How to Undermine Underdetermination?

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Abstract:

The underdetermination thesis poses a threat to rational choice of scientific theories. We discuss two arguments for the thesis. One draws its strength from deductivism together with the existence thesis, and the other one is defended on the basis of the failure of a reliable inductive method. We adopt a quasi-objective pragmatic Bayesian epistemology of science framework, and reject both arguments for the thesis. Thus, in science we are able to reinstate rational choice called into question by the underdetermination thesis. (Word count of the abstract is 92, while for the paper it is 9,271 excluding the word counts for bibliography, endnotes, tables, and mathematical notations.)

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Overview

The underdetermination thesis says that if two mutually incompatible theories are empirically indistinguishable, then we can never have good reason to believe one of the two empirically indistinguishable theories to be more likely to be true (Quine[75], and Norton [08]).[1] We will discuss two arguments for the underdetermination thesis.[2] One draws its strength from deductivism together with the existence thesis, and the other one is defended on the basis of the failure of a reliable inductive method. In this paper, we adopt a Bayesian epistemology of science framework (Howson and Urbach [93, 06]), and reject both arguments under a "quasi-objective" pragmatic Bayesian philosophy.

1. **<u>First Argument for the Underdetermination Thesis</u>**

1.1. Deductivism and its consequences

Deductivism plays an important role in an argument for the underdetermination thesis.[3] We define deductivism with respect to its two tenets: (i) all and only observable consequences of a theory are evidence for it (hereafter D1), and (ii) two theories having exactly the same observable consequences (i.e., evidence) are equally confirmed/disconfirmed (hereafter D2). D1 provides an answer to the question *what* counts as *evidence* without distinguishing observable consequences from evidence, while D2 gives a sufficient condition for theories to be equally well confirmed. For deductivists, observable consequences are all that matters in theory confirmation. They take the confirmation relation to be the converse of the deductive consequence relation.

Deductivisim is responsible for the raven paradox (Hempel, [65]) where we may generate apparently irrelevant evidence (consequences). Consider the hypothesis "All ravens are black. If x is a raven, then x is black' (call it A) is taken to be evidence for the hypothesis. The paradox results when a brown shoe or a white piece of chalk ('if x is non-black, then x is non-raven': call it B), which is in fact irrelevant to the confirmation of the hypothesis, turns out to be confirmatory evidence for the hypothesis (See Royall, [97] for a nice discussion of the raven paradox]). By D1, because A is an observable consequence of the hypothesis, A is also evidence for the hypothesis. Since B is logically equivalent to A, B is also an observable consequence of the hypothesis and hence by D1, evidence for the hypothesis. The raven hypothesis entails B, so apparently B, which seems to be irrelevant to the hypothesis, provides confirmatory evidence for it. Because deductivists take the confirmation relation to be the converse of the deductive consequence relation, deductivism is to be blamed for the paradox.

1.2. Deductivism as the first argument for underdetermination

To appreciate the argument, we need to understand two additional points related to theory confirmation, the *empirically indistinguishability* notion and the *existence thesis*. Theories are empirically indistinguishable if and only if there is no possible evidence that would confirm one and not the other or disconfirm one and not the other. Many key questions in the epistemology of science are raised by the possibility of the existence of empirically indistinguishable theories. Let us call the claim that there are such theories the existence thesis (ET).

Arguably there are interesting examples of such empirically indistinguishable theories. John Earman [92, 93] offers as an example TN + R and TN + V, [4] where "TN" stands for Newton's theory of mechanics and gravitation, "R" stands for the assumption that the center of mass of the universe is at rest with respect to absolute space, and "V" in TN + V stands for the assumption that the center of mass is moving with constant velocity relative to absolute space. In Newtonian mechanics, there is no experiment that can distinguish between the center of mass of the universe being at rest and the center of mass of the universe being in motion with constant velocity.

If we make some assumptions, we can find hosts of other examples. Assume that two theories are empirically indistinguishable theories if and only if they do not have different (actual or possible) evidential consequences. We also assume by D1 that only observable consequences are evidence. Let T1 be the original theory and T2 be the collection of all its observational consequences. Then T2 is a logical consequence of T1, and hence, all the observational consequences of T2 are consequences of T1 as well. The converse is also true, since if something is an observational consequence of T1, it is included in T2 and hence a consequence of it. T2, of course, is not incompatible with T1, but if we add to it the denial of a non-observational consequence of T1 that is logically independent of T1's observational consequences, we get a theory which is incompatible with T1 and has exactly the same observational consequences. So the theory and its set of observational consequences have the same observational consequences. Therefore, they are empirically indistinguishable.

The underdetermination thesis follows from deductivism plus a simple assumption about confirmation and reasons for belief. Suppose we have two theories, T0, T1 having exactly the same observable consequences. By D2, we derive the conclusion that T0 and T1 are equally confirmed by E where "E" is an observable consequence. If we supply the seemingly obvious claim that when two theories are equally well confirmed, we can never have good reason to believe one to be more likely to be true than the other. This shows the underdeterminaton thesis is a consequence of deductivism together with some assumptions about theory confirmation.

It should be noted that we ordinarily speak of a consequence of a theory as evidence only if that consequence is true. If deductivism is true, we can ignore this, because we obviously cannot distinguish two theories with exactly the same observational consequences on the basis of which consequences are true and which false. If any are false, both theories are false. So we can confine ourselves to discussing cases in which all are observational consequences are presumed true, at least for the sake of the discussion.

1.3. Underdetermination and its consequence

We are scientific realists. Realists hold two beliefs about empirically indistinguishable theories: semantic beliefs and epistemic beliefs. We are semantic realists, because we believe that at least some empirically indistinguishable theories have distinct content, and we are epistemic realists because we believe it is possible (in principle) to have better reasons to believe one of two empirically indistinguishable theories to be more likely to be true than another. However, the second of these beliefs is inconsistent with the underdetermination thesis and the existence thesis (ET), since the underdetermination thesis holds that if two distinct mutually inconsistent theories are empirically indistinguishable, then we can never have good reason to believe one of two empirically indistinguishable theories to be more likely to true over the other.[5] The underdetermination thesis, ET, and epistemic realism are inconsistent. The logical positivists rejected epistemic realism because they rejected the semantic realism on which it rests.

Some current philosophers (Earman [92, 93], van Fraassen [80, 89, 91]) accept semantic realism but reject epistemic realism. Later, we briefly discuss some of Earman's ideas; particularly one interesting part of Earman's position is that although he is not a deductivist because he rejects both its tenets, he accepts the underdetermination thesis. We, however, reject the underdetermination thesis, and the aim of this paper is to explain that rejection. We accept a generally Bayesian framework for the epistemology of science, and we wish to show how our approach justifies the rejection of the underdetermination thesis.

Our derivation of the underdetermination thesis is integrally linked to deductivism. There are, however, problems of deductivism. In several independent, and one jointly written paper, Laudan and Leplin (Laudan and Leplin, [91], Laudan, [95], Leplin, [97])) reject the underdetermination thesis. As a result, they raise doubts about deductivism. Although they have not explicitly addressed the above inconsistency, which is a result of accepting the underdetermination thesis, the existence thesis and epistemic realism together, they are able to salvage consistency by ruling out the underdetermination thesis. In the next section, we will discuss their account and diagnose what has gone wrong with it.

2. An Evaluation of Laudan and Leplin's View on Underdetermination

2.1. Laudan and Leplin on underdetermination

Laudan and Leplin argue that the underdetermination thesis is false because those who accept the underdetermination thesis mistakenly equate a theory's observable consequences with its supporting evidence. On their account, observable consequences are neither necessary nor sufficient for evidence. We provide two examples similar to theirs to explain their position.

