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Max Albert

Bayesian Rationality and Decision Making: A Critical Review*

Abstract: Bayesianism is the predominant philosophy of science in North-America, the most important school of statistics world-wide, and the general version of the rational-choice approach in the social sciences. Although often rejected as a theory of actual behavior, it is still the benchmark case of perfect rationality. The paper reviews the development of Bayesianism in philosophy, statistics and decision making and questions its status as an account of perfect rationality. Bayesians, who otherwise are squarely in the empiricist camp, invoke *a priori* reasoning when they recommend Bayesian methods—a recommendation that is not justified by their own standards.

1. From the Problem of Induction to Subjective Bayesianism

Induction is the process of inferring, from past observations, either predictions or generalizations that then can be used to deduce predictions.[1] David Hume had famously argued that induction is an irrational but unbreakable habit—irrational because, in fact, any beliefs about the future are as reasonable or unreasonable as any other, no matter what experiences one has made in the past. This view, often called Humean irrationalism, conflicts with the empiricist view that, by and large, science proceeds in a rational and inductive way, thus giving rise to the problem(s) of induction: Is science, or learning in general, inductive? Is it rational?

Many attempts have been made to refute Hume. One of the earliest is due to Thomas Bayes and was embraced by Pierre Simon Laplace. According to Bayes, rational learning proceeds by assigning probabilities, usually called prior probabilites, to hypotheses. Using Bayes' theorem, these prior probabilities are then updated in the light of experience.

In Laplace's account, the precise meaning of the prior probabilities was unclear. To determine these probabilities, Laplace used what is often called the *principle of insufficient reason.* There is not much of a principle, though; it all

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[1] The subsequent remarks on the history of the problem of induction are based on Gillies 1988; Hacking 1990; Humphreys 1990 and Musgrave 1993, 151–166.

boils down to intuitive judgments of equiprobability suggested by the presentation of a problem.

Subsequently, the Laplacian account of rational learning was criticized because applying the same intuition to a different presentation of the problem often yields different probabilities. About one hundred years later, John Maynard Keynes and Rudolf Carnap therefore tried to improve upon Laplace's approach by interpreting the prior probabilities as a measure of quantifying logical relations between statements. They did not succeed; the logical probabilities proved to be non-unique. Today, the theory of logical probabilities is usually viewed as untenable, although the idea of finding some objective basis for the assignment of prior probabilities has not been given up altogether (see Howson/Urbach 1993, ch. 4, 413–419; Gillies 2000, ch. 3).

While Keynes and Carnap unsuccessfully tried to save the Bayes-Laplace tradition, there emerged an alternative, very influential conception of rational learning that does not rely on prior probabilities. The statistician Ronald Aylmer Fisher and the philosopher Karl Raimund Popper sharply rejected the Bayes-Laplace tradition and proposed other, quite similar solutions to the problem of rational learning.

Fisher and Popper both accepted a new interpretation or theory of probability, the frequency theory, according to which a probability is defined as the limiting relative frequency of an event in an infinite sequence of repetitions of an 'experiment', that is, a set of repeatable conditions. Probabilities in this sense are sometimes called statistical probabilities. The frequency theory emerged in the middle of the nineteenth century as an empiricist reaction against the Bayes-Laplace tradition (Gillies 2000, 88). From the perspective of the frequency interpretation, it is not admissible to assign probabilities different from 0 or 1 to general hypotheses or any other statements because there are no experiments in which these statements are sometimes true and sometimes false.

With his theory of significance testing, Fisher generalized the χ^2-test of Karl Pearson and the t-test of William Gosset and revolutionized statistical theory and practice. Popper developed the falsificationist methodology and had a similar influence on the philosophy of science. Both solutions to the problem of rational learning are based on the same principle, namely, that it is rational to accept hypotheses if they have survived rigorous testing. In Popper's terminology, such hypotheses are called *corroborated*.[2]

Both accounts of rational learning share a common problem. Fisher and Popper both emphasized that well-corroborated hypotheses might still be false. This poses no problems in the realm of pure science, where one may be satisfied with the result that a certain hypothesis from a set of alternatives is 'the best hypothesis so far'. However, in practical decision making, this is different. Is it really reasonable to proceed as if a thoroughly tested hypothesis that did survive all tests were certainly true? And when do we reach the point when it is

[2] See, e.g., Fisher 1990 and Popper 1959. Salsburg 2001 is a popular history of statistics. An introductory philosophical textbook on probability and inductive logic is Hacking 2001. For the different theories of probability, see Gillies 2000. For attempts to integrate statistics and Popper's falsificationism, see Gillies 1971; 1973; 2000, ch. 7, and Albert 1992; 2002a.

reasonable to suspend all remaining doubts? What should be done if this point is not yet reached?

