The Limits of Probabilism

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Abstract: I argue that Bayesian probabilism is applicable only to phenomenological theories, in which empirical hypotheses can be clearly distinguished from conventions, while it fails for abstract theories as in physics, where a separation of empirical and conventional parts is usually not feasible. The argument starts from the observation that scientific theories generally contain conventions and that conventions by their very nature cannot be evaluated in terms of probabilities. I then discuss several options how probabilities might be ascribed to a conjunction of empirical hypotheses and conventions—with the result that none of them works. The most promising attempt, namely in terms of probabilities of the empirical consequences given certain conventions, fails due to the mentioned fact that empirical and conventional elements cannot be separated in abstract theories. Thus, Bayesianism cannot provide a foundation for the methodology of abstract sciences.

1. Introduction

In the past decades, there have been various attempts to explicate central concepts in the philosophy of science using methods from probability theory, in particular Bayes' Theorem. I will show in the following that such a Bayesian approach to philosophy of science cannot live up to its task. The simple reason is that it involves a category mistake to ascribe probabilities to theories in the abstract sciences, e.g. in physics. In a nutshell, the argument proceeds as follows. There are three main premises: (1) Besides empirical hypotheses, abstract scientific theories generally contain conventions (Sec. 2a). (2) To ascribe probabilities to conventions constitutes a category mistake (Sec. 2b). (3) In the abstract sciences, it is impossible to clearly distinguish between conventions and empirical hypotheses (Sec. 2c). From these three assumptions it follows that one cannot ascribe probabilities to abstract scientific theories as well as to hypotheses, which are either outright conventional or which have an uncertain conventional-empirical status (Sec. 2d). The result adequately mirrors common practice in the sciences. Bayesian probabilism has proven successful mainly in phenomenological sciences like medicine, psychology, or artificial intelligence while it has only rare applications in physics, chemistry, or other abstract sciences.

2. The argument from conventions

Let me first clarify the target of the argument. There exists a wealth of fairly recent literature in the philosophy of science that considers Bayes' Theorem as the ultimate foundation of many aspects of scientific reasoning, including confirmation, belief change, theory reduction, underdetermination, explanation etc. (e.g. Salmon 1966, Rosenkrantz 1977, Horwich 1982, Jeffrey 1983, Earman 1992, Howson & Urbach 2006) In this literature, probabilities are habitually ascribed to scientific theories as the following two examples show. The first concerns the problem of old evidence as discussed in the context of a Bayesian explication of confirmation, for example in Earman & Salmon 1992: "When Einstein proposed his general theory of relativity (H) at the close of 1915 the anomalous advance of the perihelion of Mercury (E) was old news, that is, $Pr(E|K) = 1$ [where K refers to the background knowledge]. Thus, $Pr(H|E.K) = Pr(H|K)$, and so on the incremental conception of confirmation, Mercury's perihelion does not confirm Einstein's theory, a result that flies in the face of the fact that the resolution of the perihelion problem was widely regarded as one of the major triumphs of general relativity." (98) The second example is Jon Dorling's influential Bayesian treatment of the Duhem-Quine problem (1982). Dorling reconstructs episodes mainly from the history of physics in Bayesian terms involving probabilities for physical theories like orthodox quantum mechanics, local hidden variable theories, Newtonian gravity, or general relativity. I will now proceed to show that such discussions cannot provide much methodological insight since they

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are based on the mistaken assumption that physical theories can be ascribed probabilities.

2a. Abstract scientific theories contain conventions

While the notion of convention has received its fair share of philosophical attention, it remains a notoriously difficult concept. Fortunately, we do not have to delve into the details of the debate since the two features of conventions that will be relevant for the argument against probabilism are largely uncontroversial: to each choice of a convention there exists (i) at least one viable alternative which is (ii) incompatible with the other choices. To substantiate this claim let us briefly look at two widely influential accounts by David Lewis and Henri Poincaré.

