën over de relatie tussen wetenschappelijke waarnemingen en de werkelijkheid—zijn realisme—laten zien dat Reichenbach de metafysica niet schuwde. Integendeel: de realistische benadering vormt een onlosmakelijk onderdeel van Reichenbachs filosofie.

Mijn conclusie in dit hoofdstuk is dat Reichenbach niet vatbaar is voor het merendeel van de kritiek op het logisch positivisme die geuit werd in de positivismusstreit. Reichenbachs immuniteit in deze is te danken aan de immer aanwezige sporen van kantiaanse filosofie in zijn werk. De conclusie van mijn proefschrift als geheel heeft hierin een natuurlijke positie: *de sporen van kantiaanse filosofie in Reichenbachs werk zorgen ervoor dat diens opvattingen veel subtieler (en minder vatbaar voor standaard-kritiek) zijn dan doorgaans aangenomen wordt.*

# Gnobske Gearfetting yn it Frysk

Yn myn proefskrift bestudearje ik de feroarjende opfettingen oer kânsberekkening fan de natuerkundige en filosoof Hans Reichenbach (1891-1953).

Neffens de Amerikaanske wiskundige en filosoof Hilary Putnam (1926-2016) wurde yn 'e wittenskipsfilosofy sa no en dan goede ideeen ôfketst út reden dat se ûnderdiel binne fan in âldmoadrige filosofy. In foarbyld hjirfan, seit Putnam, is it idee fan de ferneamde ferljochtingsfilosoof Kant (1724-1804) dat guon eleminten yn natuerkundige teoryen fûneminteler binne as oaren om't se de waarnimming 'konstituearje'. Kant leaude dat guon konsepten (sa as tiid, romte en determinisme) nedich binne om waarnimming mooglik te meitsjen. De kantiaanske wittenskipsfilosofy hie ta doel om de konstitutive eleminten, de eleminten dy't de waarnimming konstituearje, op te spoaren. Al yn de 19$^{e}$ iuw wie der krityk op Kant syn ideeën en yn 'e 20$^{e}$ iuw rekke de filosofy fan Kant sels yn ûnmin: de oanhingers fan it logysk positivisme (dat yn 'e jierren 20 fan 'e 20$^{e}$ iuw opkaam) woene neat mei de filosofy fan Kant út te stean hawwe.

Reichenbach, dy't de promotor wie fan Putnam, skreau yn 1915 in proefskrift wéryn oft hy noch útgong fan it kantiaanske idee fan 'e konstitutive eleminten. Mar yn syn lettere wurk rjochtet Reichenbach him ta it logysk positivisme en is der neffens Putnam fan Reichenbach syn iere kantiaanske ynslach neat mear oer. Dat betreuret Putnam sa bot dat hy seit dat lykas faker yn 'e wittenskipsfilosofy de poppe mei it waskwetter fuortsmiten is.

Ik sil sjen litte dat de trystens fan Putnam sûnder grûn is, om't der yn it lettere wurk fan Reichenbach noch hieltiten dúdlike spoaren fan kantiaanske filosofy fûn wurde kinne. Mear as dat ik leau dat de kantiaanske eleminten de opfettingen fan Reichenbach ferdigenje tsjin de gongbere krityk op de logysk positivisten. Dat Reichenbach syn proefskrift skreau by it ljocht dat fan 'e kantiaanske filosofy ôfkaam is tsjintwurdich algemiene kunde, mar dat der kantiaanske spoaren binne yn Reichenbach syn lettere, logysk positivistyske, opfettingen dat is gjin algemiene kunde.

Reichenbach syn oanpak fan de kânsberekkening stiet tsjintwurdich bekend ûnder de namme frekwintisme. Yn dizze ynterpretaasje stiet de kâns fan in barren (bygelyks de útkomst trije by de wurp mei in dobbelstien) lyk oan de relative frekwinsje fan dit barren yn in ûneinige rige fan waarnimmingen oan in eksperimint (yn ús foarbyld is it eksperimint in wurp mei in dobbelstien) wêryn it barren barre kinne soe. Yn syn proefskrift yn 1916 seit Reichenbach dat we wis wêze kinne fan it bestean fan de wiskundige funksje dy't dizze kânsen beskriuwt at we derfan útgeane dat de kânsberekkening mooglik is. Reichenbach seit yn 1916 dat we net allinnich oer de kânsfunksje wis wêze kinne, mar dat we ek wis wêze kinne dat de wiskundige funksjes dy't we brûke yn 'e natuerkunde in deterministyske wrâld beskriuwe.

De oanpak fan Reichenbach yn 1916 slút moai oan by wat bekend stiet as de klassike kânsberekkening (út de tiid fan Laplace; betide 18e iuw). At we oannimme dat Reichenbach syn kânsfunksje kontinu is, soarget dat derfoar dat de kânsen fan Laplace evenredich binne mei Reichenbach syn relative frekwinsjes.

