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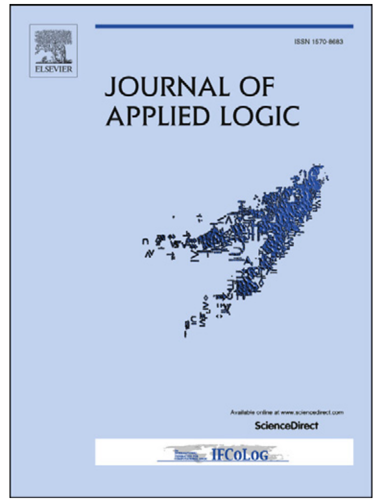

An application of Carnapian inductive logic to an argument in the philosophy of statistics

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Title
An application of Carnapian inductive logic to an argument in the philosophy of statistics

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Abstract
I claim that an argument from the philosophy of statistics can be improved by using Carnapian inductive logic. \citet{GelmanShalizi2012} criticise a philosophical account of how statisticians ought to choose statistical models which they call `the received view of Bayesian inference' and propose a different account inspired by falsificationist philosophy of science. I introduce another philosophical account inspired by Carnapian inductive logic and argue that it is even better than Gelman and Shalizi's falsificationist account.

Keywords
Inductive logic, Carnap, Popper, Falsificationism, philosophy of statistics, model choice

An application of Carnapian inductive logic to an argument in the philosophy of statistics

Abstract

I claim that an argument from the philosophy of statistics can be improved by using Carnapian inductive logic. Gelman and Shalizi [10] criticise a philosophical account of how statisticians ought to choose statistical models which they call 'the received view of Bayesian inference' and propose a different account inspired by falsificationist philosophy of science. I introduce another philosophical account inspired by Carnapian inductive logic and argue that it is even better than Gelman and Shalizi's falsificationist account.

1. Introduction

The structure of this paper is as follows: section two introduces the notion of a philosophical account of statistical model-choice, starting with the 'received view' account according to which statistical models represent all relevant factors of a statistical investigation. It summarises Gelman and Shalizi's criticism of this account. In contrast to the received view, Gelman and Shalizi see statistical models as representing only the beginning of a statistical investigation; they argue that statistical models cannot and should not describe the important tasks of searching for ways in which models fail, and then conceiving of improved models on the basis of this testing. Section two ends by presenting the 'falsificationist' account, which Gelman and Shalizi introduce as an improvement on the received view.

Section three develops an alternative 'Carnapian' account of statistical model-choice. In section four I argue that the Carnapian account is a further improvement: it shares the advantages that Gelman and Shalizi identify in the falsificationist account, and in addition is more technically fruitful and philosophically well-grounded.

Finally, section five addresses several qualms about Carnapian inductive logic which might cause misgivings about a model-choosing philosophy inspired by it.

2. Gelman and Shalizi's arguments

2.1. Philosophical accounts of statistical model-choice

Gelman and Shalizi [10], like this paper, concerns collections of succinct stipulations as to how statisticians ideally ought to conduct investigations involving probabilistic models. I call these collections 'philosophical accounts of statistical model-choice'. In order to make it easy to discuss shared features of different approaches at the same time, I do not require that accounts of statistical model-choice specify an approach to ideal model-choice completely.

Philosophical accounts of scientific methodology are important because they can influence scientific research. As Gelman and Shalizi put it,

> ...even those [scientists] who believe themselves quite exempt from any philosophical influences are usually the slaves of some defunct methodologist."
>
> Gelman and Shalizi [10, p.31]

2.2. Criticism of the 'received view of Bayesian inference'

Gelman and Shalizi aim to counteract the influence of what they call 'the received view of Bayesian inference', a philosophical account of statistical model-choice that, they claim, has had a negative effect on statistical research. According to the received view, Gelman and Shalizi write,

> Anything not contained in the posterior distribution $p(\theta \mid y)$ is simply irrelevant...
>
> Gelman and Shalizi [10, p.9]

I therefore consider the following, slightly more general, stipulation to be a key tenet of the received view:

RV All desiderata that are relevant in a statistical investigation should be represented formally in a statistical model. Other factors should be disregarded.

This stipulation does not amount to a fully-fledged philosophy of statistics and therefore should not be seeen as encapsulating the received view, which must include other stipulations: perhaps that models should be chosen so as to fit given data. Nonetheless, since it is where Gelman and Shalizi focus their criticism, this is the only aspect of the received view that we need to consider.

Gelman and Shalizi argue that **RV** is incompatible with certain facts about statistical research as it goes on in the real-world, as there are important uses in statistical investigations for knowledge that is not represented by a statistical model.

In Gelman and Shalizi [10, §3] they argue that it is practically impossible to represent all the assumptions that might be entertained during the course of an investigation in the form of a statistical model.

In Gelman and Shalizi [10, §4], Gelman and Shalizi claim that knowledge that is not represented in a statistical model plays an important role in model-checking. Bayesian models are typically tested by investigating the ways in which empirical data differs from data simulated using the fitted model. Statistician's knowledge enables them to devise tests which distinguish unimportant, patternless discrepancies which can safely be ignored from systematic differences which might cause the model to be revised. According to **RV**, such knowledge should be disregarded unless it is represented in a statistical model: Gelman and Shalizi argue (Gelman and Shalizi [10, §4.3]) on methodological grounds that this is not always feasible.

2.3. The falsificationist account

Gelman and Shalizi propose an alternative philosophical account of statisticians' model-selection choices that is inspired by falsificationism, a prominent approach to the philosophy of science. Given Gelman and Shalizi's use of the term, I call this philosophical account 'falsificationist'. This label should not be read as indicating agreement with the philosophy of Karl Popper: I argue below that, as Gelman and Shalzi present it, the falsificationist account is in fact incompatible with Popper's philosophical approach. The falsificationist account consists of the following stipulation:

F A candidate statistical model should first be chosen for consideration using the statistician's judgement. The model should then be confronted with data and either rejected or cautiously accepted depending on how well it is found to resemble the data source.