Arguably, the classic philosophical treatment of convention in the twentieth century is due to David Lewis (2002). Lewis gives a somewhat lengthy definition in terms of a regularity in the behavior of members of a community resulting from a coordination problem within this community (78). Essentially, if most members conform to a certain regularity, then the preferences of all members should be such that everyone conforms to this regularity. The behavior of the majority thus determines the preferences of the individual member. According to Lewis, if the majority had opted for a different regularity, the individual members would have had to follow along. Lewis stresses that there is "no such thing as the only possible convention" and that "it is redundant to speak of an arbitrary convention" (70). Thus, Lewis' definition implies (i) and (ii), since there are always alternative choices which are mutually incompatible, because the different kinds of behavior are incompatible. Typical everyday conventions like dress codes or traffic rules fit well with Lewis' definition but also scientific conventions like the choice of base units.

In comparison with Lewis' account, Henri Poincaré has a much narrower focus on the role of conventions in science and especially in physics:

> "Are [conventions in physics] arbitrary? No; for if they were, they would not be fertile. Experience leaves us our freedom of choice, but it guides us by helping us to discern the most convenient path to follow. Our laws [when of conventional nature] are therefore like those of an absolute monarch, who is wise and consults his council of state. Some people have been struck by this characteristic of free convention which may be recognised in certain fundamental principles of the sciences. Some have set no limits to their generalisations, and at the same time they have forgotten that there is a difference between liberty and the purely arbitrary." (Poincaré 1905, xxiii)

While Poincaré also stresses the existence of incompatible alternatives, he is less clear in comparison with Lewis to which extent the choice between alternatives depends only on the behavior of the majority. For example, Poincaré pointed out the conventional nature of the axioms of geometry, but notoriously claimed that there is one best choice, namely Euclidean geometry, which Poincaré singles out in terms of 'commodité', i.e. convenience. While Poincaré agrees with Lewis that there is always some freedom, the specific choice of a convention need not be arbitrary. Still, Poincaré's account implies that there is at least one viable, incompatible alternative to each convention which might however fare worse in terms of convenience. Thus, the characteristics (i) and (ii) are fulfilled, which is enough for the argument in the next Section 2b to go through.

Let me illustrate these properties by means of the convention that length is measured in meters. Clearly, there is a variety of other units of length that would be equally good, for example measuring length in feet. There even exists an infinite number of alternative choices, if meter is multiplied with an arbitrary number and the result taken as the base unit. These choices are incompatible since we have to decide, which numbers to write on measuring rods, maps and traffic signs and to use for calculations of length in scientific or everyday contexts. Thus, in agreement with Lewis' account, the members of the relevant community have to conventionally fix the unit of length in the interest of a common goal, namely communicating distances. Then, they must act according to the established convention, resulting in regularities of behavior. Obviously, there are important contextual and pragmatic factors that narrow down sensible choices of the unit of length. It is no coincidence that the fundamental unit is of the order of the human body size. After all, most problems that we deal with in every-day life are of that order of magnitude. Finally, one certainly cannot speak of meter being the right or wrong or even probable base unit. Rather, it is a suitable or convenient choice with respect to certain applications.

There is no shortage of conventions in scientific theories. Let me list a number of different types without being exhaustive: (1) Whenever a measurable continuous quantity forms a part of a scientific theory, a convention is necessary to determine the unit of this quantity. That we measure length in meters is an example, or energy in joules, time in seconds etc. (2) Conventions are also required when scientific theories with invariances or symmetries are applied. Take for example homogeneity in space as a symmetry (or invariance) of physical theories. Whenever a specific set-up is considered, the symmetry is broken and a suitable origin of the spatial coordinate system must be introduced. The choice of the origin is of course conventional. Another example concerns the Lorentz-invariance of the theory of special relativity. For specific calculations, a convention must be introduced that fixes the velocity of the observer and thereby the choice of inertial system. (3) Often, concepts are introduced in scientific theories in terms of suitable conventional definitions. As an example, Newton's second axiom has frequently been interpreted as a definition of the (very useful) quantity of force from mass and acceleration. On the basis of this example, let me briefly provide some plausible considerations that in fundamental theories the conventional part includes several of the core statements of the theory. The reason is that fundamental theories themselves introduce the language required to describe the respective phenomena. They thus serve a linguistic function and must generally contain explicit or at least implicit definitions of the core terms of the theory. Now, these definitions are at least in certain aspects of conventional nature. Since they often link central concepts of the theory, they plausibly belong to the core of fundamental theories, as in the case of Newton's second axiom.