A. **Observable consequences are not sufficient for having supporting eviden*ce***

Consider a hypothesis (H):"You and I have identical pocket contents". An observable consequence (E) for H is: "If you have 5 coins in your pocket, then so do I". Assume that you do not have 5 coins in your pocket. Then the observable consequence (E) expressed in the conditional statement is true. However, establishing E by observing that you do not have 5 coins in your pocket, intuitively speaking, does not confirm H. Therefore, an observable consequence fails to be regarded as supporting evidence for the theory.

B. **Observable consequences are not necessary for having supporting evidence**

Consider a hypothesis (H) that the coin in our hand is a fair (unbiased) coin. Suppose D is the data statement that out of 100 flips of the coin, 48 are tails and 52 are heads. D is not an observable consequence of H because D is not a deductive consequence of the theory. However, the probability of H given the data D, Pr(H|D), is higher than the probability of just the hypothesis, Pr(H). Here, we are operating with the background information that there is a relation between a coin being fair and the existence of a particular ratio of heads to tails (as defined through the binomial distribution). Although D is not an observable consequence, E counts as supporting evidence for H. Therefore, observable consequences are not necessary for having supporting evidence for the hypothesis. Hence, according to Laudan and Leplin, observable consequences are neither necessary nor sufficient for having supporting evidence of a theory.

2.2. Evaluation of Laudan and Leplin's account

Laudan and Leplin argue that the underdetermination thesis is false because it mistakenly identifies observable consequences with supporting evidence for a theory.[6] That not all observable consequences are evidence does not cast any doubt on D1, because it does not lead us to doubt that theories with the same observational consequences have the same evidence. However, that evidence need not be a consequence does cast doubt on D2, because it opens the possibility that there may be evidence for only one of the two theories with the same observational consequences. But it is difficult to know what to make of this point unless one says something about confirmation, a subject Laudan and Leplin do not address.

Although we are generally sympathetic toward the conclusion of their argument that the underdetermination thesis is false, a positive account of confirmation is needed as a reason to reject the underdetermination thesis beyond distinguishing between observable consequences and evidence. Here we would like to make it explicit that problems with deductivism along with a need for a positive account of confirmation have motivated us to look for an alternative account. In the next section, we will offer a Bayesian confirmation theory that addresses when and how one of the contending empirically indistinguishable theories with the same observable consequences in a domain could be better supported by evidence.

3. Sketch of a Pragmatic Bayesian Approach

Our view of the epistemology of science is an amalgam of Bayesianism and pragmatism. We accept some form of Bayesian conditionalization as the proper account of how evidence supports theories. Bayesian conditionalization provides a rationale regarding how an *idealized* rational agent should change her belief function in light of new evidence. On this view, if an agent learns that a proposition E is true in her probability space, then she ought to update her belief function by conditionalizing on E. Suppose Pr(H) is a belief function before conditionalization and the agent learns that E is true, then according to the principle of condionalization her new belief function is Pr(H|E) = [Pr(E|H)Pr(H)]/Pr(E) (assuming Pr(E) >0). Therefore, when we conditionalize, the posterior probability of H (Pr(H|E)) is directly proportional to the prior probability (Pr(H)) multiplied by the likelihood (Pr(E|H)).

We combine this conditionalization principle with a pragmatic approach to the setting of prior probabilities, Pr(H). An agent's prior probability for a theory represents the agent's prior degree of belief in the theory independent of any *new* evidence regarding the correctness of the theory.[7] Here "evidence" could include "data," "background knowledge" or observable consequence or all of them together.[8] The likelihood function, P(E|H), tells us how likely the evidence is given the theory. Two empirically indistinguishable theories have likelihood functions that are equal (up to a constant), and therefore if anything allows one theory to be preferred over the other, it must be in their prior probabilities.

3.1. Concerns with prior probabilities

Sources of priors are matters of concern both for some Bayesians and non-Bayesians alike.[9] We should be explicit about the sources of prior probabilities to show our awareness of this concern. On our account, priors result from two sources: (i) the *a priori* probability and (ii) the conditionalization of the *a priori* probability on all the evidence one has acquired up to the moment the prior probability of that hypothesis in question is fixed.

The *a priori* probability of a hypothesis is the probability an agent has independent of any evidence (or background information) whatsoever. Borrowing an example from Stephen Hawking and Leonard Mlodinow [10], we consider the statements "The Pope is a Chinese and "the Pope is a German."[10] With absolutely no background information, we have no reason to prefer one over the other. However, if we conditionalize on the background information that there are more Chinese people that than German people in the world presently, we have reason to assign higher probability to the first sentence. Of course, there is additional background information, dependent on the agent that would change the assignment of the prior probabilities. Only when we bring in our background information should we have a higher probability for one sentence over the other. In fact, the *a priori* probability of both the sentences should be moderate. An agent's *a priori* probabilities for sentences (i.e., theories) are constrained by their simplicity.

3.2. Factors determining simplicity

Two sorts of factors, formal and non-formal, determine the simplicity of a theory.[11] Formal factors may be illustrated by considering a case of the curve-fitting problem. This problem arises when one seeks to recover a formula relating two variables when one is given a set of data presumed to contain some errors. In this case, a formal factor could be the paucity of adjustable parameters; functions of lower degree are assigned higher prior probability than functions of higher degree. We use the following example involving polynomials for illustrative purposes without implying that all cases of the curve-filling problem are cases involving polynomials (Table 1).

Table 1: Four polynomial functions that can be considered for a curve-fitting problem, where a_i, i=1,2,3, are parameters (unknown).

Hypothesis	Polynomial form
H_1	$y = a_0 + a_1x$
H_2	$y = a_0 + a_1x + a_2x^2$ where $a_2>0$
H_3	$y = a_0 + a_1x + 7x^2$
H_4	$y = a_0 + a_1x + a_2x^2 + a_3x^3$ where $a_3>0$

H_1 is simpler than H_2 because H_1 has fewer parameters than H_2; hence H_1 is assigned a higher *a priori* probability than H_2. Though the formal factor is the only factor that dictates our choice of *a priori* probabilities for H_1 over H_2 in the curve fitting example, it often does not suffice to determine the simplicity of a theory.

Our choice of one theory over the other may also depend on our epistemological/background information that we regard as one aspect of the non-formal factor of simplicity. We call this non-formal consideration epistemological because under these circumstances the notion of simplicity is dependent on our background knowledge. The epistemological consideration does not, however, exhaust the entirety of the non-formal consideration. For instance, scientists routinely ignore solutions to equations that are not sensible in a given situation (e.g., negative values for quantities which can only plausibly be positive). We ignore these solutions on the basis of our background knowledge telling us they make no sense.

Scientists' concerns for pragmatic considerations in theory choice constitute the other side of the non-formal consideration of simplicity. For example, whether an equation is mathematically tractable, plays a vital role in theory choice, and hence in the assignment of prior probabilities to theories. There are some well-known *objective* measures of simplicity. However, instead of addressing such measures, we refer to Table 1 and bring H_3 into the discussion. Recall our formal approach. H_1 is simpler than H_2 because it has fewer adjustable parameters. However, when we compare H_1 with H_3, we find that both have two adjustable parameters, but still conclude that H_1 is simpler than H_3. Whether an equation is easy to handle plays a vital role in theory choice, and hence in the assignment of prior probabilities to theories. Working scientists rely on this reason frequently, though it may often manifest as complete exclusion of less tractable theory and preferences for the more tractable theory. Given our selection criterion of simplicity, we will take H_1 to be simpler than H_2 because we take it to be easier to comprehend the former than the latter.