Gelman and Shalizi think that the way in which resemblance between models and data sources should be measured, as well as the level of non-resemblance required for rejection, should depend on the particular circumstances of the investigation. They write:

> . . . the hypothesis linking mathematical models to empirical data is not that the data-generating process is exactly isomorphic to the model, but that the data source resembles the model closely enough, in the respects which matter to us, that reasoning based on the model will be reliable.
> Gelman and Shalizi [10, p.20]

Gelman and Shalizi's principled abstention from specifying exactly how 'resemblance' between statistical models and data sources should be measured distinguishes their philosophical account from **RV**. According to the received view only resemblance that is represented formally in a statistical model can be relevant in a statistical investigation, whereas the falsificationist account allows tests of resemblance that do not have this property.

The key feature of resemblance between models and data, according to Gelman and Shalizi, seems to be that, if there is resemblance, then the assumption that 'reasoning based on the model will be reliable' is justified. This desideratum encompasses standard tests of model-fit to the extent that reasoning based on poorly-fitting models is unreliable.

Below I grant all of Gelman and Shalizi's claims about statistical research in practice. Specifically, I assume that working statisticians often have reasons other than knowledge-representation for choosing between statistical models, as well as uses for knowledge that is not represented in the form of a statistical model. In addition I assume that statisticians do and should choose models in roughly the way suggested by Gelman and Shalizi's falsificationist philosophical account, cautiously adopting models after testing them for reasoning-justifying resemblance to data in the situation- and priority-sensitive way that Gelman and Shalizi outline. These assumptions allow me to focus on other aspects of Gelman and Shalizi's account that I find controversial.

Despite the fact that I do not dispute its empirical adequacy, I argue below that Gelman and Shalizi's falsificationist philosophical account should be rejected in favour of an alternative Carnapian one.

3. The Carnapian account

In this section I introduce the Carnapian account of statistical model choice, according to which statistical models should be chosen according to their performance as formalisations of inductive assumptions. I claim that this account is more fruitful than the falsificationist one and also in better harmony with Gelman and Shalizi's arguments.

Section 3.1 summarises the philosophy that inspires this account, including the devices—'system of inductive logic'—which Carnap proposed for formalising inductive assumptions, along with Carnap's approach to formalisation itself. As far as possible, I reproduce Carnap's original presentation from [3] and [4], although I make certain terminological departures, indicated below, for the sake of ease of exposition and agreement with more recent literature on inductive logic. Section 3.2 shows how any statistical model of a certain kind—probabilistic constraints on finitely valued random variables—can be interpreted as a system of inductive logic. Section 3.3 uses this discussion to formulate the Carnapian account of statistical model choice.

I emphasise that I see the significance of the Carnapian account as narrowly philosophical. I do not mean to suggest that statisticians ought to adopt the formal framework of Carnapian inductive logic. This would involve undue effort and in any case, as we shall see, the Carnapian framework is limited in certain ways. Rather, I discuss the relationship between systems of inductive logic and statistical models with a view to applying philosophical arguments from the literature on inductive logic to the problem of statistical model-choice.

3.1. Carnapian inductive logic

A Carnapian 'inductive method' is an ordered pair (L, m) where L is a formal language and $m : SL \to \mathbb{R}_{\geq 0}$, known as a 'measure function', is a function mapping L's sentences to positive real numbers.[1]

[1]Carnap formulated inductive methods slightly differently, as pairs (L, c) where the 'confirmation function' $c : SL \times SL \to \mathbb{R}_{\geq 0}$ has two arguments. However, all inductive methods that feature in this paper can be formulated using either measure functions or confirmation functions by stipulating that $c(h, e) = \frac{m(h \wedge e)}{m(e)}$ if $m(e) \neq 0$ and is undefined otherwise. Carnap

This formal apparatus restricts the Carnapian inductive logician in several ways. The requirement that measure functions' outputs be single real numbers precludes the possibility of representing inductive reasoning using functions that associate sentences with sets of real numbers, while the requirement that their domain be the set of all sentences rules out representations where certain sentences do not receive any values at all.

3.1.1. First-order unary languages

Carnap focused on inductive methods with first-order unary languages. All the languages I mention below are assumed to be of this kind. A first-order unary language L_k has an alphabet consisting of: some natural number k one-place relation symbols $P_1, \ldots, P_k$ known as primitive predicates, countably infinitely many constant symbols $a_1, a_2, \ldots$, the logical connective symbols $\wedge$, $\vee$ and $\neg$, the quantifier symbols $\forall$ and $\exists$ and countably infinitely many variables $x_1, x_2 \ldots$, together with standard rules specifying which combinations of these symbols are formulae and sentences. Such languages can be identified by their number k of primitive predicates. For ease of notation, where this number is unimportant I sometimes refer to languages using L.

For every language L_k there are 2^k open formulae $\alpha_1(x), ..., \alpha_{2^k}(x)$ which, following Paris and Vencovská [15], I call 'atoms'. Each atom $\alpha_i(x) = \pm P_1(x) \wedge \ldots \wedge \pm P_k(x)$ is a conjunction of either the negated or un-negated form of every primitive predicate in L_k. For any constant a, $\bigvee_{i=1}^{2^k} \alpha_i(a)$ is logically true while $\alpha_i(a) \wedge \alpha_k(a)$ is logically false, provided that $i \neq k$. Since they are analogous in this way to the atoms of a boolean alegbra, I prefer to discuss atoms rather than 'Q-predicates', which play an analogous formal role in the writings of Carnap and other authors.

The requirement to use first-order unary languages imposes some restrictions on the phenomena that inductive logic can describe: in particular, reasoning involving non-unary relations or continuous magnitudes is difficult if not impossible to depict without resorting to richer languages.