Items (1) and (2) show that conventions have to be introduced whenever a theory is applied to a specific physical set-up. Thus, a theory can only be confirmed or disconfirmed by observations if conventions are taken into account. Confirmation thus never concerns the bare structure of the theory itself, but always the structure plus a suitable choice of conventions. Item (3) suggests that fundamental theories will contain conventional elements that concern the very core of these theories, because they themselves introduce the vocabulary necessary for an adequate description of the respective phenomena.

2b. No probabilities for conventions

I will now argue against ascribing probabilities to conventions. The argument, which holds independently of a specific interpretation of probability, is based on the concept of convention as spelled out in the previous section, in particular on the two crucial characteristics that were pointed out: namely that to every convention there is (i) at least one alternative choice, which is (ii) incompatible with the first.

Consider a certain choice C_1 of a convention, e.g. C_1 = 'the unit of length is one meter'. Two cases are distinguished, both of which run into contradictions. In the first case, the probability of the convention is assumed to be considerably smaller than one, i.e. $P(C_1) = p << 1$. The problem with this option is that no theory or more generally set of propositions T containing the convention C_1 can attain a probability that is larger than p since: $P(T) = P(T \setminus C_1 \mid C_1)\, P(C_1) \leq p$, where $T \setminus C_1$ refers to the set of propositions T without the convention C_1. In other words, there would be no well-confirmed theories if the probabilities of conventions would be considerably smaller than one.

In the second case, if the probability is approximately 1, i.e. $P(C_1) = p \approx 1$, then all other choices C_2, C_3, etc. must have a probability close to zero. This can be derived easily from the axioms of probability: $P(C_1 \vee C_2) = P(C_1) + P(C_2) - P(C_1 \,\&\, C_2) = P(C_1) + P(C_2) \leq 1$, from which follows that $P(C_2) \leq 1 - p \approx 0$. In the derivation, the fact was used that C_1 and C_2 are incompatible choices and therefore $P(C_1 \,\&\, C_2) = 0$. By this reasoning the probabilities of all alternative choices C_2, C_3, etc. must be close to zero, and therefore any theory or set of statements containing these conventions would have a probability close to zero. But this contradicts the second assumption that at least one alternative C_2 is a viable choice. For example, one can very well construct a highly plausible theory containing the convention that "the unit of length is one foot".

The problems remain even if a specific choice of convention is considered to be the best or in Poincaré's words the most convenient choice. Essentially, there is no reason to suppose that a measure of convenience will obey the probability axioms. For example, one cannot reasonably assume that the degree of convenience of all possible alternatives should add up to one. After all, if the degree of convenience of a particular choice increases due to some new information or a change in context, why should the degree of convenience of the other choices decrease accordingly? One may well imagine that some new information leads to an increase or decrease in the convenience of *all* possible alternatives.