This problem is sometimes called the pragmatic problem of induction (see, e.g., Musgrave 1989, section 4). Bayes had solved it in principle because his account of learning incorporated the problem of practical decision making; however, his solution was based on the problematic prior probabilities. Adherents of the frequency theory of probability tried to solve the problem in a different way, but the solution was not entirely satisfactory.

Based on Jerzy Neyman and Egon S. Pearson's theory of hypothesis testing, which emerged in the 1930s from a criticism of Fisher's theory of significance testing, a statistical decision theory was developed by Abraham Wald in the 1940s (Neyman/Pearson 1967; Wald 1950). This theory applied to situations where a statistical model—a limited set of alterative hypotheses each specifying a probability distribution—was taken as given. Such situations quite naturally emerge in problems of statistical quality control, where depending on a sample from a batch of goods a decision maker must determine whether to accept the batch, reject it, or suspend the decision in favor of drawing a further sample. In such a context, statistical decision theory considered the selection of a strategy, or complete contingent plan, from the set of all possible strategies.

According to this theory, the decision maker should choose a strategy under the assumption that a malevolent adversary determines which of the possible hypotheses from the set of alternatives is true. It is assumed that the adversary, often called 'nature', makes her choice knowing the strategy chosen by the decision maker. Thus, selection of a strategy is made by comparing the expected outcome of each strategy in the worst case allowed by the statistical model. The decision criterion reflecting this approach is called either the maximin criterion ("maximize the minimum of the expected gain") or minimax criterion ("minimize the maximum of the expected loss"), depending on whether the consequences of the decisions are described in terms of gains or losses (see also Luce/Raiffa 1957, 278–80).

The interpretation of the maximin criterion, as it will subsequently be called, in terms of an adversary was suggested by v. Neumann and Morgenstern's theory of zero-sum games. In this work (see also Aumann 1990, 8–18), an axiomatic theory of decision making under risk was developed. This theory shows that reasonable preferences over probability distributions of valued outcomes can be represented by the expected utilities of these outcomes. The theory, however, presupposes known (frequentist) probability distributions (Hacking 1990, 176). In contrast, statistical decision theory only assumes a statistical model, that is, a set of alternative hypotheses each specifying a different probabilitiy distribution. To a situation where the true distribution is unknown the theory of decision making under risk does not apply. Assuming that an adversary selects the true hypothesis removes the indeterminateness concerning the probability distribution.

The maximin criterion replaced the questionable prior probabilities by the paranoid assumption of the malevolent adversary who, knowing one's decision, ensures that the worst case obtains with certainty. A less paranoid presentation is that, conditional on the assumption that the statistical model is correct, the maximin criterion selects a strategy with a guaranteed minimal gain. In any case, there was no justification for the pessimism or, alternatively, the obsession with certainty embodied by the maximin criterion.

A further problem for the frequentist or classical school of statistics (to use a term that refers to Fisher's approach as well as to that of Neyman, Pearson, and Wald) arose from a problem within the frequency theory of probability, the so-called problem of the single case. The classical school recommends statistical methods that imply low probabilities of committing certain errors, for instance, the error of rejecting a true hypothesis, often called error of the first kind. According to the frequency theory, the probability of a first-kind error is the limiting relative frequency of committing this error in an infinite sequence of applications of the method. Why should anybody care for this limiting frequency when using the method once or, which comes to the same thing, in a finite number of cases?

The problem of the single case had already been raised by Charles Sanders Peirce in 1878 (Hacking 2001, ch. 22). Neyman bit the bullet and argued that it is rational to adopt methods for the single and finite case that are good in the infinite long run; this is his view of 'inductive behavior' as a solution to the problem of induction.[3]

Thus, the frequentists' solution to the problem of induction faced foundational problems. The problem of the single case undermined the whole theory of probability upon which the rejection of the Bayes-Laplace tradition was built. And the problem of the decision criterion opened the door for the use of prior probabilities, once a replacement for the infamous principle of insufficient reason could be given.

In fact, Bayes himself had already proposed a justification for attaching prior probabilities to hypotheses. This justification had been lost in Laplace's version of the theory. In the 1920s, however, even before the advent of the Neyman-Pearson theory of statistical inference, Frank Plumpton Ramsey and Bruno de Finetti independently of each other criticized the theory of logical probabilities and came up with, essentially, Bayes' theory, which

is a subjectivist version of the logical theory, embracing the non-uniqueness of logical probabilities as an expression of personal beliefs. This theory is subsequently called Bayesianism.[4]

The most influential version of Bayesianism has been proposed by the statistician Leonard J. Savage (1954). In the 1950s, the ground was already well

[3] See also Hacking 2001, chs 19, 22. Mayo 1996, ch. 11, argues that Pearson differed from Neyman in this respect and defends a Pearsonian "evidential" version of the Neyman-Pearson approach against Neyman's "behavioral" version. However, Mayo's approach is explicitly based on the frequency theory (1996, 89 n. 23), while it seems that evidential interpretations must be based on single-case considerations (Albert 1992, 17–21; 2002b). Bayesians, of course, can do without frequentist probabilities; however, Howson/Urbach 1993, ch. 13, argue—in a nice twist but, in the last analysis, unconvincingly (Albert 2003)—that frequentist probabilities are relevant for Bayesians, if for nobody else.