Such enrichments have been attempted. Paris and Vencovská [15] attempt to address the first of problem by extending inductive logic to polyadic languages. Skyrms [21] contains a discussion of attempts to solve the problem of depicting reasoning about continuous magnitudes by abandoning the framework of classical logical languages altogether. Skyrms claims that Ferguson [8] and Blackwell and MacQueen [1] show how to construct "confirmation functions for the case where the outcome can take on a continuum of possible values" in a way that, despite the use of a different formal framework, is nonetheless "quite consonant with Carnapian techniques".

introduces confirmation functions using this convention at Carnap [3, p.295]; The difference is therefore merely terminological.

3.1.2. Systems of inductive logic

A 'system of inductive logic' is a statement with the form 'inductive methods that do not satisfy the set of conditions X are inadequate'. Systems of inductive logic differ only in the conditions that they require inductive methods to satisfy.

Carnap sought to discover systems of inductive logic that can usefully formalise real-world inductive assumptions, to study such systems' mathematical properties and to promote their use in science and philosophy.

3.1.3. Carnap on formalising inductive assumptions

Carnap coined the term 'explication' to describe the situation that obtains when such a useful formalisation occurs, and it becomes practical to replace an informal term, or 'explicandum'—in our case, the natural-language expression of an inductive assumption—with a formal 'explicatum' such as a system of inductive logic.

Carnap drew a sharp distinction between what he called internal and external questions that may arise in connection with inductive-logical explicata. The internal questions relate to which inductive methods the chosen system of inductive logic identifies as inadequate, and are in general answerable using deductive reasoning. External questions, on the other hand, concern whether the choice of system was well-made, and are far less easily answered. Carnap puts this point as follows:

> An internal question of induction is a question within a given system, e.g., concerning the value of $c^{\star}$ for a given case. The answers to questions of this kind are analytic. On the other hand, an external question of induction is raised outside of the inductive system; a question of this kind may concern the choice of an explicatum for probability, in other words, the practical question whether or not to accept a certain c-function or at least a class of such functions...
> Carnap [5, p.981]

Although Carnap thought that the answer to an external question of induction is rarely clear-cut, he nevertheless proposed four broad criteria for judging explicata in general, which are as follows[2]. Explicata should, Carnap thought, be at least similar enough to their explicanda to replace them in some contexts without misunderstanding ('similarity'), as well as forming part of a harmoniously connected system of scientific concepts ('exactness') and being as easy as possible to understand ('simplicity') and theorise about ('scientific fruitfulness').

3.1.4. Some important adequacy criteria

Below I characterise several adequacy conditions that make up systems of inductive logic that become important later in this paper and explain the in-

[2]See Carnap [3, p.4] for remarks on clear-cutness and Carnap [3, §3] for Carnap's criteria.

ductive assumptions that they are intended to explicate.[3]

Probabilism. Probabilism requires inductive methods (L, m) with first-order unary languages to satisfy the following conditions for all predicates P and sentences θ, ϕ and $\exists x(P(x)) \in SL$:

P1 If θ is a logical truth then $m(\theta) = 1$

P2 If θ logically entails $\neg\phi$ then $m(\theta \vee \phi) = m(\theta) + m(\phi)$

P3 $m(\exists x(P(x))) = \lim_{n\to\infty} m(P(a_1) \vee \ldots \vee P(a_n))$

Any measure function of an inductive method that satisfies probabilism can be defined by specifying how it treats sentences consisting only of atoms[4].

Probabilism formalises the inductive assumption that reasoning should proceed probabilistically. This assumption has been defended on various different grounds. For example, Howson argues that probabilistic reasoning is uniquely 'logically' justified[5], and there is also a well-known 'Dutch Book' argument to the effect that reasoning should be probabilistic if it is linked to betting behaviour in a certain way[6]. The assumption that reasoning should be probabilistic might also be defended for methodological reasons such as those outlined by Gelman et al. [9, Ch.1].

Constant exchangeability. Constant exchangeability imposes the following condition on inductive methods (L, m):

Ex For any natural number $n \in \mathbb{N}$, any sentence $\theta(a_1, \ldots, a_n) \in SL$ mentioning constants $a_1, \ldots, a_n$ and any permutation σ of the natural numbers, $m(\theta(a_1, \ldots, a_n)) = m(\theta(a_{\sigma(1)}, \ldots, a_{\sigma(n)}))$

Constant exchangeability explicates the assumption that it is irrelevant from an inductive point of view which particular entities happen to be represented by particular logical constants. This assumption might be appropriate, for example, if the constants represent individuals and the available information does not discriminate between them.

Johnson's sufficientness postulate. Johnson's sufficientness postulate requires that inductive methods (L, m) have the following property:

JSP For any natural number n, atoms $\alpha_j(x)$ and $\alpha_{h_1}(x), \ldots, \alpha_{h_n}(x)$ of L and constants $a_1, ..., a_{n+1}$, the value of $m\left(\alpha_j(a_{n+1}) \mid \wedge_{i=1}^{n} \alpha_{h_i}(a_i)\right)$ depends only on n and the number r of conjuncts $\alpha_{h_i}(a_i)$ in the second argument such that $h_i = j$.

[3] Definitions of all these conditions are taken from Paris and Vencovská [15]. See Ch.3 for probabilism, Ch.6 for constant exchangeability and Ch.17 for Johnson's sufficientness postulate.

[4] See Paris and Vencovská [15, Ch.8]

[5] See Howson [11]

[6] See Paris and Vencovská [15, Ch.5] for a presentation of the 'Dutch Book' argument.

Given a suitable interpretation, JSP formalises a natural inductive assumption. Conditional measure functions must be interpreted as representing the degree to which their second arguments give evidence for their first ones, and constants as representing individuals whose interesting properties are represented by which atom they instantiate. JSP can then be taken to formalise the assumption that there are only two relevant factors influencing the degree to which a sample of n individuals' interesting properties is evidence for a different individual having some configuration α_j of properties: the number n of individuals sampled and the number r of those individuals with configuration α_j.