3.3. Simplicity, truth, and constraints on *a priori* probabilities: A broader concern

The connection between "simplicity" and "truth" and that of "simplicity" and "*a priori probability*" are unsettled epistemological issues. This section is intended to provide an outline of how those issues could be approached without implying any definitive answer to those deep epistemological issues. In this regard, we will make four observations.

(i)The preference for simpler theories is widely recognized as a feature of scientific practice, as well as of common sense reasoning, even though there is no widely recognized way of making this criterion both precise and adequate for all the cases where simplicity is relevant. It is not clear that science can proceed without it without falling into a very through-going skepticism.

(ii) There must be an *a priori* element in any inquiry which confirms or rejects theories on the basis of data. There are indefinitely many functions possible from data to theory; some of them could be quite bizarre. To use data to support conclusions, one must have method of relating data to theory, and that method cannot, on pain of circularity, be chosen on the basis of what data determine. The generally Bayesian approach to epistemology encapsulates this element in the *a priori* probability function, and in the use of standard Bayesian picture of updating an agent's probability function in light of data, though admittedly this need not be the only possible approach. A possible question is, "how be this *a priori* element determined"? There could be three routes one could adopt. One could adopt the route of subjective Bayesians and say that it is arbitrary and beyond reasons. Or, one could appeal to pragmatic reasons, among them simplicity. Or, one could go with the tradition and generally stick with methods that have successfully been used in the past, until one finds reasons not to do so for the data which the investigators are presently confronted. Since simplicity is a traditional part of the *a priori* basis for inference, it can be supported either by the second or third approach. One might also take a combination of all three approaches, leaving the first for what is left unsettled by the second two.

(iii) Posterior and prior probabilities of hypotheses are mostly used measures of how likely it is that the hypothesis is true. Therefore, if we settle on a method that assigns *a priori* probability distribution which makes simpler theories more probable than to more complex theories, then we have essentially stated that, other things being equal, simpler theories are more likely to be true. In the typical Bayesian context, to determine how to deal with the *a priori* element in inference is to decide, *a priori*, that some theories are more likely to be true than others. Some such choice is unavoidable. In non-Bayesian setting, this preference for simpler theories still exists (e.g., use of Akaike's Information Criterion) for model selection.

In the curve-fitting problem in Table 1 can be offered as an illustration connecting how likely a hypothesis is to be true with its simplicity. Suppose we have only four data points and wish to choose between H_2 and H_4. In this case, while we can fit the cubic term, it is likely that the curvature captured by the cubic form is due to the particular realization of data (a result of sampling error) we encountered and therefore, we believe H_2 is more likely to be true than H_4, in line with the assignment of probabilities based on simplicity.

(iv) In our view, *a priori* probabilities used in choosing between two competing theories should include simplicity constraints. We provide an account of simplicity that will show how such a constraint works in theory choice. This is an account of how *sometimes* simplicity functions in the context of scientific theories. It is not intended to capture a *global* account of simplicity (Sober [74, 95, and 2002a]).

3.4. Pragmatic versus epistemic reasons for believing a hypothesis:

Many philosophers contend that reasons for believing a hypothesis may be numerous. Some reasons for believing are pragmatic while others are epistemic. A pragmatic reason for believing a hypothesis provides a practical reason for believing it without providing any reason for believing in its truth. An epistemic reason for believing a hypothesis provides a reason for believing in the truth of the hypothesis. In the curve fitting example, we consider H_1 the simplest hypothesis because it is easiest to work with a hypothesis with fewer parameters. Though our selection of H_1 as the simplest hypothesis is based on a pragmatic consideration, this pragmatic consideration is not necessarily devoid of any relationship with our epistemic reason for embracing H_1 as the simplest hypothesis. (Bandyopadhyay, Boik, and Basu, [96], Bandypadhyay and Boik, [99.])

Gilbert Harman [97] argues that the distinction between pragmatic reasons and epistemic reasons need not be exclusive. In other words, pragmatic reasons can sometimes be epistemic reasons and vice versa. Various considerations support this view. For instance, an idealized agent's epistemic reasons for believing a theory could be constrained by her pragmatic reasons for believing the theory. According to our version of Bayesianism, an idealized agent has *a priori* probability function defined over sentences in the First-order language. At the same time she might have *epistemic* reasons for preferring one *a priori* probability function over the other. The agent's *a priori* probability assigned to a theory has to do with her degree of belief, and is therefore related to her epistemic reason for believing the theory. However, it is very likely that she has a preference for non-zero, and non-one *a priori* probabilities for empirical propositions (e.g., hypotheses). Her preference for such probabilities is constrained by her desire to learn from experience using probability theory. That is, unless the agent's epistemic probability for an empirical proposition is different from zero or one, she could not learn from experience using Bayesian conditionalization. This learning from experience has a survival value for the agent and is thus this is a pragmatic reason, desire to learn for better survival, propelling the agent's epistemic reason for preferring non-zero and non-one probability functions.

Similarly, following Harman, we contend that if a consideration makes a difference in the probability of believing a theory, then it is an epistemic reason for believing the theory. Therefore, if we use pragmatic reasons to assign the simplest theory the highest *a priori* probability because it is easier to work with than with its more complex counterparts, then this is a pragmatic--epistemic reason for believing the theory to be more likely to be true than its contenders. That is, it is likely that a simpler theory will be true, in the end, if we have evidence for it which its rival theory lacks (Harman [97].

3.5. Elements of pragmatism and varieties of Bayesianism

One might wonder how this pragmatic feature of Bayesianism we have outlined in section 3.4 is related to Bayesianism as usually understood among philosophers. Bayesianism has been broadly divided under two heads: (i) subjective Bayesianism and (ii) objective Bayesianism.[12] Typically all Bayesians take it to be necessary that an agent's degree of belief should satisfy the rules of the probability theory. Subjective Bayesians, in addition, assume it is also sufficient that the agent's degree of belief satisfy the rules of the probability theory. Our pragmatic Bayesian account is objective in the sense that we require the agent's degree of belief to be constrained by simplicity considerations over and above satisfying the rules of the probability theory. This constraint on an agent's a *priori probability* eliminates several sets of prior probabilities as untenable, but it does not *uniquely* determine a set of priors as acceptable for a set of competing hypotheses. As a result, this pragmatic Bayesian account also falls under the subjective definition. Therefore, because our account is neither fully objective nor fully subjective, we call it "quasi-objective." (For a detailed discussion on this stance, see [Bandyopadhyay and Brittan, 10]). This "quasi-objective" pragmatic account is able to bring back a degree of objectivity in theory choice by uniquely choosing one theory as the best after the data have been gathered, even if they cannot be distinguished based on their likelihood functions. This has important implications related to the underdetermination thesis.

4. A Pragmatic Bayesian Approach to Indistinguishable Theories

Under our pragmatic Bayesian approach we may be justified in choosing one competing theory over another in a domain even though they are empirically indistinguishable. [13] Recall in section 1.3, we have defined theories are empirically indistinguishable if and only if there is no possible evidence that would confirm one and not the other or disconfirm one and not the other. Given this definition, which is in line with usual practice, we do *not* require that any evidence that confirms a given theory should *confirm* both empirically indistinguishable theories to the *same* degree, but merely that it should confirm it. The underlying intuitive idea behind confirmation is that data confirm a theory to a degree if and only if the posterior probability of the theory (Pr(H|D)) is greater than its prior probability (Pr(H)). How weak or strong is the confirmation of a theory depends on how much is the difference between its posterior and prior probabilities. The following provides an illustration of how the account of simplicity associated with the assignment of priors helps us prefer one theory over the theory although they are empirically indistinguishable theories.