[4] The theory is also known as subjective(ly) expected utility (SEU) theory. The term Bayesianism is sometimes used in a wider sense and then includes the logical theory (if there is one) and other attempts to objectify prior probabilities.

prepared by the developments of the preceding decade, which might explain why his book had a greater immediate impact than the earlier work of Ramsey and de Finetti. Savage could refer to both statistical decision theory and the axiomatic theory of decision making under risk, which he extended in a way already anticipated by v. Neumann and Morgenstern.[5] He showed that a reasonable preference order over the set of all conceivable strategies can be represented by expected utilities of strategies, where now not only the utilities, but also the probabilities for computing the expectations are derived from the preference order.[6]

Savage (1954) provided a general theory of rational learning and decision making. For many, the axioms of Savage define rationality. Today, Bayesianism is the most influential surviving version of empiricism and the predominant philosophy of science in North-America at least. Many statisticians view themselves as Bayesians, or would be Bayesians if they thought that Bayesianism was computationally feasible. In economics and some parts of political science and sociology, Bayesianism is the most general version of the standard theory of behavior, and even for those who reject it as a theory of actual behavior and prefer some version of bounded rationality, it remains important as the benchmark case of perfect rationality.

The importance of Bayesianism lies in the fact that it is a very general philosophy that seamlessly covers science and decision making from the problem of induction, which provides the context where it originated, to the theoretical and practical problems of statistical inference, which provide the background for its comeback. The present paper presents a systematic criticism of Bayesianism on both levels. Section 2 considers the problem of induction and asks whether Bayesian rationality constitutes progress beyond Humean irrationalism. The problem of induction is concerned with learning from scratch, beginning with complete ignorance. Section 3 considers the less ambitious idea that Bayesianism might be helpful in situations characterized by partial ignorance. This is the level of statistical methods and practical decision making. Section 4 comes back to the question of whether objective Bayesianism has still something to offer. Section 5 concludes.

[5] See Hacking 1990, 176, on the relation of v. Neumann-Morgenstern and Savage. See Savage 1954, ch. 9, on the shortcomings of the minimax criterion. See Luce/Raiffa 1964, ch. 13, for a discussion of the different approaches to decision making under uncertainty in the 1950s. See Kiefer/Nyarko 1995 for a concise exposition of Savage's approach. For a critical but sympathetic discussion, see Earman 1992. Howson/Urbach 1993 give an introduction to philosophy of science from a Bayesian perspective. Pratt/Raiffa/Schlaiffer 1995 do the same for statistics on an elementary level.

[6] Savage 1954 uses "act" instead of "strategy". In his account, all probabilities are subjective degrees of belief, but Anscombe/Aumann 1963 soon proposed a system of axioms where objective-probability distributions and subjective degrees of belief coexist.

2. The Problem of Induction: Thomas Bayes Meets the Chaotic Clock

We start with a simple problem: sequential betting on an infinite sequence of 0s and 1s, where nothing is known about the mechanism generating the sequence. Even if we believe that some deterministic law must be at work, this does not restrict the range of possible irregularities, as is well known. For instance, the following deterministic process will generate any conceivable infinite sequence of 0s and 1s, depending on the starting point θ:[7]

$$\begin{aligned} x_t &= 2z_t \text{ div } 1 \\ z_{t+1} &= 4z_t \mod 1 \\ z_1 &= \theta \in [0,1) \end{aligned} \tag{1}$$

The process can be illustrated by a simple device I call the chaotic clock (fig. 1).

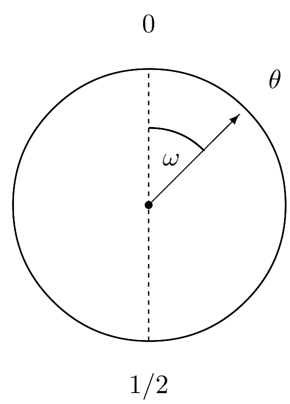

Figure 1: A chaotic clock (Davies 1987, ch. 4). With each tick of the clock, the angle ω is quadrupled. When the pointer is in the first (second) half of the dial, the digit 0 (1) appears on a screen.

On the dial, there are all the real numbers of the half-open interval $[0,1)$. There is one pointer that can point to all real numbers in the interval. The vertically upward position is zero and the vertically downward position is $\frac{1}{2}$. Initially, the pointer deviates by an angle $\omega = 2\theta\pi$ from the vertically upward position, thus pointing at the real number θ. At $t = 1, 2, \ldots, \infty$, the pointer moves by quadrupling the angle ω. The pointer itself cannot be observed. However, if the pointer comes to rest in the first half of the dial and points at a number in $[0, 0.5)$, we observe a 0; otherwise, we observe a 1.