This assumption is often appropriate: for example, suppose one is drawing sweets from a large (for the sake of argument suppose it is large enough to contain infinitely many sweets) bag and assigning degrees of belief to the possible flavours—lemon or lime—of the next sweet. If one has some initial hunch as to the likely distribution of flavours, but also wishes to take into account the evidence in one's hand, one might reasonably adopt the policy of initially allocating degrees of belief based on the hunch, and then steadily adjusting them so as to agree more and more with the observed frequencies. The inductive assumption formalised by Johnson's sufficientness postulate would ensure that this policy is observed.

3.2. Connection with statistical models

Below I develop an approach to formulating statistical assumptions in the form of systems of inductive logic suggested by Zabell [24]. Zabell claims that statistical research can be improved by considering statistical models' corresponding systems of inductive logic because the latter are a useful vehicle for clearly expressing inductive assumptions.[7] He names the general approach of choosing between statistical models on the basis of such assumptions 'pragmatic Bayesianism'.

I agree with Zabell's claim, and in this section build on his work by showing how any statistical model of a certain kind—a set of probabilistic constraints on finitely-valued random variables—has a corresponding system of inductive logic.

In contrast with results such as De Finetti's representation theorem, which have important applications to the practice of statistics, my demonstration is not technically significant, as it amounts to a re-writing of the statistical model using different terminology.

Nonetheless, I believe the exercise is interesting from a philosophical point of view, as it shows that there is a clearly specified sense in which an important class of statistical models can be thought of as systems of inductive logic. This

[7]For an interesting counterpoint see Romeijn [19], which contains an argument for the converse claim that inductive logic can be improved by incorporating ideas from mathematical statistics.

means that Carnap's arguments concerning choices of systems of inductive logic can also be applied to the problem of choosing between such statistical models.

3.2.1. Probabilistic constraints on finitely-valued random variables

Statistical models often take the form of probabilistic constraints on finitely-valued random variables. Finitely-valued random variables are functions $X : \Omega \to R \subset \mathbb{R}$ from an underlying, at-most-countable, state-space Ω to a finite range R of real numbers. I use the symbol $\mathbf{X}$ to represent the set of all random variables under consideration in a particular case. Random variables are often used to represent repeated experiments, with the members of Ω standing for repetitions and the members of R for the possible results. For example, $X(\omega_6) = 1$ might represent that the sixth sweet drawn from a tube has flavour number 1.

A 'configuration' of random variables is a set of states $K = \{\omega : X_1(\omega) = v_K^1, \ldots, X_n(\omega) = v_K^n\}$ determined by the values of the random variables $X_1, \ldots, X_n$.

Probabilistic constraints on finite-valued random variables are sets of probability spaces $M = \{(\Omega, \mathcal{P}(\Omega), \mathbf{X}, Pr) : conditions\}$ such that $Pr : \Omega \to [0, 1]$ satisfies conditions preventing it from assigning certain numbers to certain configurations of random variables.

As examples of probabilistic constraints on finitely-valued random variables, consider the following models, which Gelman and Shalizi take as typical of statistical research in practice[8] and were part of an investigation into how voting behaviour is related to income in different states of the USA:

$$Pr(y = 1) = logit^{-1}(a_s + bx) \qquad \text{(Model One)}$$

and

$$Pr(y = 1) = logit^{-1}(a_s + b_s x) \qquad \text{(Model Two)}$$

In these models y, s and x are random variables, b is a real-valued parameter and a_s and b_s stand for real-valued components of the parameter vectors $\vec{a}$ and $\vec{b}$. $logit^{-1}$ is the logistic function, which is a transformation used to make the linear constraints on the right hand sides of the equations above apply to the probabilities on the left hand sides. The underlying state-spaces are not explicitly referred to, but their states represent voting acts.

In both models the random variable y has two possible values, 1 and 0, representing a vote going to one or the other of two available political parties. s has 50 values corresponding to the vote occurring in one or another state, and x has five values representing the income quintiles a vote's voter might belong to. The models' parameters can be adjusted so as to achieve the best possible fit with empirical data documenting real votes.

Each model is a probabilistic constraint on finite-valued random variables: in this case the members of Ω are the voting acts; each member of the set of random variables $\mathbf{X} = \{y, s, x\}$ has a finite range and the equations specifying each model

[8]See Gelman and Shalizi [10, §2.1]

exclude probability models depending upon the values their measures assign to certain configurations of the random variables in $\mathbf{X}$.

3.2.2. Probabilistic constraints as systems of inductive logic

Every probabilistic constraint $M = \{(\Omega, \mathcal{P}(\Omega), \mathbf{X}, Pr) : \textit{conditions}\}$ can be expressed in the form of a system of inductive logic using the procedure I outline below. The idea is to find a logical language whose constants represent M's states and whose atoms represent the possible values of its random variables, and then to reproduce M's conditions as inductive logical adequacy criteria. After showing how this can be done in general I work through an example using the voting models introduced above.

First, suppose that each of the random variables $X_1, ..., X_l \in \mathbf{X}$ have ranges $R_1 = \{v_1^1, \ldots, v_{q_1}^1\}, \ldots, R_l = \{v_1^l, \ldots, v_{q_l}^l\}$, each with, respectively, $q_1, q_2, \ldots, q_l \in \mathbb{N}$ possible values. Choose the language with $q_1 + q_2 + \ldots + q_l$ primitive predicates and a set of constants with the same cardinality as Ω. Call this language L_M and the set of its constants C_M.

Let $f : \Omega \to C_M$ be an arbitrary bijection associating the states of Ω with the constants of L_M.

L_M will have exactly one primitive predicate for every possible value of each of M's random variables, and its constants will correspond to M's states. The predicates can be labelled $P_{X_1=v_1^1}, \ldots, P_{X_l=v_{q_l}^l}$ accordingly. However, L_M's atoms cannot do the job of representing configurations of values of random variables without imposing more restrictions, because we have not yet built in the impossibility of different values of the same random variable being instantiated at the same time.