Also, it is often the case that a convention has an infinite number of alternatives. If the measure of convenience is normalized to one, as required by the axioms of probability, and a reasonably smooth curve for the convenience measure is assumed, i.e. that similar choices of conventions should not differ significantly in terms of convenience, then we must ascribe to all choices of convention a degree of convenience of zero. This consequence is absurd, since intuitively at least some plausible choices should have a finite convenience different from zero. For example, the convenience of measuring length in meter is certainly finite, even though there exists an infinite number of alternatives. This problem is specific to conventions and does not occur for empirical statements due to a crucial difference: the values of conventional quantities are always exact, while the values of empirical quantities come equipped with an error function. Consider for example the proposition P: 'quantity X has a value of 1.3'. If P is of conventional nature, then *all* further digits of X are fixed to be zero by convention. Therefore, the measure of convenience of P will always be zero, as long as the convenience function over possible alternative values for X remains smooth and continuous. By contrast, if P is of empirical nature, an error function is automatically associated with X. In other words, the proposition P then implies that X can take on different values within a certain interval, say between 1.250 and 1.349. Taking into account this range of error, the probability of P can be finite and even approach one, although the distribution over alternative values remains smooth and continuous. Thus, there arises a problem with the updating process for conventions. In the case of smooth and continuous distribution functions, the convenience of certain choices of conventions will always stay zero, while the probability of empirical statements can take on any value between zero and one. In addition, there may be some technical difficulties regarding a supposed Bayesian updating of continuous convenience functions, since Bayes' Theorem usually works with discrete distributions. However, these difficulties could presumably be overcome by employing more sophisticated techniques, e.g. a discretization of the problem. In summary, any convenience measure that is supposed to handle cases with an infinite number of alternatives cannot be normalized to one and therefore will not satisfy the probability axioms. Not surprisingly, convenience is not probability.

As a last resort, maybe probabilities should not be ascribed to particular choices of conventions but to equivalence classes of conventions. But it is generally impossible to choose equivalence classes such that the probability axioms are satisfied. Consider once more the measure of length. In order to solve the problems mentioned above, the equivalence class should comprise at least all base units that differ from meter by a simple factor. Also, this equivalence class should be given probability one. However, one can easily imagine more complex choices of measure where the base unit relates to the meter in terms of a space- and time-dependent function (resulting essentially in non-Euclidean geometries). Should these complex choices all be given a probability zero? Are they always and in all contexts less convenient then the ordinary meter and its equivalence class? Into which equivalence classes should we partition these further measures of space? In the end, these difficulties are insuperable. Equivalence classes cannot provide a basis for ascribing probabilities to conventions.

2c. Criticizing the conventional-empirical distinction

In the phenomenological sciences that deal with more or less directly observable events it is not difficult to formulate 'theories' that are purely empirical. However, in abstract and fundamental sciences like physics this is not possible since as we had seen in Section 2a, conventions usually concern some of the core elements of the theory. Also, a clear distinction between the empirical and the conventional part of such theories cannot be drawn—resulting in considerable disagreement between different scientists about the empirical or conventional nature of certain propositions. Often, there is no disagreement about the propositions themselves or about their formulation but only about their empirical or conventional status. Furthermore, the *same* scientist may consider a proposition empirical in some contexts and conventional in others. This point is of course closely related to well-known criticism of the analytic-synthetic distinction.[2]

Let me present some examples. (1) The choice of measure for fundamental quantities is often much more complex than suggested in the discussion concerning the unit of length in Section 2a. This can already be deduced from the fact that large amounts of money continue to be invested in metrology, i.e. the science of measuring. Fundamental units continue to be redefined—always connected with a shift in the empirical or conventional status of fundamental propositions.[3] Generally, what is at stake in choosing a fundamental unit is more than just a simple factor as in: one meter equals 3.28 feet. Rather, the choice of measure is much more complex, and is deeply interwoven with the progress in the respective science itself. Chang (2007) demonstrates this in a very detailed historical case study using the example of temperature.

Another good example concerns the debate regarding the conventionality of geometry beginning in the second half of the 19th century. Around that time, mathematicians realized that non-Euclidean geometries, i.e. geometries which do not obey the parallel postulate, can be formulated in a consistent way. Soon mathematical physicists like Hermann von Helmholtz and Henri Poincaré realized that such non-Euclidean geometries can be used for representing physical space and the motion of particles therein if complementary changes are introduced for the physical laws (e.g. von Helmholtz 1870). This insight resulted in the thesis of the conventionality of geometry, i.e. that there is a number of possible choices (Euclidean and various non-Euclidean) how the geometry of physical space can be consistently formulated—each involving a different choice of measure and corresponding changes in the fundamental laws of physics. Even though Einstein still in 1921 held that the thesis of conventionality of geometry was in principle correct, there has been a tendency to empiricize the geometry of physical space after the advent of general relativity. Over the last hundred years the debate concerning the empirical or conventional nature of geometry has continued without a clear result, which is hardly surprising given the complexity of the issue.