[7] The process is a slight modification of the well-known baker-map dynamics (see also Ford 1983). "div" denotes integer division; "mod" denotes the indivisible rest of the integer division, i.e., $x \mod n \overset{\text{def}}{=} x - (x \text{ div } n)$. The modification is introduced in Albert 1999; 2001.

This chaotic clock can generate any infinite sequence of 0s and 1s. For any such sequence, there is always an infinite number of starting points generating it. The chaotic-clock model provides a simple example of a set of causal hypotheses rich enough to explain any observations. It fulfills all the formal requirements of a scientific theory. It assumes a simple mechanism governed by a law of motion that produces different results according to the initial position of the mechanism. Processes that lead to chaotic dynamics are not rare, and imperfect observability can produce the kind of irregular behavior characteristic of the chaotic clock. Moreover, although the chaotic-clock model assumes a continuum of starting points (corresponding to more hypotheses than we could ever consider explicitly), it is less complicated than models encountered in physics or economics. It would be difficult to find any acceptable formal requirement that excludes the chaotic clock from consideration.[8]

Any kind of practical prediction problem can be cast into the form of predicting a 0-1 sequence since in practical problems information is finite and can be digitalized, at least in principle. The level of ignorance depends on the set of sequences that can be excluded as impossible before any observations are made. If no sequence can be excluded, complete ignorance holds and the problem is equivalent to the problem of induction.

When applied to this problem, it turns out that Bayesianism is empty. Let X be the set of all finite 0-1 sequences. A forecast function $\rho\colon X \to (0,1)$ assigns the probability $p = \rho(x)$ to the event that the next observation is 0 if the finite sequence $x \in X$ has been observed. A forecast function is the most general way of describing forecasts. Given a forecast function, a betting strategy is determined by maxmizing the expected utility of a bet on the next digit. Then, the anything-goes theorem (Albert 1999; 2001) says that any forecast function can be rationalized by a prior probability measure on the set of all starting points for the pointer. This also implies that any betting behavior can be rationalized. Moreover, it means that the posterior probability measure concerning the current pointer position is completely arbitrary, even after a long sequence of observations.[9]

In plain words, whatever one's experiences, any beliefs about the future, and any choice of strategy, are equally reasonable or unreasonable, since any beliefs can be rationalized by a suitable prior. Thus, Bayesian rationality is just a complicated restatement of Hume's irrationalism.

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[8] The chaotic clock poses a generalized version of Goodman's (1955) 'new riddle of induction'. The set of hypotheses considered by Goodman is countable and, therefore, too small to lead to the problems discussed in the present paper. Using the chaotic clock for presenting the problem of induction has the advantage that no 'gruesome' predicates appear.

[9] Since the emphasis is on decision making, the restriction of the forecast function's set of values to the open interval $(0,1)$ is immaterial: the same bet that is chosen for a degenerate forecast probability will be chosen for an almost degenerate probability. Excluding degenerate probabilities has the advantage that the problem of choosing a new prior when observing a zero-probability event need not be considered (see also Kiefer/Nyarko 1995).

3. Bayesian Experts and Bayesian Methods

Although Bayes attempted to refute Hume, it may be considered unfair to confront Bayesianism with a problem where nothing is known in advance. Even if Bayesianism is not helpful when nothing is known, it might be helpful in the case of partial rather than complete ignorance (see also Luce/Raiffa 1957, 299–306). Thus, we descend from the lofty heights of the problem of induction to the realm of practical decision making and statistical inference.

In this realm, Bayesianism might provide, in the form of prior probabilities, the means by which experts in the relevant field of application can bring their experience to bear on a problem. Moreover, in cases where the problem in question is so well defined that a statistical model has already been accepted, recommendations derived by Bayesian methods sometimes are in conflict with those derived by classical methods, and Bayesians argue that their recommendations are superior.

3.1 Model Building and Strategic Simplifications

Real-world decision problems often have to be simplified to become tractable. In such cases, it seems that experts may hit on strategical simplifications because of their prior experience. According to contemporary model-building wisdom, finding the right simplifications is an art, not a science; it involves tacit knowledge and requires experts in the field. Not surprisingly, this conviction is widely shared by experts.

Bayesianism, it seems, gives the experts a possibility to bring their experience to bear on the problem. They can choose a prior probability measure in the light of their experience. Given this choice, which can be communicated to others, decision making can proceed if the computations are feasible; if not, one can try to find an approximation. Indeed, model building is itself a matter of approximations; Bayesian experts might construct simplified models by excluding possibilities that they assign, in the light of their experience, a low prior probability.