Consider our second sort of example from section 1.2. Let T_1 be some theory, and let T_2 be a theory consisting of all the observational consequences of T_1 plus the denial of some non-observational consequence of T_1 which is logically independent of those observational consequences. T_1 is plainly simpler than T_2. In fact, we can hardly get a grip on what T_2 says apart from T_1. It is obviously much simpler and more straightforward to use T_1 as a basis for calculating than to first use T_1 to determine T_2 and then to calculate using T_2 directly. And so we will assign T_1 a higher *a priori* probability than T_2.

Recapping our discussion on simplicity, *a priori* probability function and the truth of a theory, we have found that as simplicity often observed to play a significant role in both scientific practice and common sense reasoning, it has to be a feature of *a priori* reasoning because it is not a part of data gathered to check whether that data provide more support for the hypothesis than its contender. If simplicity is a feature of *a priori* reasoning, then, from a Bayesian framework, it should be a feature of an agent's *a priori* probability function on which it places a constrain if it places a constraint on anything at all. Unless simplicity consideration of a theory encapsulated in an agent's *a priori* probability function makes the theory more likely to be true, there is no reason why simplicity should be a constraint on an agent's *a priori* probability function.

Posterior probabilities of hypotheses are often interpreted as the probability of the hypotheses being true. This begs the question then, if we assign higher prior probabilities to simpler hypotheses means that we believe that they are more likely to be true. Instead, first, we ask what "likely" means. In a Bayesian context it is taken to mean "having a higher probability." Hence, in terms of this Bayesian framework, "simpler theories are likely to be true" means simpler theories, other things being equal, are considered more probable, and this follows from the decision to accord a simpler theory a higher *a priori* probability. In non-Bayesian setting, this preference for simpler theories still exists (e.g., use of Akikean Information Criterion) for model selection, thought it is not explicitly embedded in the method. If we have argued successfully so far about the relationship between of *a priori* probability function and simplicity, given the Bayesian framework, for having the *a priori* probability reflects simplicity, then, from a Bayesian standpoint, a theory which is simpler than the other, other things being equal, has to do with it being more likely to be true. So, as pragmatic Bayesian realists, if we are justified in preferring T_1 over T_2 because it is simpler, then we are also justified in *believing* T_1 as more likely to be the true theory.

5. <u>Reviewing the First Rejoinder to the First Argument for Underdetermination and its Consequence for Deductivism:</u>

In preceding sections, we found that the first argument for the underdetermination thesis is based on amalgamation of deductivism and the existence thesis. We rejected this version of the argument for two reasons: (1) We argued that deductivism is false because both its tenets are false, and (2) that the existence thesis does not entail the underdetermination thesis. Although, the existence thesis says there are empirically indistinguishable theories, the thesis does not imply that there is never a good reason to prefer, or even believe, one theory over the other.

The first tenet of deductivism is false due to the fact that evidence for a theory need not always come from its observable (i.e., deductive) consequences. In this regard, we agree with Laudan and Leplin in that some evidential considerations might not be observable consequences of a theory. The second tenet is also false, because, as Bayesians, we do not think that two distinct but incompatible theories, having the same observational consequences are confirmed/disconfirmed to the same degree with respect to a given dataset. We contended that one theory could be confirmed to a higher or lesser degree in a domain with respect to the same body of data due to different prior probabilities assigned to the theories based on a probabilistic measure of their simplicity. In conclusion, the first argument for the underdetermination thesis fails because of the more sophisticated relationship between data, background information and the competing theories in question than envisaged in deductivism. However, there is another argument to salvage the underdetermination thesis based on the failure of inductive inference as a reliable method.

6. Second Argument for Underdetermination

6.1. Failure of reliability: Second argument for underdetermination

Earman approaches the question of underdetermination through the notion of reliability. He defines several notions of reliability, but the weakest he defines requires that for a very rich set of possible worlds,[14] the inductive method will eventually settle correctly on the truth or falsity of a hypothesis (for particulars see [Earman, 93, p. 23]). Although he accepts Bayesian methodology, he rejects differentiating among theories on the basis of priors, because he considers *all* assignments of priors to competing theories subjective [Earman, 93, pp. 26-33]. He rejects restricting the possible worlds to, e.g., those in which the future resembles the past, because he considers this “dogmatic” [Earman, 93, pp. 27-28].[15]

Having forbidden himself these resources, Earman finds himself left only with versions of Bayesianism that are rather skeptical. For instance, according to him, a reliable inductive method cannot give us confidence that the sun will rise tomorrow based on a long string of sun-risings (though eventually, in the long run it may reliably tell us that; for details, see [Earman, p. 28]). Important to our discussion, Earman also contends that if any inductive methods were reliable, then the underdetermination thesis would be false. However, he is unable to reject the underdetermination thesis because of his belief that inductive methods are not reliable.

Earman takes Bayesian methodology to be the best of its kind, though after investigating whether Bayesian methodology is reliable, he concludes that it is not. Again, let us consider the two Newtonians theories TN + R and TN + V which are equally supported by all possible evidence and hence have equal likelihoods. Then, the choice of one theory over the other depends on what priors we assign to the theories and since, [16] according to Earman, assignment of priors is notoriously subjective, there is no good reason to believe one theory over the other. He assumes that human beings are not usually dogmatic in their assignment of priors to the competing theories. Hence, he concludes the underdetermination thesis is an obvious corollary of Bayesian methodology in conjunction with the empirical indistinguishability thesis and the existence thesis.

We, however, question Earman's argument for two reasons: (1) we do not think invoking priors for discriminating two empirically indistinguishable theories is *necessarily* subjective, and (2) we question the demand for reliable inductive methods in the sense in which he uses the term, "reliable". It is an excessively strong requirement on an inductive method that it be reliable in all possible worlds or even in a rich subset of possible worlds.

6.2. The first rejoinder to the second argument for underdetermination:

Our response to the charge raised by Earman that the use of priors necessarily smacks of subjectivity and arbitrariness has at least two parts. First, the charge that the use of priors is necessarily subjective could be misleading because the notion of subjectivity is ambiguous. Second, the charge overlooks that the use of priors could very well depend on the pragmatic nature of the problem with which an investigator is confronted without necessarily opening the door to subjectivity.

Our response to the charge raised by Earman that the use of priors necessarily smacks of subjectivity and arbitrariness has at least two parts. First, the charge that the use of priors is necessarily subjective and arbitrary needs to be clarified. Once it is clarified, it well be seen that some degree of subjectivity may not be unreasonable. Second, the charge overlooks how the use of priors could very well depend on the pragmatic nature of the problem with which an investigator is confronted, and could thus avoid unreasonable subjectivity.

For the first part, we must consider that some Bayesians have held that any priors are reasonable, so long as they satisfy the probability calculus and give contingent propositions probabilities that are neither 0 nor 1. This seems to be the greatest degree of subjectivity. Bayesians, however, need to be so completely subjective. They may hold that priors are constrained by objective considerations such as simplicity. In this case we need to ask whether the sorts of rational constraints placed on priors are sufficient to justify the degree of consensus we see in the practice of science. If so, the Bayesian with such objective constraints is able to claim that scientific consensus is reasonable.