Yn in artikel út 1925 ("Die Kausalstruktur der Welt") nimt Reichenbach ôfstân fan it determinisme en yn 1935 skriuwt hy in boek ("Wahrscheinlichkeitsrechnung") mei deryn in opskave ferzje fan syn frekwintisme. Wêr't Reichenbach syn betide frekwintisme in fysyse teory oer barrens wie, is de opskave ferzje fan syn frekwintisme alderearst in logyske teory oer wiskundige rigen fan eleminten. De teory kin fansels tapast wurde op fysyse barrens, mar yn it frekwintisme fan Reichenbach is dat in oanfoljende stap. Dizze stap wurdt troch in oar ferneamd frekwintist, Richard von Mises, net ûnderkend. Omdat yn Reichenbach syn opfetting de oergong fan teory nei waarnimming in ekstra stap is, moat hy dizze oergong útlizze. Hy moat útlizze hoe oft einige rigen fan waarnimmingen harren ferhâlde ta de ûneinige rigen dy't lyk steane oan kânsen. Reichenbach yntrodusearret it konsept fan kânsfermoeden om as brêge te fungearjen tusken teory en waarnimming. In útspraak oer in kâns is net, sa as gewoanwei fan útspraken yn de logika sein wurdt, wier of nwier, mar we hannelje as oft dat wol sa is. We moatte wol, neffens Reichenbach, want sûnder kansfermoedens kinne we neat. Reichenbach jout in soad oandacht oan dizze saken, want hy siket nei de grûn foar de mooglikheid fan wittenskiplike kunde. We sjogge dat Reichenbach syn brûken fan it konsept kânsfermoeden in dúdlike stap yn it kantiaanske programma is.

Yn 1938 skriuwt Reichenbach in boek (yn it Ingelsk; "Experience and Prediction") mei deryn in úteinsetting fan syn hiele wittenskipsfilosofyske wrâldbyld. It belang fan de rol fan syn frekwintisme hjiryn kin hast net oerskat wurde: neffens Reichenbach is syn idee fan de kânsfermoedens itjinge dat ús tastiet te leauwen oan it bestean fan de fysike wurklikheid. Dat wurket sa. Troch ivige fersteuringen yn alles wat we mjitte kinne, kinne we nea earne wis fan wêze. At we ferwachtsje fan teoryen dat se ús wissichheid jouwe, dan sizze dy teoryen hielendal neat! Neidat we ûnderkenne dat wittenskiplike útspraken oer de bûtenwrâld kânsfermoedens binne, krije sokke útspraken de betsjutting fan fysyse tspraken. Wy sjogge dat it frekwintisme fan Reichenbach essinsjeel is foar syn realistyske wittenskipsfilosofyske opfetting.

It byld fan Reichenbach dat us hjirút temjitte komt is net dat fan de gongbere logysk positivist. De logysk positivisten woene neat út te stean hawwe mei metafysika, mar yn it wrâldbyld fan Reichenbach is der in tige wichtich plak foar metafysika. Fierder wurdt fan de logysk positivisten faaks sein dat se wat nayf wiene oer útspraken oer mjittingen en tochten dat der in ien-op-ien relaasje is tusken mjittingen en waarnimmingen. Letter hie Popper hjiroer in oar idee: hy leaude dat waarnimming altyd 'laden is mei teory'. Ek yn dit aspekt is Reichenbach in stik subtiler dan dat trochgeans tocht wurd: Al lang foardat Popper it hie oer teory-laden waarnimmingen, skreau Reichenbach oer syn kantiaansk-lykjende idee fan teoretyske eleminten dy't waarnimmingen konstituearje.

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Universiteit Utrecht

Throughout the work of Hans Reichenbach there are traces of Kantian philosophy. It is common knowledge that Reichenbach started out as a neokantian, but it is less well-known that there are neokantian traces within Reichenbach's mature, logical positivist, writings. The author of this dissertation tracks the Kantian traces from Reichenbach's early frequentist interpretation of probability in 1916 to Reichenbach's later work – work which is written from a logical positivist standpoint.

In the first part of this dissertation we investigate the early development of the theory of probability, and particularly the role played in this by the concept of the *a priori*. The second part of this dissertation concerns the role of the *a priori* in the work of Reichenbach. The clearest trace of neokantianism in Reichenbach's philosophy is an *a priori* (not depending on observations) element within his probability interpretation – a priori in a neokantian sense: it is a condition for the possibility of a certain type of knowledge.

The neokantian trace runs from Reichenbach's idea of a 'continuous probability function' (1916); via his 'probabilistic posit' (1935/49); to the idea that the relation between reality and our observations thereof is a 'projection' (1938). The neokantian a priori forms a persistent and essential element in Reichenbach's philosophical views.

This dissertation ends with an 'apologia'. It is shown that much of the traditional criticism levelled against the logical positivists does not apply to Reichenbach.

*Summaries in Dutch and Frisian are included at the end of the dissertation.*