Thus, it could be argued that Bayesianism describes a rational way of expressing partial expert knowledge that cannot easily be expressed in another way.

However, Bayesianism leaves in the dark how experts proceed when trying to transform experience into a prior. Experience may just cause experts to have a prior; it might change one's beliefs in the same way as a blow on the head might do. In this case, it is hard to see why one should call in an expert rather than somebody else.

On the other hand, experts might learn from experience in a rational fashion. In this case, we already know how ideal Bayesian experts proceed. They start with a prior probability measure before making experiences, updating their prior, and when after some time they are viewed as experts, the prior they bring to a new problem is actually a posterior probability measure embodying their experience.

The problem with this analysis is, however, that the anything-goes theorem implies that the expert's posterior is completely arbitrary. According to Bayesianism, all conclusions drawn from experience are equally reasonable or unreasonable. There is no reason why the prior probability measure chosen by an expert should be better or worse than the one chosen by a complete novice. In this sense, there is no Bayesian way to reduce the complexity of problems with the help of experts, and no reason why one should pay a high fee to the expert. If a Bayesian calls in an expert, it is because he or she happens to believe in experts, but Bayesianism itself provides no reason to do so. To believe in do-it-yourself is equally reasonable, even for complete novices.

3.2 Recommending Bayesian Methods: A Principal-Agent Model

The argument of the preceding subsection can be made more precise if we consider well-defined problems where Bayesianism competes with classical methods. In statistical inference, Bayesianism gives general advice of how to proceed when a statistical model has been selected. In such a case, ignorance has already been reduced very far; it is assumed that one statistical hypothesis from a restricted set is true, but it is unknown which.

For a given statistical model, Bayesianism is not necessarily empty. There are cases where Bayesianism rejects strategies recommended by the maximin principle (see, e.g., Lindley 1972, 13–15), which for present purposes we can identify with classical statistics. Let us rule out the possibility of misspecification, that is, the possibility that the truth is actually not included among the different hypotheses that jointly form a given statistical model. The simplest problem, then, where Bayesianism and classical statistics disagree involves two hypotheses and three strategies. A clash occurs when the maximin principle recommends a strictly dominated strategy (see fig. 2).

An action is strictly dominated if it will never be selected by Bayesian maximizing subjectively expected utility, no matter how the Bayesian assigns subjective probabilities to the two hypotheses. Classical statisticians and Bayesians can agree that expected utilities of strategies can be computed under the assumption that one or the other hypothesis is true. These utilities appear on the axes in figure 2. For a Bayesian, the overall expected utility of a strategy is the weighted average of these expected utilities, where the weights are the subjective probabilities of the hypotheses. Bayesian indifference curves would appear in figure 2 as straight lines with a slope of $-(1-p)/p$, where p is the subjective probability of hypothesis 1.

Accordingly, a Bayesian will be indifferent between S_1 and S_2 if and only if his subjective probability of hypothesis 1, p, takes on some intermediate value $\bar{p}$, in which case the points S_1 and S_2 are on the same indifference curve. If $p < \bar{p}$, which is the case illustrated in figure 2, the agent will prefer strategy S_2 because point S_2 is then on a higher indifference curve than point S_1. If $p > \bar{p}$, he will prefer strategy S_1. Obviously, there is no constellation where S_3 is on the highest indifference curve. However, S_3 is selected by the maximin criterion because the minimal utility achieved by this choice is higher than the minimal utilities of S_1 and S_2.

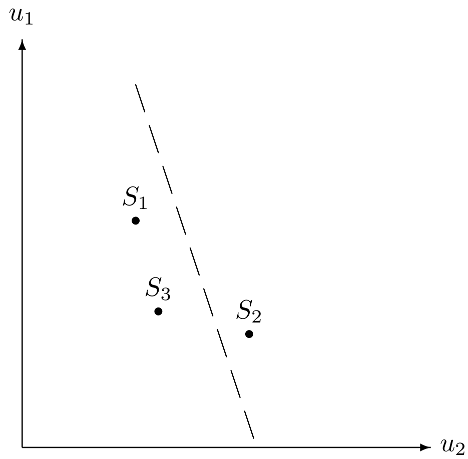

Figure 2: The axes measure the utilities of strategies S_1, S_2 and S_3 depending on whether hypothesis 1 or hypothesis 2 is true. Indifference curves of a Bayesian decision maker are parallel lines with negative slope. If the slope is relatively high, as shown by the broken line, the decision maker selects S_2, where he reaches the highest indifference curve. Strategy S_3 is strictly dominated since S_1 or S_2 will always be on a higher indifference curve.

Thus, a Bayesian will never select S_3. However, is there any basis on which a Bayesian can recommend that other people should not select S_3? Bayesian statisticians argue that use of the maximin criterion implies a violation of the Savage axioms, which in their view define rationality. However, the recommendation to use the Bayesian approach—and *a fortiori* the enforcement of its use by, for instance, a researcher in a leading position—is itself a decision. By their own standards, Bayesians should apply the Bayesian approach to the question of whether they should recommend the Bayesian approach or enforce its use.