6.2.1. Simplicity constraints and subjectivity:

It should be clear that we do not endorse the highest degree of subjectivity. We already argued in section 3 that we are not full-blown subjectivists in this sense that we have imposed objective constraints on how priors should be distributed among contending hypotheses. Here we are more explicit about the requirements any assignment of priors must satisfy in the context of simplicity constraints: (1) It must satisfy the probability calculus, and (2) it must order hypotheses with respect to simplicity in which simplicity should be understood in terms of its formal and non-formal aspects constrained by epistemological/pragmatic considerations of competing theories/hypotheses in a domain.

In most applications, the priors on a finite set of hypotheses can be ordered in an objective manner. We argued in Section 3.1 and elsewhere (Bandyopadhyay, Boik, and Basu, [96,], Bandyopadhyay and Boik [99]) that sources of prior probability come from two places: (i) the *a priori* probability independent of any background information and (ii) the conditionalization of the *a priori* probability on all the evidence and background information gained before the time the prior is fixed. In addition, the simplicity constraint has to be on the *a priori* probability if simplicity has any role in constraining the values of the priors. On our account, simplicity of a theory is a function of two factors, formal and non-formal factors as discussed in Section 3.2. Hence, we are not subjective in the first sense because we do not accept that any assignment of priors is reasonable even though it obeys the probability calculus. This assignment must satisfy additional constraints like the simplicity consideration.

To see how much variation these constraints may allow in choice of *a priori* probability, let's consider these following *a priori* probabilities in Table 2.

Table 2: Several possible forms of a priori probability incorporating simplicity constraints, whereas *k* is the number of adjustable parameters and *n* is the sample size.

A Priori Probability	Equation
$Pr_1(H_k)$	$2^k; k \geq 1$
$Pr_2(H_k)$	$(e-1)e^{-k};\ k \geq 1$
$Pr_3(H_k)$	$(\sqrt{n}-1)\,n^{-k};\ \ k \geq 1$

Table 2

That is, ordering priors with respect to simplicity does not dictate the specific form of the *a priori* probability. That is, an agent can still choose any of the three *a priori* forms from Table 2 or *some other prior form* that orders hypotheses with respect to simplicity. An agent adopting this approach is responsible for justifying his/her choice. If the prior acquired from *a priori* probability cannot be justified, then the agent has fallen prey to the charge of subjectivity in the first sense. However, we have a pragmatic justification for our priors and we have already argued in section 3.4 that this justification need not be independent of epistemic justification.

With such constraints infinitely many priors are unacceptable (e.g., the assignment of these priors, Pr(H1) = 1/1000, Pr(H2) =1/100, and Pr(H3) = 1/10, may be unacceptable), while at the same time infinitely many other priors are acceptable (e.g., Pr(H1) = 1/10, Pr(H2) = 1/100, and Pr(H3) = 1/1000), and hence rational, thus failing to assign one unique set of priors as acceptable. We concede that we *are* subjective to this degree. This admission captures what is going on in the actual practice of science when different investigators confronted with a problem bring their distinctive background information to bear on solving that problem. We will also argue this degree of subjectivity does not ultimately endanger the objectivity of scientific inference in the sense of making it non-rational, but rather it captures the spirit of scientific practice.

The important question is whether the degree of subjectivity that we tolerate leads to a conflict in theory choice that is incompatible with scientific practice. We argue that the third sense of subjectivity does not necessarily hold in *general*, because all of the acceptable prior forms may well agree in choosing between the hypotheses, in cases where scientific practice settles on a single hypothesis ([Bandyopadhyay and Boik 99, Bandyopadhyay and Brittan, 01]). To illustrate this, we will take a detour of recent works on model selection within a Bayesian framework to show why in most cases it is of paramount importance that we at least end up with a unique choice of a hypothesis.

6.2.2. Bayesian model selection, subjectivity, and our pragmatic Bayesianism

Within Bayesian model selections, in some cases an agent may be able to show that for priors chosen from a well-defined class, the results are invariant with respect to the specific choice of a prior from that class. In this case, the agent need only defend the class of priors and not any specific choice from within the class. Schwarz's Bayesian Information Criterion (BIC) can be defended in this manner (Schwarz, [78]), and our use of Bayes' theorem, which we call Bayes' Theorem Criterion (BTC) elsewhere (Bandyopadhyay, and Boik, [99]) can be seen a general case of BIC. BTC roughly states that $Pr(H_k|E)$ is proportional to log-likelihood of the sample size n multiplied by priors of the hypotheses in question. BIC, on the other hand, says that $Pr(H_k|E)$ is proportional to log-likelihood of the sample size n multiplied by $n^{-k/2}$, where k is the number of estimated parameters. BTC is equivalent to BIC, provided that H_k is not a function of n. The idea here that priors chosen from a large class all yield the same value of criterion, (e.g., BTC, BIC), to a first order approximation. Also, for moderate to large sample sizes, these priors play a negligible role. Then, as shown in (Bandyopadhyay and Boik, [99]), even though any of a class of priors referred to above is justified; the chosen prior does not affect the selection of a unique hypothesis as the best in a given domain after data have been gathered as evidence for these hypotheses.

Our account gives due consideration to the nature and the context of these problems without trying to provide a general recipe justifying all kinds of priors an agent could have recourse to in theory choice. Our account is pragmatic for two reasons, and we return to the curve fitting problem in Table 1 for illustration of the first; assignments of priors, for example, in connection with the curve-fitting problem, ½ to H_1, ¼ to H_2 and 1/8 to H_4 depend on pragmatic and background assumptions. We have assigned higher priors to H_1 because we argue it is easier to work with than its contenders. Second, our account is pragmatic because it is crucial to understand the complexities of scientific problems we encounter. Whether the choice of priors plays a significant role in theory choice depends largely on the nature of the problems we confront. For example, the role, choice, and influence of priors is arguably different for estimation problems than for hypothesis testing problems, two general contexts Bayesian statisticians confront (see Kass and Raftery, [95]). [17] If Bayes Factors [which is defined as the ratio of posterior odds of H_1 (or H_2) to the prior odds of H_1 (or H_2).

$$\frac{\Pr ob(H_2 \mid D)}{\Pr ob(H_1 \mid D)} = \left(\frac{\Pr ob(D \mid H_2}{\Pr ob(D \mid H_1}\right)\left(\frac{\Pr ob(H_2}{\Pr ob(H_1}\right) \quad] \qquad (1)$$

are used, the explicit use of prior odds of the hypotheses in the calculation, introduces a greater potential for subjectivity than for estimation problems where the data often swamp the prior for reasonably chosen priors. However, in principle, there is a way to ease the problem by BIC approximation.

We again argue that priors are not necessarily unreasonably subjective. Most of the priors can be justified in a pragmatic fashion. Therefore, the degree of subjectivity we tolerate is innocuous since it does not affect the unique choice of a hypothesis (([Bandyopadhyay and Boik 99; and Bandyopadhyay and Brittan, 01]), which is largely the goal of science. And so the reasonable constraints on priors allow for the actual practice of science and make it possible in many cases for one of two empirically indistinguishable theories to be better confirmed than the other.

6.2.3. Reviewing the first rejoinder to the second argument for underdetermination:

We began section 6 with a discussion of Earman's first argument for underdetermination. This first argument involves his views on the reliability of the Bayesian inductive method where he maintains that for an inductive method to be reliable, it must be reliable at least for a rich set of possible worlds. He also argues that Bayesian methodology cannot be reliable because it rests on the heavy use of prior probabilities of competing theories, and that is irreparably subjective for him. Had Bayesian methodology been reliable, he contends, we would have good reason to believe one of the two empirically indistinguishable theories to be more likely to be true, but instead underdetermination has become an inevitable corollary.