This feature can be captured by labelling each of L_M's atoms either 'good' or 'bad' depending on whether or not it negates all except one of the predicates corresponding to each random variable. To be precise, an atom is 'good' if and only if, for every random variable X_i, it fails to negate exactly one predicate out of $P_{X_i=v_1^i}, \ldots, P_{X_i=v_{q_i}^i}$, while negating all the others. Each good atom then picks out a configuration of values of random variables, which can be identified according to the predicates it does not negate: for example $\alpha_{v_a^1 v_b^2 v_c^3}(x)$ represents the configuration $X_1 = a, X_2 = b, X_3 = c$.[9]

With these terminological choices made, the probabilistic constraint can be reproduced as the system of inductive logic including the following adequacy conditions, applying to all inductive methods (L, m):

- $L = L_M$

[9]This labelling procedure is similar to the one outlined by Carnap at Carnap [6, §2.A], where each predicate is allocated a 'family' in such a way that the members each family's predicates partition of the language's constants, that is, for any family $F = \{P_{F_1}, \ldots, P_{F_n}\}$ and constant a, $P_{F_j}(a) \wedge P_{F_k}(a)$ is logically false provided that $j \neq k$ and $\bigvee_{i=1}^{n} P_{F_i}(a)$ is logically true. Such families are analogous to the ranges of finitely-valued random variables, so the admissible predicates of a language with families correspond exactly to the 'good atoms'.

- Probabilism.
- $m(\alpha_b(a)) = 0$ for any bad atom α_b and constant a.
- For any state ω such that $X_1(\omega) = v^1_{j_1}, \ldots, X_l(\omega) = v^l_{j_l}$, if M's conditions prevent $Pr(\omega)$ from being a certain number, then $m(\alpha_{v^1_{j_1} \ldots v^l_{j_l}}(f(\omega)))$ cannot be that number either.

3.3. Carnapian account of model choice

The fact that probabilistic constraints on finitely-valued random variables correspond to systems of inductive logic in this way suggests that inductive logical arguments can potentially be applied to discussions about statistical models. The following 'Carnapian' philosophical account of statistical model-choice does exactly this:

C Statisticians ought to choose whichever statistical models best explicate the inductive assumptions they wish to entertain.

This account is 'Carnapian' in the sense that the proposed rationale for choosing between statistical models is exactly the one that Carnap proposed for choosing between systems of inductive logic, namely suitability as an inductive assumption.

Reading this inductive logical rationale into statistics is plausible because statisticians often treat statistical models as embodying assumptions. This can be seen, for example, from Gelman and Shalizi's account of models as sanctioning certain kinds of reasoning in the context of model-checking. In this regard it is important to note that the Carnapian account does nothing to prevent statisticians from testing models on the basis of how well they fit given data. To the extent that bad fit is evidence that the assumptions a model embodies are implausible and therefore not worth entertaining, the Carnapian account advocates fit-based model-checking. Conversely, circumstances in which this is not the case, and even very badly-fitting models are worth entertaining, either because of untrustworthy data or lack of alternatives, present no problem for the Carnapian account.

Another reason why it makes sense to apply Carnap's philosophical approach to statistical model-choice is that, as we have seen, statistical models and systems of inductive logic are formally similar. In the case of statistical models in the form of probabilistic constraints on finite random variables, the models themselves can be interpreted directly as systems of inductive logic. In cases, such as those involving continuous random variables, where statistical models do not obviously have corresponding systems of inductive logic, there is nonetheless a strong analogy.

I emphasise that the Carnapian account does not commit a statistician to using either the formal framework of Carnapian inductive logic or sharing Carnap's views about the conditions under which particular assumptions, such as constant exchangeability or Johnson's sufficientness postulate, are appropriate.

3.3.1. Example

This section shows how Gelman and Shalizi's example of reasoning about statistical models can be interpreted according to the Carnapian account. Model One and Model Two from the example above are formulated as systems of inductive logic, and Gelman and Shalizi's reasons for Model Two are construed as demonstrating its suitability as an inductive assumption.

Both models have three random variables with, respectively, 2, 50 and 5 possible values. We therefore choose a language L_{57} with 57 primitive predicates.

500 out of L_{57}'s 2^{57} atoms are good, as there are $2 \times 50 \times 5 = 500$ possible combinations of values of random variables. These good atoms can be labelled so that, for example $\alpha_{y_1 s_{44} x_{-2}}$ might represent that a vote for party number 1, the Republicans, occurs in state 44, say New Hampshire, and is performed by a voter in income quintile -2, the bottom one.

A system of inductive logic can now be introduced whose adequacy conditions reproduce the constraints in the original statistical model. For both of our models this is straightforward: the system of inductive logic corresponding to Model One, henceforth 'System One' requires that for any constant d and values s_j and x_k, there are real-valued parameters b and $a_1, ..., a_{50}$ such that

$$m(\alpha_{y_1 s_j x_k}(d)) = logit^{-1}(a_j + bx_k). \tag{1}$$

Similarly, 'System Two', corresponding to Model Two, imposes the requirement that there must be real-valued parameters $a_1, ...a_{50}$ and $b_1, ..., b_{50}$ such that

$$m(\alpha_{y_1 s_j x_k}(d)) = logit^{-1}(a_j + b_j x_k). \tag{2}$$

Interpreting the two models as systems of inductive logic in this way, we can see that the reasoning that lead Gelman and Shalizi to prefer Model Two to Model One can be reconstructed as arguments that would plausibly persuade a Carnapian inductive logician to reject System One in favour of System Two.