(2) A related issue concerns the empirical or conventional nature of constants as can be illustrated using the vacuum speed of light. The question if this constant is empirical or conventional depends on the stance that one assumes towards the relation between space and time. One might, à la Minkowski, insist that we ultimately live in a four-dimensional space-time, i.e. that space and time are just different dimensions of one and the same entity. From this viewpoint, it is a historical coincidence, resulting from a premature understanding of physics, that we happen to measure space and time with different units. In principle, leaving aside pragmatic considerations, one should use the same measure for time as for space. Consequently, the velocity of light is a mere convention.

To ask if a conventional constant can change over time is nonsensical. Nevertheless, physicists take seriously such a possibility on cosmological scales, obviously denying a strong conceptual identity of space and time. Still worse, the same physicists sometimes treat the velocity of light as a convention in certain contexts, for example when dealing with events in terrestrial laboratories, but might be willing to concede some limited empirical content when considering astronomical scales. Thus, there is no general agreement if the velocity of light is an empirical or conventional constant and given the complex ramifications with immensely difficult questions like

2 Essentially, I agree with Duhem that difficulties with the analytic-synthetic distinction are relevant only for abstract sciences, but not for phenomenological sciences. This underdetermination in empirical-conventional content of abstract theories serves an important function for scientific progress, as will be shown on another occasion.

3 An interesting recent development in metrology aims at the redefinition of four of the seven international base units, namely the kilogram, the ampere, the kelvin, and the mole. The new definitions will rely on fixing four fundamental constants, namely the Planck constant, the elementary charge, the Boltzmann constant, and the Avogadro constant, respectively. Strictly speaking, these constants will be turned into conventions.

the conceptual nature of space-time, it is not very plausible that there will ever be a definite answer.

(3) The empirical-conventional distinction is also blurred when it comes to the question which quantities are fundamental in a theory and which are secondary or merely defined. Consider again Newton's second axiom/law as an example: force = mass x acceleration. Throughout the history of physics, the exact status of this axiom has been debated. The conventional-empirical status of the second axiom obviously depends on the intricate issue which of the quantities figuring in the second axiom are fundamental and which are not.

The fixing of measure for fundamental quantities, the determination of fundamental constants, or the determination of which quantities are fundamental and which derived—all these issues are tasks rather for abstract sciences than for phenomenological sciences since the latter mostly rely on a language determined by other (abstract) sciences.

2d. Bringing together the argument

On the basis of the premises 2a-c, I will now argue that abstract theories and hypotheses cannot be ascribed probabilities. Emphatically, a conception of theories as a conjunction of empirical hypotheses, as is prevalent in some of the Bayesian literature, is too simplistic for the abstract sciences. Rather, as shown in Section 2a, abstract scientific theories generally involve conventions besides empirical hypotheses. They may of course contain still other elements which might also not be probability bearers but here it suffices to focus on conventions.

Consider a toy theory t made up of hypotheses $h_1, \ldots, h_n$ and conventions $c_1, \ldots, c_m$. There are several ways how one could ascribe probabilities to such a theory: i) $P(t) = P(h \& c) = P(h|c) P(c) = P(c|h) P(h)$; ii) $P(h|c)$, which allows for an inverse probability $P(c|h)$; iii) $P_c(h)$, which does not allow for an inverse probability.

The first option i) must be excluded since it involves ascribing probabilities to conventions either in $P(c)$ or in $P(c|h)$ while in Section 2b we have shown that this constitutes a category mistake. Option ii), the probability of the empirical hypotheses given certain conventions, must be excluded for the same reason that probabilities are ascribed to conventions. After all, according to the definition of conditional probability, we have $P(h|c) = P(h \& c) / P(c)$.