Let us therefore consider a simple principal-agent model where a Bayesian principal employs several statisticians as agents. The statisticians make decisions in very different fields: medicine, banking, etc. In contrast to standard principal-agent models, principal and agent share the same utility function. Any conflict of interest arises because of differences in beliefs. In terms of figure 2, this means that they agree with respect to the coordinates of the points but differ with respect to the slopes of their indifference curves. This assumption reflects the fact that we discuss scientific methods; a common aim can be assumed but beliefs differ.

Concerning beliefs, we consider two variants of the model, which I call the strong and the weak version. In both versions, different agents will in general have different priors. In the strong version, the principal knows the true hypothesis in the statistical problems considered by her agents. In the weak version, the principal also has a prior probability distribution on the hypotheses, which in general will differ from the priors of her agents. In both versions, the principal cannot advise the agents on how to choose in each instance. She can only give them the general advice either to maximize, in each instance, their subjectively expected utility, or to take always the action recommended by the maximin principle.[10]

Obviously, the strong version is more interesting, because the principal has privileged knowledge, which implies that her decision carries more weight. However, we will also look shortly at the weak version.

The solution to the principal's problem depends, of course, on what she believes about the priors of her agents. Let us consider the strong version first. The principal's beliefs about her agents' priors boil down to beliefs concerning the probability that an agent facing one of the problems will actually assign a high enough probability to the truth, which is known to her but not ot the agent, and select the strategy that is best in the light of the truth.

The calculus of the principal can easily be understood in terms of figure 2. Assume that the principal knows that hypothesis 1 is true; thus, only the vertical axis is relevant to her. On the vertical axis, S_1 is the best and S_2 is the worst strategy, while S_3 is in between. She assigns a certain subjective probability q to the possibility that her agent will actually select S_1. Thus, *her* subjectively expected utility of letting the agent maximize *his* subjectively expected utility is $qu_1(S_1)+(1-q)u_1(S_2)$. If she recommends the maximin principle, her expected utility is $u_1(S_3)$.

Therefore, she should recommend Bayesian methods if and only if $q \geq \bar{q}$ where $\bar{q} \stackrel{\text{def}}{=} (u_1(S_3) - u_1(S_2))/(u_1(S_1) - u_1(S_2)) < 1$.

Let us say that a prior that leads its bearer to select the objectively best strategy in the light of his aims 'fits with reality'. A Bayesian principal should recommend the use of Bayesian methods if and only if she believes that, on average, the priors of her agents 'fit with reality'. A principal who believes this is called a Bayesian optimist.[11]

Bayesianism does not imply Bayesian optimism; the principal may as well be a pessimist, believing that her agents' priors are, so to speak, out of tune with reality. In figure 2 and under the assumption that hypothesis 1 is true, pessimism means $q < \bar{q}$, that is, the principal's subjective probability that the agent's prior leads him to select S_1 is so low that the maximin method is more attractive than Bayesian methods. If the principal believes that the priors of the agents are, on average, 'biased against the truth', she is better off if she recommends the use of the maximin principle.

It is certainly not the case that the principal should be a Bayesian optimist just because she knows that her agents are Bayesians, because even after years of experience Bayesianism as such does not ensure that an agent's subjective

[10] She might also require them to randomize. However, the use of randomization in order to improve upon the strategy recommended by the maximin principle belongs to the classical methods and is rejected by Bayesians.

[11] Forster discusses the question of the fit with reality of methods of learning with respect to machine learning, where theorems similar to the anything-goes theorem are discussed under the heading of no-free-lunch theorems (1999, section 5), and with respect to Bayesianism (1999, section 6, esp. 555), where he also states an informal version of the anything-goes theorem.

probabilities fit with reality. The anything-goes theorem implies that no amount of experience puts any constraints on the posterior. Hence, from an *ex ante* point of view, an experienced Bayesian's subjective probabilities can be expected to fit with reality only if his prior before any experience already fitted with reality.

From a scientific point of view, Bayesian optimism is a completely arbitrary position unless one believes in some hypothesis explaining how it comes about that the agents have priors that fit with reality. As already discussed, the fact that the agents are Bayesians does not provide such an explanation. In order to be a *scientifically rational* Bayesian optimist, the principal would have to believe in some specific causal mechanism tending to produce a fit between priors of her agents and reality.

In my view, it is rather unlikely that a plausible hypothesis describing such a process will ever be produced. Bayesian methods are free-floating, that is, they are not tied to some area. An evolutionary mechanism operating among Bayesian agents could produce priors fitting with reality in those problem areas that are important for reproductive success. However, in the areas we are discussing here, statistical problems in medicine, banking and so on, such an evolutionary process is in fact absent. Moreover, evolutionary processes involve no guarantees either; depending on the mechanism generating priors and the environment where it operates, evolution could also lead to the extinction of the whole species of Bayesians.