On this inductive logical reconstruction, the choice of which system to use, if any, is an external question: the problem is to determine whether either system usefully formalises an inductive assumption. As Gelman and Shalizi note, System One has the advantage of simplicity, since it is has a well-understood form and not too many parameters. It was therefore a reasonable first attempt at a useful formalisation. However, in view of its systematic lack of fit with the empirical data, System One arguably represents an implausible inductive assumption. While it might be useful to explicate even such a dubious assumption in some circumstances, the availability of System Two, which is more plausible but not too much more complicated, means that in this circumstance the best option is to reject System One in its favour.

4. Advantages of the Carnapian account

The Carnapian account of how statisticians ought to choose statistical models has two advantages over the falsificationist account. First, treatingt statistical models as systems of inductive logic can make it easier for statisticians to articulate their reasons for choosing particular models (§4.1). Second, the Carnapian account is in better harmony than the falsificationist account with Gelman and Shalizi's claims about good statistical practice (§4.2).

4.1. Technical Fruitfulness

The Carnapian account is technically fruitful, as considering statistical models' corresponding systems of inductive logic can allow statisticians to express some of their reasons for choosing models more articulately than would be possible otherwise.

When it is possible to find a model's corresponding system of inductive logic, and that system explicates a particular inductive assumption, the model can be defended or attacked according to how appropriate that assumption is. In this way, previously unarticulated justifications can be replaced by transparent ones based on explicitly stated principles.

Finding statistical models' corresponding systems of inductive logic can also help to make clear whether, in a particular case, a model's adoption is primarily based on analytical convenience or if it represents a substantive judgement about the nature of the scientific problem being confronted.

This advantage was identified by Zabell[10], who argues that statisticians' reasons for choosing models often lack such clarity.

To demonstrate this technical fruitfulness, I present below two examples of important classes of statistical models whose corresponding systems of inductive logic formalise natural inductive assumptions.

4.1.1. Independent, identically distributed random variables and constant exchangeability

De Finetti's representation theorem identifies a duality between the system of inductive logic requiring that probabilism and constant exchangeability be satisfied and an important statistical tool, namely 'IID models'.

An IID model consists of a set A of probabilistic constraints on finitely valued random variables satisfying the 'IID conditions', together with a probability distribution $\mathbf{Pr} : \mathcal{P}(A) \to [0,1]$.

The IID conditions require that the sets A have only independent and identically distributed random variables. To see what this means, consider an arbitrary set of constraints $A = \{(\Omega, \mathcal{P}(\Omega), \mathbf{X}, Pr) : \textit{conditions}\}$. The 'independence' requirement imposes the equality $Pr(\{\omega : X_i(\omega) = v_i\} \cap \{\omega : X_j(\omega) = v_j\}) = Pr(\{\omega : X_i(\omega) = v_i\}) \cdot Pr(\{\omega : X_j(\omega) = v_j\})$ on all values v_i and v_j of all random variables X_i and X_j in $\mathbf{X}$. Given a value v The condition of identical distribution requires $Pr(\{\omega : X_i(\omega) = v\})$ to be the same number for all random variables X_i.

De Finetti's representation theorem shows that every distribution that is part of an IID model has a unique corresponding Carnapian inductive method

[10] See Zabell [24, p.291].

that satisfies probabilism and constant exchangeability, and vice versa.[11]

Thanks to this correspondence, statisticians' choices to use IID models can be justified or criticised according to whether constant exchangeability and probabilism represent reasonable inductive assumptions in the relevant scientific situation. In circumstances where available information makes it prudent to distinguish between individuals, constant exchangeability is not a reasonable inductive assumption and therefore IID models should not be used, according to the Carnapian account.

This recommendation seems to have been adopted in principle by the statistical community. For example, Gelman et al. write:

> The usual starting point of a statistical analysis is the (often tacit) assumption that the n values y_i [in this context the indices stand for states of a statistical model, which are analogous to constants of a logical language] may be regarded as *exchangeable*... A nonexchangeable model would be appropriate if information relevant to the outcome were conveyed in the unit indexes... Generally, it is useful and appropriate to model data from an exchangeable distribution as independent and identically distributed...
> Gelman et al. [9, p.6, round parenthesis and italics original, square parenthesis added]

The Carnapian account gives a plausible interpretation of what is meant by the condition that states "may be regarded as exchangeable": it means that constant exchangeability and probabilism explicate appropriate inductive assumptions.

4.1.2. Dirichlet distributions and Johnson's sufficientness postulate

Every IID model whose distribution is a member of the Dirichlet family corresponds to a Carnapian inductive method that satisfies, in addition to probabilism and constant exchangeability, the adequacy condition 'Johnson's sufficientness postulate'[12].

The same reasoning that favours using IID models when constant exchangeability is justified extends to this more specific case: statisticians following the Carnapian account can therefore adopt IID models with Dirichlet distributions on a principled basis. The scientific situation they face must render appropriate the inductive assumption that the satisfaction ratio of the values of the vari-

[11] See Jeffrey [12] and Carnap [7, p.217] for presentations of De Finetti's representation theorem and discussions of how it relates to Carnapian inductive logic.

[12] This relationship was demonstrated in principle by Johnson [13] in the 1930s. Kemeny [14, § 4] later used Johnson's sufficientness postulate, probabilism and constant exchangeability to characterise the so-called 'continuum of inductive methods', but did not explicitly connect these inductive methods with IID models with Dirichlet distributions. Zabell [23] makes this connection (see equation 2.14), as well as presenting a more rigorous and general version of Johnson's proof.

ables in some samples, together with the sample sizes, should be the only factors relevant to predictions of future values of the variables.

According to Zabell[13], future work may produce more correspondences of this kind, allowing statisticians to clarify the assumptions underlying other choices of models. Such research can only add to the Carnapian account's technical fruitfulness.

4.2. Agreement with Gelman and Shalizi's arguments

The philosophy behind the Carnapian account—Carnap's philosophy of inductive logic—is in remarkable harmony with Gelman and Shalizi's view about the practice of statistics. On the other hand there are several fundamental tensions between Gelman and Shalizi's position and Karl Popper's philosophy of science. These parallels and contrasting tensions demonstrate that the Carnapian account of statistical model-choice has the following advantages over the falsificationist account.