Recapping our first response to Earman, we have argued that the claim that priors are subjective is complex, because priors need not be completely subjective. After disambiguating the notion, we contend that it is not *necessarily* subjective to give certain theories higher prior probabilities than others. It would be subjective if we did not have reasons for doing so, or if our reasons could not be expected to appeal to anyone other than ourselves. But, we claim that the reasons we give for giving some theories higher prior weights than others may appeal to anyone who chooses priors based on pragmatic considerations. Hence, our account is not necessarily subjective.

The emphasis on our account being pragmatic does not imply that it invites subjectivism of the form that is easy to criticize. On many occasions, the choice of priors is made based on the nature of problems with which we are confronted. Two investigators confronted with the same problem are likely to choose the same family of priors. On some occasions, such as for using BIC (or even Akikean Information Criterion, Akaike [73] and Forster and Sober [94]) any investigator choosing the same criteria chooses the same prior (e.g., the unit prior). Although Bayesians do not have a general cookbook recipe for justifying priors any agent could invoke, they could at least evaluate the contexts and problems and see whether in that context some specific choice of priors *could* be justified. And we discussed how in various contexts priors *could* be justified without opening the door for the specter of subjectivity.

It is also true that in one sense our account is subjective because elimination of infinitely many priors as unacceptable does leave open for another infinitely many priors as acceptable. We, however, argued that this subjectivity does not hinder a unique choice of hypothesis as the best after data have been gathered (([Bandyopadhyay and Boik 99, and Bandyopadhyay and Brittan, 01]). Since our pragmatic account incorporates both objective and subjective elements, we call our account a "quasi-objective" Bayesian account. Our account being a quasi-objective Bayesian account suggests that even though there are some subjective elements in theory-choice capsulated in various prior forms, the objective constrain such as simplicity imposed on the family of various prior forms shows that we have good reason to think that our inductive method need not be unreliable contrary to what Earman has argued that the method has to be unreliable since the prior probabilities on which it relies are necessarily subjective. As a result, between two mutually incompatible empirically indistinguishable theories in a domain, we may have good objective reason to believe one over the other. Therefore, the underdetermination thesis does not necessarily follow even though there are two mutually incompatible empirically indistinguishable theories in a domain.

6.3. The second rejoinder to the second argument for underdetermination:

Consider Earman's second charge that an inductive method has to be reliable in possible worlds or in a very rich subset of possible worlds. An explanation of Earman's motivation might help to understand his position. He thinks that if an agent would like to reject the underdetermination thesis, then one would be required to provide "good reasons" for rejecting it (Earman [93]). Under this scenario, he continues, someone other than a subjective Bayesian might not like the agent's good reason for rejecting the thesis in terms of the agent's subjective degree of belief. So, he explores a possibility in which an agent might still be able to offer good reasons for rejecting the thesis if an inductive method could supply the reason for her belief. He argues that if that inductive method is reliable then only that it can supply her good reasons for giving up the thesis. We have seen that Earman does not think that inductive method is reliable, and, hence he does not believe an agent will ever be able to offer good reasons for rejecting the undetermination thesis.

In contrast, we propose how the notion of reliability could be construed in tune with the practice of science without claiming to offer a knock-down argument against Earman. We would like our inductive methods to work in the actual world and in others more or less like it.[18] But they need not work in all possible worlds, not even when the possibilities are limited formally to those in which all logically possible sequences of observations of a given type occur. Our methods must be adapted to ourselves and to our world; this is reflected in the reasons we offer for assigning different priors to different theories. No doubt our methods will lead us astray in some possible circumstances-- for instance, most dramatically in worlds in which evil demons deceive us or we turn out to be brains in vats. While we do not here mean to address the larger question of radical skepticism that such hypotheses raise, we think they remind us that our inductive methods and the possibilities of knowledge presume all sorts of things about our world. It is evident that when we argue our methods, are by and large reliable, contrary to Earman's argument, we do assume certain structure about the world and the mechanism we are investigating with the help of theories, auxiliaries and background information that has possibly generated the data.

We do not expect our methods to be reliable under all the circumstances that we can imagine, nor do we anticipate we will never be permanently deceived about deductive proofs by following our methods. To assume the world is knowable is not to beg the question or to act as dogmatists. Recall that when the skeptics claim that we do not know that we are not brains in vats, one standard response to the skeptic's challenges in traditional epistemology is that the skeptics have raised too high a standard for what counts as knowledge. Earman's position is similar to that of the skeptics. To demand reliability over an excessively large range of possible worlds is to invite skepticism in through the front door, and this we decline to do.

Summing up:

Deductivism stunted the growth of last century's epistemology of science by proliferating certain theories of science and various problems associated with this theorizing. Various theories of science, such as -- verificationism, hypothetico-deductivism, and instance confirmation-- bear witness to this influence. Its effect is felt in the creation of artificial problems like the underdermination problem, which generates "too many hypotheses" and the raven problem, which generates "too much evidence".

There is a further complication for accepting the underdetermination thesis. If one accepts the underdetermination thesis along with the existence thesis and epistemic realism, then a realist is forced into an uncomfortable position. To be consistent, Earman and van Fraassen reject epistemic realism and accepted the underdetermination thesis. Earman is a Bayesian. He has, however, gone so far as to conclude that there is no escape from accepting the underdeterminaton thesis--even for an inductivist of the Bayesian variety. As a result, epistemic antirealism is the only way out. We have sketched here a pragmatic Bayesian approach that instead motivates abandoning the underdetermination thesis. In addition, Earman argued for the underdetermination thesis on the basis of the demand for reliability, under his strict definition of reliability. We question that demand on the grounds that it was unrealistic and excessive. A more modest notion of reliability, suitable for our quasi-objective stance toward scientific theorizing which is ultimately rooted in pragmatic Bayesianism, remains a desideratum.

We diagnosed the problems of deductivism related to the underdetermination thesis and proposed a solution for these problems by using Bayesianism. Deductivism, however, mistaken as it is as a theory, provided answers to two questions, (i) what counts as evidence? and (ii) how well is a theory confirmed? In this paper, we provided an answer to (ii). As a corollary of our response, the underdetermination thesis is called into question. Hence, one need not be an epistemic antirealist in order to be consistent. However, we are yet to provide an answer to the first question, what counts as evidence?[19] Whether our quasi-objective Bayesianism has a non-trivial answer remains to be seen.[20]

Bibliography

Akaike. H. (1973). “Information Theory and an Extension of the Maximum Likelihood Principle.” Pp.267-281 in *Second International Symposium on Information Theory*, (eds.) by B. N. Petrov and N. Csaki. Budapest: Akademiai Kiado.

Bandyopadhyay, P.S. Boik, R.J. and Basu. P. (1996). “The Curve Fitting Problem: A Bayesian Approach.” *Philosophy of Science,* 63 (supplement): Pp. 264-272.

Bandyopadhyay. P.S. and Boik, R.J. (1999). “Logical Consequence and Beyond: A Look at Model Selection in Statistics.” *Philosophy of Science*, 66 (supplement): Pp. 391-402.

Bandyopadhyay. P.S. and Brittan, G. (2001). “Logical Consequence and Beyond: A Close Look at Model Selection in Statistics.” *Logical Consequence*, edited by J. Woods and B. Hepburn, Pp. 1-17. Oxford: Hermes Science.

Bandyopadhyay. P.S. and Brittan, G. (2006). “Acceptance, Evidence, and Severity.” *Synthese*, 148: Pp. 259-293.