Thus, the premises 2a+b imply that probabilities cannot be ascribed to the whole set of propositions of an abstract theory nor to those statements or hypotheses in the abstract sciences which are either outright conventional or have an uncertain conventional-empirical status. Crucially, as we saw in Section 2a, the conventional part generally comprises core elements of abstract theories. Thus, a Bayesian approach to philosophy of science must at least be reoriented or refined with respect to what is meant by the probability of abstract theories or hypotheses.

The plausible candidate for making sense of such probabilities is option iii), i.e. $P_c(h)$, which also refers to the probability of the empirical hypotheses given certain conventions, but unlike in option ii), inverse probabilities $P_h(c)$ are not allowed. Thus, probabilities for conventions are avoided. Apparently, this option works well in the phenomenological sciences like medicine, psychology etc., where hypotheses are often purely empirical and 'theories' just conjunctions of empirical hypotheses. In the most benign cases, the probabilities of such purely empirical theories or hypotheses are independent of the specific choice of conventions, i.e. $P_c(h) =: P(h)$. In more malicious cases, probabilities change with different choices of conventions.[4]

However, option iii) fails if a distinction between empirical hypotheses and conventions cannot be drawn. As we have laid out in Section 2c, this is the case in abstract sciences like physics. Essentially, physical theories have functions beyond making assertions about the world, they also provide an adequate language for speaking about physical phenomena by introducing the necessary vocabulary in terms of conventional definitions. It is with respect to the first function that the notion of probability makes sense but not with respect to the second function. Since both functions are inextricably intertwined in physics, one cannot speak of the probability of a physical theory. Also, single propositions in the abstract sciences often serve both an empirical and a definitional-

4 Confer discussions of language change for example in Williamson (2003) or Romeyn (2005, Ch. 8.6).

conventional purpose and therefore cannot be ascribed probabilities either. Arguably, this holds for many axioms in physics like the Newtonian axioms or the axioms of relativity theory including the constancy of the speed of light, as was discussed in Section 2c.

A Bayesian probabilist may nevertheless insist to identify the probability of an abstract theory with the probability of its empirical consequences $P_x(h)$, where x contains both the clearly conventional part of the theory and those propositions that have a doubtful status. But then, probability as a measure of confirmation will concern only the empirical consequences of the theory, but never the entire theory including x. For example, Bayes' Theorem would read: $P_x(h|e) = P_x(e|h) P_x(h) / P_x(e)$. Since x functions merely as an index, all reconstructions of scientific concepts relying on this version of Bayes' Theorem could never provide insights regarding the crucial role of x.

These difficulties could possibly be avoided if one assumed that the observational consequences uniquely implied the conventional part x. However, in scientific practice, this never seems to be the case. Also, many interesting questions concerning the relation between the empirical and the conventional would automatically be suppressed. Indeed, many fundamental concepts in the philosophy of science concern exactly the definitional-conventional function of abstract scientific theories in relation to the empirical basis. For example, underdetermination is about different descriptions of the same phenomena which stand in a non-trivial relation with each other. Holism is partly about different perspectives on what terms are fundamental and what terms are defined. Theory reduction is about connecting different languages, usually macro and micro, and so on. All these methodological concepts thus cannot be explicated in probabilistic terms.

3. Conclusion

Degrees of belief in abstract theories or abstract hypotheses cannot be spelled out in terms of probabilities, not even in terms of qualitative probabilities. In a sense, 'belief' in abstract theories has a passive and an active component: A passive, evidential component referring to empirical facts and an active, conventional component denoting the willingness of a scientist to stick to certain propositions. The first can be spelled out in terms of probabilities, the second cannot. If these passive and active components cannot sensibly be separated, as is the case for abstract theories or hypotheses, then probabilities cannot be ascribed and a Bayesian approach is not feasible.

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