Fist, to the extent that Gelman and Shalizi's claims are correct, and shared by other working statisticians, the parallels show that statisticians can adopt the Carnapian account without committing to unfamiliar or implausible philosohpical positions. They therefore constitute evidence of the Carnapian account's feasibility.

Secondly, the parallels show that the Carnapian account of statistical model-choice can be seen as part of a more general underlying philosophy of science. This kind of well-connectedness is advantageous as it allows similarities between foundational problems in statistics and other areas to be more easily identified and exploited. On the other hand, since the falsificationist account cannot accommodate Gelman and Shalizi's claims at the same time as Popper's philosophy, it must either be philosophically un-connected or else inconsistent with its authors' views.

4.2.1. Parallels with Carnap's philosophy

Just as Gelman and Shalizi argue that, in practice, statistical models can have other functions than representing knowledge, Carnap argued that systems of inductive logic need not represent beliefs in order to be useful explicata. Carnap's expression of this point of view is very similar to Gelman and Shalizi's:

> The adoption of an inductive method is neither an expression of belief nor an act of faith, though either or both may come in as motivating factors. An inductive method is rather an instrument for the task of constructing a picture of the world on the basis of observational data...
>
> Carnap [3, §18]
>
> [A choice of inductive method] will take into consideration ... the truth-frequency of predictions and the error of estimates; further,

[13] See Zabell [24, p.292]

> the economy in use, measured by the simplicity of the calculations required; maybe also aesthetic features, like the logical elegance of the definitions and rules involved.
>
> Carnap [4, p.55]
>
> ...'the model', for a Bayesian, is the combination of the prior distribution and the likelihood, each of which represents some compromise among scientific knowledge, mathematical convenience and computational tractability. ...we do not have to worry about making our prior distributions match our subjective beliefs...
>
> Gelman and Shalizi [10, p.19-20]

Similarly, Carnap would have been at ease with Gelman and Shalizi's insistence that the way in which resemblance between models and data sources is measured should depend on the "respects that matter to us", that some degree of non-resemblance should be tolerated and that the ultimate criterion for selection should be whether or not "reasoning based on the model will be reliable" in the future.

Carnap thought that inductive logic was essentially a tool for formalising scientific assumptions. From this pragmatic point of view it is only natural that the way systems of inductive logic, and analogously statistical models, are evaluated should depend on the priorities of the investigator. He would also have seen nothing problematic about failing to reject statistical models that are not completely satisfactory: this is exactly his view of a system of inductive logic that he advocated at one point:

> It will not be claimed that $c^\star$ [the only confirmation function allowed by the system of inductive logic that Carnap was advocating] is a perfectly adequate explicatum...For the time being it would be sufficient that $c^\star$ be a better explicatum than the previous methods.
>
> Carnap [3, p.563, square parentheses added]

Finally, Carnap made philosophical arguments that are analogous to Gelman and Shalizi's criticism of the received view. Just as Gelman and Shalizi see no problem in leaving the Bayesian inferential framework in order to evaluate statistical models, and therefore reject **RV**, Carnap rejected the analogous stipulation that choices between systems of inductive logic should depend only on inductive-logically represented factors. Such external questions, he thought, ought to to be answered using testing and the experience of specialists rather than general philosophical proscriptions:

> The acceptance or rejection of abstract linguistic forms, just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. To decree dogmatic prohibitions of certain linguistic forms instead of testing them by their success or failure in practical use, is worse than futile...Let us grant to those who work

> in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function.
>
> Carnap [2, §5]

4.2.2. Problems viewing Gelman and Shalizi as Popperians

In contrast to their natural fit with Carnap's philosophy, Gelman and Shalizi's arguments are hard to square with the philosophy of Karl Popper.

Contrary to Gelman and Shalizi's view that measures of resemblance between models and data sources should take into account the investigation's priorities, Karl Popper argued that disagreement between scientific systems and empirical facts should be investigated in an objective way that does not depend on what matters to scientists. The nature of Popper's view is clear from this passage, where he criticises a proposal by Reichenbach to define statistical hypotheses' probability as the relative frequency with which they have previously been instantiated:

> ...the suggested definition would make the probability of a hypothesis hopelessly subjective: the probability of a hypothesis would depend upon the training and skill of the experimenter rather than upon objectively reproducible and testable results.
>
> Popper [16, p.256]

Popper also thought that the conditions under which a system should be rejected were sharply defined: he claimed that rejection should occur whenever a reproducible effect that is inconsistent with the system is discovered[14]. This view seems at odds with Gelman and Shalizi's view that some kinds of non-resemblance between models and data sources should be tolerated.

Gelman and Shalizi seem to acknowledge this divergence from Popper's views on the question of model-checking, writing that "Popper's specific ideas about testing require, at the least, substantial modification"[15]. However, there are further points of tension between their position and Popper's.

Popper saw scientific research as primarily concerned with attempting to demonstrate that theories are false: he saw activities that do not assist this process as not strictly scientific. It seems difficult, following such an approach, not to construe the adoption and rejection of statistical models as expressions of belief and disbelief, or to find conscionable Gelman and Shalizi's claim that desiderata that have little to do with truth and falsity, such as convenience or tradition, can be important in statistical research.

Finally, Karl Popper was an anti-inductivist: he thought that the adoption of scientific theories should never depend on judgements about the accuracy of

[14] See Popper [16, p.56].
[15] SeeGelman and Shalizi [10, p.28].

their future predictions based on past observations. Popper believed that such inductive judgements were unscientific:

> Induction, i.e. inference based on many observations, is a myth. It is neither a psychological fact, nor a fact of ordinary life, nor one of scientific procedure.
> Popper [17, p.53]

Gelman and Shalizi, in contrast, argue that judgements, based on past observations, about the future reliability of reasoning based on statistical models should play an important role in statistical investigations. According to them, such judgements are required in order to determine how to measure resemblance between models and data sources and how much non-resemblance can tolerated without rejection. Gelman and Shalizi therefore seem to be inductivists according to Popper's sense of the word.