Bandyopadhyay. P.S. (2007). “Why Bayesianism? A Primer on a Probabilistic Philosophy of Science.” *Bayesian Statistics and Its Applications*. (ed.), S. K. Upadhyayay, U. Sing and D.K. Dey. Anamaya Publishers, New Delhi, India.

Bandyopadhyay, P.S. and Brittan, G. (2010). “Two Dogmas of Strong Objective Bayesianism.” *International Studies in the Philosophy of Science*, vol. 24, March, pp.45-65.

Boyd. R. (1996). "Realism, Approximate Truth, and Philosophical Method,” in *Philosophy of Science*, (ed.). Papineau. D. Oxford University Press. Pp. 215-255.

Earman. J. (1992). *Bayes or Bust*? MIT Press, Mass: Cambridge.

Earman. J. (1993). "Underdetermination, Realism, and Reason” in French. P, et al (eds.). *Midwest Studies in Philosophy*, Vol XVIII. University Press. Pp.19-38.

Earman. J. and Salmon. W. (1992). "The Confirmation of Scientific Theories" in *Introduction to the Philosophy of Science*, (eds). Salmon. M et al, University of California Press, Berkeley.

Earman. J (2000): *Hume’s Abject Failure: The Argument against Miracles*, Oxford University Press: England, 2000.

Earman. J. and Roberts. J. (Forthcoming). "Contacts with the Nomic: Two Challenges for Deniers of Humean Supervinience about Laws of Nature"

Forster. M. and Sober. E(1994): “How to Tell when Simpler, More Unified, or Less *Ad Hoc* Theories will Provide More Accurate Predictions,” *British Journal for Philosophy of Science*, **45**, Pp.1-35.

Gelman. A., Carlin. J., Stern. H. and Rubin. D. (2000): *Bayesian Data Analysis*. Chapman & Hall, New York. 2000.

Glymour. C. (2001): *The Mind's Arrow*. MIT Press, Cambridge, Mass.

Good. J. (1992). "The Bayes/non-Bayes Compromise: A Brief Review," in *Journal of the American Statistical Association*, 1992, vol. 87, pp.597-606.

Good. J.(1983). *Good Thinking*: *The Foundations of Probability and Its Applications*, Minneapolis: University of Minnesota Press, 1983.

Grunbam. A. and Salmon. W. (1988). *The Limitations of Deductivism*, (eds.) University of California Press, Berkeley, 1988.

Harman. G. (1997). "Pragmatism and Reasons for Belief," in *Realism/Antirealism and Epistemology*, (ed.) C. B. Kulp. Totowa, NJ. Rowman and Littlefield, 1997. Pp.123-147.

Hawking. S. and Mlodinow, L. (2010). *The Grand Design*. Bantom Books, New York.

Hempel. C. (1965). *Aspects of Scientific Explanations and Other Essays in Philosophy of Science*, The Free Press, New York, 1965.

Hoefer. C. and Rosenberg. A. (1994): "Empirical Equivalence, Underdetermination, and System of the World." *Philosophy of Science*, 61, 1994. pp. 592-607.

Howson. C. and Urbach. P. (1993) *Scientific Reasoning: The Bayesian Approach*, 2nd edition and 3rd edition, Open Court, La Salle, Illinois, 1993 & 2006.

Kass. R. E. and Raftery, A. F (1995) "Bayes Factor," in *Journal of the American Statistical Association*, June, 1995, vol. 90, no. 430.

Kelly. K. (1996). *The Logic of Reliable Enquiry*, Oxford University Press, New York, 1996.

Kelly. K. (2000). "The Logic of Success," in *British Journal for the Philosophy of Science*, **51**, 2000, pp.639-666.

Kukla. A. (1993). "Laudan, Leplin, Empirical Equivalence and Underdetermination," in *Analysis*, 53.1, pp.1-7.

Kukla. A. (1998). *Studies in Scientific Realism.* Oxford University Press, New York, 1998.

Laudan. L (1990). "Demystifying Underdetermination," in *Scientific Theories* (ed.) C. Wade Savage, vol. XIV, University of Minnesota Press, Minneapolis, pp. 267-297. 1990

Laudan. L, and J. Leplin. (1991). "Empirical Equivalence and Underdetermination," *Journal of Philosophy*, 88: pp. 449-472.

Laudan. L. (1995). "Damn the Consequenences," *Proceedings of American Philosophical Association*, 69:2, Presidential Address. Pacific Division, pp. 27-34.

Leplin, J. (1997). *A Novel Defense of Scientific Realism*, New York: Oxford University Press, 1997.

Mayo. D. (1996). *Error: The Growth of Experimental knowledge*, University of Chicago Press, Chicago, 1996.

Press. S. and J. Tanur. (2001). *The Subjectivity of Scientists and the Bayesian Statistical Approach*. John Wiley & Sons: New York.

Norton, J. 2008, "Must Evidence Underdetermine Theory?", in *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, M. Carrier, D. Howard, and J. Kourany (eds.), Pittsburgh: University of Pittsburgh Press, 17–44.

Popper. K. (1959): *The Logic of Scientific Discovery*. London: Hutchinson.

Quine. W. (1975). "Empirically Equivalent Systems of the World", *Erkenntniss*, 9: p. 313-328.

Raftery. A (1995): "Bayesian Model Selection in Social Research," in *Sociological Methodology* (with Discussion), 1995, pp. 111-195 P.V. Marsden (ed.), Cambridge, MA: Blackwell.

Robert. C (1994): *The Bayesian Choice*, New York: Springer, 1994.

Royall. R (1997): *Statistical Evidence*: *A Likelihood Paradigm*. Chapman Hall, New York.

Schwarz. G (1978): "Estimating the Dimension of a Model," *The Annals of Statistics*, 6, pp. 461-464.

Sober. E (1974): *Simplicity*, Oxford University Press, England, 1975.

Sober. E (1995): *From a Biological Point of View*, Cambridge University Press, England, 1995.

Sober. E (2002a): "Comments on Prasanta Bandyopadhyay's "Simplicity: Our View, Their View." *APA, Central Division*, Chicago.

Sober. E (2002b): "Bayesianism—Its Scope and Limits," in Swinburne (2002).

Skyrms. B. (1984): *Pragmatism and Empiricism*. Yale University Press, New Heaven, 1984.

Swinburne, R. (2002): *Bayes's Theorem:* Oxford: Oxford University Press., (ed.).

Van Fraassen. B. (1980). *The Scientific Image*, Oxford University Press, 1980

Van Fraassen. B. (1989). *Laws and Symmetry*, Oxford University Press, 1989.

Van Fraassen. B.(1991). *Quantum Mechanics: An Empiricist View*, Oxford University Press, 1991.

Weinberg. S. (1992) *Dreams of a Final Theory*. New York; Pantheon Books, 1992.

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[1]For a nice survey of the literature contending why the underdetermination thesis is wrong see (Norton [08]).This notion of underdetermination is different from the way Kelly has construed the term in his writings. Kelly (Kelly, [96]) writes, "Underdetermination occurs, roughly, when the same data may arise regardless which of the two competing hypotheses is correct. This is just a special way to argue that no logically reliable inductive method exists relative to scientist's background assumptions. . .. I propose to define underdetermination as the impossibility of reliability (in a given sense) relative to the assumptions at hand. . . We may then hope to map out the structure of degrees of underdetermination in a systematic manner." In his recent article (Kelly [00]), he distinguishes two kinds of underdetermination, global underdetermination and local underdetermination. He has also discussed different degrees of local underdetermination. Although we will consider an argument for underdetermination from a failure of a

reliable method, unlike Kelly we don't construe underdetermination in terms of its hierarchies corresponding to different notions of reliability.