However, even if the principal could think of a hypothesis able to explain the fit between priors and reality, the question of the success of Bayesian method would remain an empirical question. After all, this ingenous hypothesis might be false. A reasonable Bayesian principal would reject the appeal to the plausibility of the Savage axioms (or any other system of axioms) as a valid reason for recommending Bayesian methods. She would have to observe her agents while they employ Bayesian methods in order to find out whether they in fact tend to make the right decisions sufficiently often.

I conclude that the strong version of the principal-agent problem suggests that there is no basis for the recommendation to use Bayesian rather than classical methods in statistics.

Let us shortly consider the weak version of the principal-agent model. In this version, there are two possibilities. If the principal herself is indifferent, or nearly indifferent, between S_1 and S_2, she will prefer Bayesian methods. If her prior is such that S_3 is her second-best choice, she will recommend Bayesian methods only if she expects that, on average, her agents' priors are similar enough to her own.

There is of course a potential basis for explaining a fit between the priors of principal and agents: it might be a psychological law that people come up with similar priors when confronting similar problems. Whether such a law holds is again an empirical question. Quite apart from this question, the basis of the principal's recommendation is very weak since she has no privileged knowledge. Consider a Bayesian statistician who is asked by her students why she recommends Bayesian methods. It is hardly conceivable that she could convince them of the value of these methods by pointing out that she believed that these methods would lead them to take the same decisions as she would take in their shoes.

Thus, it seems that the recommendation of Bayesian methods, if it is to carry any force, must be based on a convincing case for Bayesian optimism. First, one would have to have some hypothesis of how a fit between reality and the priors of diverse agents working on diverse problems might come about. Then, one should set out to test this hypothesis. Even a Bayesian could end up with a posterior that assigns a low probability of success to Bayesian methods.

But this is all speculation. So far, Bayesians argue in favor of Bayesianism on *a priori* grounds. This is not acceptable by their own, thoroughly empiricist standards.

The argument against *a priori* justifications of Bayesian methods extends to all methods, whether Bayesian or not. The claim that a certain method is reliable in some sense is a lawlike hypothesis and should therefore be tested. In the case of free-floating methods, however, it is difficult to see how one could seriously claim that a certain method will be reliable.

It is important to note that the maximin principle is not subject to these objections. If it is true that one of the two hypotheses is true, the kind of reliability claimed by the maximin principle is a certainty. Of course, this is a big if; misspecification is a serious problem, and neither Bayesianism nor the methods developed by the Neyman-Pearson-Wald school are able to solve it in a satisfactory way (Albert 1992). However, Fisherian tests like the χ^2 goodness-of-fit test do not assume a given set of hypotheses. These tests are often used as misspecification tests, and the reliability claimed by them—a certain probability of first-kind errors—is again a certainty.

Traditionally, Bayesians do not claim reliability; they invoke *a priori* reasoning when they justify the use of Bayesian methods. The argument of the present section shows that these arguments—whether one accepts them as justification or not—cannot be used to recommend Bayesian methods. Even a Bayesian should think twice before recommending Bayesian methods because, according to Bayesian standards, there is no reason to assume that any good comes from using Bayesian methods.

4. Objective Bayesianism

The previous discussion addresses subjective Bayesianism, according to which a prior before experience is arbitrary. Some Bayesians argue that, in fact, this is not true. When nothing is known, one should use a non-informative prior. The idea is to let the data speak for themselves; if nothing is known, only the data should influence the posterior.

This is all very well, except that such a non-informative prior does not exist (Leamer 1978, 61-63; Howson/Urbach 1993, 413–419). The idea of using non-informative priors is just the old principle of insufficient reason. Just to see how this idea founders again, let us apply it to the problem of induction.

Remember that the chaotic clock is a theory of everything, excluding nothing.

It is just a means to express a state of complete ignorance. Therefore, objective Bayesianism requires that one chooses a non-informative prior on the set of all starting points of the chaotic clock. Clearly, any prior breaking the symmetries of the presentation would be informative. Hence, if there exists a non-informative prior at all, it could only be the uniform prior.

However, while one might call the uniform prior non-informative, it clearly does not allow the data to speak for themselves. Indeed, the uniform prior means that any data are completely irrelevant because the posterior distribution is always uniform, too. An non-informative prior yields a non-informative posterior; the data are silent. Thus, objective Bayesianism is even worse then subjective Bayesianism when applied to the problem of induction.[12]

When we consider restricted problems where a statistical model is given, the problem of specifying the non-informative prior becomes more difficult. However, let us just imagine that objective Bayesians can agree on some way of finding the correct objective prior for any problem. Is there any reason to assume that the prior preferred by objective Bayesians fits reality better than some other prior? Why should the objective Bayesian's scheme of determining a prior be good across all problems? We could recast these questions in terms of a principal-agent problem, where the agents have agreed on a scheme of assigning an objective prior. A principal who knows the truth in all the problems where the prior is going to be applied might very well conclude that it would be better to use the maximin principle instead of maximizing expected utility on the basis of this prior. How do objective Bayesians know, then, that their's is a good idea? This is an empirical question, after all.