Contrary to Gelman and Shalizi's assessment, their claims about the practice of statistics seems to be more than superficially incompatible with Popper's philosophy of science. They disagree with Popper on the fundamental question of why theories should be accepted and rejected and that of whether science should be inductive.

5. Allaying qualms about Carnapian inductive logic

Despite its advantages over the falsificationist account, statisticians might be hesitant to adopt the Carnapian account due to suspicions about its underlying philosophy. In this section I address several such concerns.

5.1. The problem of induction

It might be thought that the Carnapian account is particularly vulnerable to the worries about the rationality of induction raised by Hume, since it has an 'inductive' underlying philosophy. An anti-inductive philosophy of statistics, according to which there is no need to reason inductively, might seem preferable.

I think that such caution about induction is unnecessary, as the Carnapian account suffers no more from the problem of induction than any anti-inductive alternative. systems of inductive logic are intended merely to formalise already-existing inductive assumptions for the sake of practical convenience rather than to justify them. While systems of inductive logic may be just as difficult to justify as the everyday inductive assumptions that they explicate and Hume problematised, no additional difficulty arises from the formalisation.

On the other hand, anti-inductive philosophies must address the 'practical problem of induction', as set out in Salmon [20]. This is the problem of finding rational but non-inductive grounds to trust some un-falsified systems more than others when making practical decisions. It is hard to deny that some un-refuted theories are more trustworthy than others: the absurdity of the opposite position is clear from Worrall's example of a person who decides to make decisions based on the un-falsified theory that attempting to float down from the Eiffel tower will soon become safer than taking the lift[16].

However, just as in the case of the original problem of justifying induction, it is difficult to say what kind of reasoning could provide the required rational grounds, as in this context deductive reasoning cannot discriminate between un-falsified theories, while inductive reasoning has been ruled out by stipulation. Solving this problem seems to be just as difficult as solving the original problem of induction.

5.2. Excessive ambition

It has been suggested that Carnap sought to discover a single system of inductive logic that explains all of scientific reasoning. Putnam, for example, attributes to Carnap the view that

> . . . something like a formal method ('inductive logic') underlies empirical science, and continued work might result in an explicit statement of this method. . .
>
> Putnam [18, p.189, parentheses and quotation marks original]

Statisticians might justifiably be wary of such an ambitious philosophy: it is not clear that there even is a single method underlying all of empirical science, let alone one that can feasibly be formalised.

However, Carnap's ambitions were not so lofty. As shown by the following quotation, Carnap did not think it was feasible to explicate all of scientific reasoning using inductive logic:

> . . . there are many situations in science which by their complexity make the application of inductive logic practically impossible. For instance, we cannot expect to apply inductive logic to Einstein's general system of relativity. . .
>
> Carnap [3, §49]

Carnap explains the true aims of his research at Carnap [3, §49]: he wanted inductive logic's scientific contribution, when it was fully developed, to consist in providing "systematic unity" to mathematical statistics, as well as "a clarity and exactness of its basic concepts" by expressing various statistical methods and concepts using inductive logic, analogously to the way that Russell and Whitehead attempted to formulate mathematical concepts using deductive logic. While this goal is certainly ambitious, it is at least potentially feasible, in contrast to the incorrectly attributed goal of formalising all of scientific reasoning.

[16] See Worrall [22]

5.3. Apriorism

Finally, a sceptical statistician might believe that Carnap wanted to find logical or a-priori reasons for choosing between systems of inductive logic. This 'apriorist' view might seem discordant with the conduct of practising statisticians, who typically use non-logical, factual information to build and test statistical models.

However, such scepticism would be based on a misconception about the kind of 'logicality' that Carnap had in mind. Carnap explains at Carnap [3, §10-12] that he wanted systems of inductive logic to be 'logical' only in the weak sense that they mention only formally well-defined concepts. This requirement relates only to the way in which systems of inductive logic are specified, serving to ensure that the process of explication results in formalised inductive assumptions, rather than just differently stated informal ones.

Carnap was quite happy, on the other hand, for the reasons for which systems of inductive logic are chosen not to be logical or a-priori in any sense. This can be seen from this passage from the appendix to *Logical Foundations of Probability*:

> The system of inductive logic here proposed. . . is intended as a reconstruction. . . of inductive thinking as customarily applied in everyday life and in science.
>
> Carnap [3, p.576]

Clearly the question of which systems of inductive logic reconstruct inductive thinking is not entirely logical; nor can it be answered using exclusively a-priori reasoning. Some a-posteriori investigation into the nature of thinking as it occurs in everyday life and science needs to take place. Thus Carnap's aim cannot have been to find purely logical or a-priori grounds for choosing between systems of inductive logic.

This reading of Carnap is somewhat controversial. It is widely believed that the early Carnap was an apriorist, but that he abandoned this position in his later work. While I believe that quotations like the one above undermine this view, showing that neither early nor late Carnap were apriorists, the sceptical statistician can safely disregard this exegetical debate by taking the late Carnap's writing to constitute the philosophical background for the Carnapian account.

6. Conclusion

In summary, the Carnapian account of how statisticians ought to choose statistical models seems superior to the falsificationist account found in Gelman and Shalizi [10]. It is technically fruitful, potentially allowing more articulate expression of the reasons behind the selection of particular models. Clearly stated modelling assumptions can only improve statistical research. In addition, unlike the falsificationist account, the Carnapian account has a viable underlying philosophy that is in tune with Gelman and Shalizi's claims about the practice of statistics. Since the Carnapian account has these advantages, I conclude that

Gelman and Shalizi's argument would be improved by substituting it for the falsificationist account.

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