[2] These two arguments are relatively new and there is not much discussion of them in recent literature on the topic. Representative examples are Kukla [98], and especially Mayo [96]. Although Mayo discusses and ultimately rejects different versions of the underdetermination thesis, she has not addressed these arguments.

[3] The term "deductivism" is a misleading term if it is not properly defined. In their jointly edited book, Grunbam and Salmon (Grunbam and Salmon, [88]) use the term in two different ways. Grunbam while reconstructing Coffa's view on "deductive chauvinism," writes, "Coffa applied the term 'deductive chauvinism' to the view that the higher the probability assigned to an explanandum event, the better the explanation yielding it (private communication)." Salmon, on the other hand, says "Deductivism read: deductive chauvinism) is the view that the only logical devices required in the empirical sciences are deductive." Salmon in his joint article with Earman (Earman and Salmon [92]) uses deductivism in the same sense when he brands Popper as a deductivist. These two notions of deductivism seem to be misleading unless an attempt is made to relate one notion to the other. If we are to be charitable to them, we should accept Salmon's construal of deductivism, because it is the standard characterization. Salmon's sense of deductivism has to do with taking deductive method as used in mathematics, logic and geometry as the paradigm method of understanding science. We, however, are not working with this sense of deductivism.

[4] Earman has attributed this example to Laudan and Leplin. See Laudan and Leplin [91].

[5]We follow Boyd [96] in distinguishing the underdetermination thesis from empirical indistinguishability thesis.

[6] For a different kind of criticism against Laudan and Leplin, see Kukla's papers. (Kukla, [93], and Kukla [98])

[7]We are aware that there are serious controversies about the adequacy of Bayesianism for scientific purposes, but obviously we cannot address all such issues in this paper. For more on those issues see Howson and Urbach [93], and Earman [92] for further discussion on these topics.

[8] Our claim that "evidence" could include observable consequence is consistent with our previous argument that observable consequence is neither necessary nor sufficient for being supporting evidence. When we say that evidence is not necessarily observable consequences, this merely says that some evidence may be something that is not an observable consequence. In our case, it was something that was not a consequence at all. That, of course, leaves it open that some (or even most) evidence may be observable consequences. Similarly, in denying that being an observable consequence is sufficient for being evidence, we simply asserted that some observable consequences may not be evidence. The claims we make are very week, but suffice to dispose of the view that evidence = logical consequences.

[9] We are two Bayesians who are worried about the subjective nature of prior probability. Several non-Bayesians also find it to be problematic with Bayesianism. For example, one of Mayo's main criticisms (Mayo, [96]) against the Bayesian is that the latter's notion of prior smacks of subjectivism. For a defense of Bayesian position, see Howson and Urbach (93), chapter 15, pp. 417-419. In the following pages especially in section 6, we find fault with her criticism because it is over-simplicistic.

[10] Hawking and Mlodinow [10], p.141. They discuss only the probability of the Pope being a Chinese without addressing Bayesianism or *a priori* probability.

[11] For a different perspective on the notion of simplicity see Sober, (Sober [75], Sober [1995], and also Sober in Forster and Sober [94]). In fact, Sober has subscribed to two accounts of simplicity. 1. A theory is simpler than the other with regard to substantive background assumptions. His first notion of simplicity is influenced by Jack Good's views on confirmation. See Good [83]. 2. According to Sober's second notion, a theory is simpler than the other just in case the former contains fewer numbers of adjustable parameters than the latter with respect to some background assumptions of the contending mathematical theories. Sober's second notion draws its strength from his joint work with Forster. We thank Sober for helping us understand his position better. Our view of simplicity overlaps with Swinburne's view of simplicity. For the latter view see Swinburne [01].

[12] See Swinburne's introduction in Swinburne ed. [02]. See also Sober [02b] in Swinburne, ed. [02]. Although the former is a Bayesian and the latter is a non-Bayesian, both of them share the same view about the division of Bayesianism under these two heads

[13] Empirical indistinguishability is sometimes defined in terms of theories entailing (perhaps with background assumptions) the same evidence. Our definition is more general, since it does not specify the relation between evidence and the theory it confirms.

[14] He requires the set to include all but a set of measure 0 of possible worlds from a larger set of possible worlds including all possible values of evidence statements.

[15] The problem related to reliability of scientific methods is Earman's one live research problem. For his recent position on reliability, see Earman and Roberts [forthcoming]. In this paper, they offer two arguments, epistemological and semantic arguments for the Humean supervenience (HS) thesis. According to HS thesis, there do not exist any two possible worlds that agree with respect to Humean base, but disagree on what the laws are. The central theme of their arguments is that if laws matter to science, then the only way to make sense of that is to assume HS thesis. Their epistemological argument rests heavily on assuming the reliability of scientific methods. This line of defense of HS thesis on the basis of reliability of methods goes against the spirit of Earman's argument for underdetermination. However, we won't address this yet to be published Earman's paper.

[16] Earman's views on Bayesian subjectivism have undergone changes over the years. In Earman's *Bayes or Bust?*, he was prone to Bayesian personalism (subjectivism). However, he made an observation that gave the impression that he also cared for objectivity of scientific inference. To quote, "This [i.e., the value of the crucial factor prob(E/not T & K)] may be acceptable to the thorough going Bayesian personalists, but it is unacceptable to anyone who wants to find a modicum of objectivity in scientific inference." (p.168). In 92, he was yet to be a through going personalist. He didn't seem to endorse full-scale personalism in that context. In 93 during the time Earman wrote Earman [93], he became converted to personalism. One possible way out of the inconsistent triad (that consists of semantic realism, epistemic realism and the underdetermination thesis together) was to invoke subjective priors. He, however, didn't adopt that move in that paper. He opted for antirealism instead. In this paper, however, he didn't consider one possibility, that is, there could be some constraints on priors which need not lead to subjectivism. In his recent book, *Hume's Abject Failure*, he has considered both constraints, subjective and objective, on priors. It is clear that he is a full-blown subjectivist (personalist) in this book. According to Earman, satisfying probability calculus provides the only constraint on the structure of an agent's belief within Bayesianism. He holds further that this rationality constraint of Bayesianism fails to deliver the truth or the objectivity of the agent's beliefs. We thank Earman for clarifying his position to us through e-mails.

[17] Some Bayesians might think that our approach to inferential problems, whether one employs estimation technique, or hypothesis testing technique, is completely misguided. According to our Bayesian critics, after all, the purpose of most inferential studies is to provide the statistician or the client with decisions. Hence, for them, it is much more relevant to provide evaluation criteria of *decision* (for such a position, see Robert, C [94, p.39]) However, we disagree with them, because we are primarily interested in non-decision theoretical approach to theory choice.

[18] We are not alone in construing reliability in this manner. Goldman [86] also confines reliability to a process to our actual world. See Goldman [86, pp. 105-113]. We also come across similar views about the requirement of an inductive method in Glymour when he discusses success criteria of an algorithm. Following Earman, we talk about the reliability of inductive methods. Glymour, in constrast, writes, "an algorithm for learning need not succeed in all possible worlds, but only in large and interesting set of possible worlds…; weakening the success criteria… strengthens the logical content of learnable hypotheses. The less that is demanded for success, the more success there will be." (Glymour, 2001, p.16). However, one fundamental difference between Glymour and us is that while Glymour is a non-Bayesian, we are Bayesians.

[19] For a Bayesian account of evidence, see Bandyopadhyay et. al., [06] and Bandyopadhyay et. al., [07].

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