Objective Bayesians resort again to *a priori* reasons, which are not able to demonstrate that one will be more successful if one adopts their scheme than if one does not.

5. Conclusion

Bayesianism was originally proposed as an empiricist solution to the problem of induction. The most fundamental criticism of Bayesianism is therefore that it is empty. By choosing an appropriate prior, any reaction to any experiences can be justified in advance. Thus, Bayesianism implies 'anything goes' as long as one rationalizes choices in the Bayesian way, which can always be done. In other words, Bayesianism is a probabilistic version of Humean irrationalism, the position it was intended to overcome.

The comeback of Bayesianism in the second half of the twentieth century was due to the inherent problems of the frequentist approach to statistical inference and decision making. Since then, Bayesian methods compete with classical

[12] This seems to be just another way of deriving Carnap's result concerning his logical probability measure $c^{\dagger}$, see Howson/Urbach 1993, 62–66. The problem does not depend on the assumption of a continuum of starting points. Assume that the clock moves only finitely often. In that case, we need only distinguish finitely many intervals of starting points. Assuming equiprobability for these intervals leads to the same result: no learning will take place; observations do not matter.

methods in statistics, where it is usually assumed on both sides of the debate that at least something is known. The implications of the anything-goes theorem in contexts where only partial ignorance is assumed are not obvious. Possibly for this reason, Risse shrugs off the anything-goes theorem:

> "While this result questions Bayesianism as an explanatory tool, from the standpoint of decision theory advising [sic!] the first-person perspective, it is hard to interpret. When making a decision, an agent does so in light of her beliefs. Why should she be discouraged from following Bayesian advice if for each available action there exist prior beliefs (most likely not hers) rendering it rational?" (Risse 2003, 231)

However, this is just begging the question. The anything-goes theorem pulls the rug from under the position that Bayesianism is a general account of inductive rationality. The question is whether Bayesian advice has any other basis.

It is a curious fact that Bayesians, who otherwise are squarely in the empiricist camp, invoke *a priori* reasoning when they justify the use of Bayesian methods. They rely on axioms of rational decision making they view as self-evident. Since, however, self-evidence comes with no guarantees, non-Bayesians and Bayesians alike should consider the question of whether there is a Bayesian basis for recommending Bayesian methods.

The answer to this question is negative. The recommendation of Bayesian methods seems to rest on an unfounded empirical assumption which I have called Bayesian optimism: the assumption that prior beliefs fit with reality. A Bayesian argument for Bayesian methods and against classical methods should involve at least one causal hypothesis explaining the assumed fit of priors with reality.

Of course, nothing prevents Bayesians from accepting the self-congratulatory point of view that in terms of their beliefs their choices are justified. But in a more pensive mood, even a Bayesian might ask himself why he should use his own beliefs for decision making. From a Bayesian point of view, nothing speaks against it; but neither is there anything that speaks in favor of it.

Bayesianism is an apriorist account of rationality. I think that this is a contradiction in itself. Rationality has something to do with how one goes about to achieve one's aims, at least unless these aims are not restricted to taking (or trying to take) actions for their own sake.

For instance, Bayesians recommend Bayesian methods. Unless they want to argue that this should be done for its own sake, these methods must be good for something. And if Bayesians wish to be taken seriously, these methods should be good for something non-Bayesians care about. Thus, just like everybody else who proposes some means, Bayesians should say what the ends are.

It does not matter what kinds of ends Bayesians come up with. In any case, the hypothesis that Bayesian methods help to achieve these ends is a lawlike statement. If Bayesians argue that Bayesian methods should be used in the inquiry of lawlike statements, this applies also to their own claims.

The same line of argument applies to any account of rationality. One can always ask why it would be a good idea to be rational in the sense of some definition of rationality. If there is an answer, it must be a lawlike statement.

The next question is whether, according to the standards of rationality just proposed, one should accept this lawlike statement. It is a minimum requirement that an account of rationality survives this self-consistency check.

Popper's critical rationalism survives such a self-consistency check (Musgrave 1989). It can even accommodate a theory of significance testing along Fisherian lines, once the deficient frequency theory of probability is replaced by the propensity theory, Popper's later account of single-case probabilities (Albert 2002b). Bayesians so far have not even recognized that their own empiricist standards require that they abandon their apriorist justifications and come up with a similar defense of their views.

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