Do we need an integrated Bayesian/likelihood inference?

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Abstract. Murray Aitkin’s recent book, *Statistical Inference*, presents an approach to statistical hypothesis testing based on comparisons of posterior distributions of likelihoods under competing models. The author develops and illustrates his method using some simple examples of inference from iid data and two-way tests of independence. We analyze in this note some consequences of the inferential paradigm adopted therein, discussing why the approach is incompatible with a Bayesian perspective and why we do not find it useful in our applied work.

Keywords: Foundations, likelihood, Bayesian, Bayes factor, model choice, testing of hypotheses, improper priors, coherence.

1 Introduction

Following a long research program on the topic of integrated evidence, Murray Aitkin has now published a book entitled *Statistical Inference*. The book, subtitled *An Integrated Bayesian/Likelihood Approach*, proposes handling statistical hypothesis testing and model selection via comparisons of posterior distributions of likelihood functions under the competing models or via the posterior distribution of the likelihood ratios corresponding to those models. Instead of comparing Bayes factors or performing posterior predictive checks (comparing observed data to posterior replicated pseudo-datasets), *Statistical Inference* recommends a fusion between likelihood and Bayesian paradigms that allows for the perpetuation of noninformative priors in testing settings where standard Bayesian practice prohibits their usage [DeGroot 1973]. While we appreciate the effort made by Aitkin to place his theory within a Bayesian framework, we remain unconvinced of the said coherence, for reasons exposed in this note.

From our perspective, integrated Bayesian/likelihood inference cannot be Bayesian, and its attempt to salvage noninformative priors is doomed from the start. When noninformative priors give meaningless results for posterior model comparison, we see this as a sign that the model will not work for the problem at hand. Rather than trying to keep the offending model and define marginal posterior probabilities by fiat (whether by
Integrated Bayesian/likelihood

BIC, intrinsic Bayes factors, or posterior likelihoods), we prefer to follow the full logic of Bayesian inference and recognize that, when a model that gives inferences that cannot be believed, one must change either one's model or one's beliefs (or both). Bayesians, both subjective and objective, have long recognized the need for tuning, expanding, or otherwise altering a model in light of its predictions (see, for example, [Good, 1950] and [Jaynes, 2003], and we view improper marginal densities and undefined Bayes factors as an example of settings where previously-useful models are being extended beyond their applicability. To try to work around such problems without altering the prior distribution is, we believe, an abandonment of Bayesian principles and, more importantly, an abandoned opportunity for model improvement.

Unlike the author, who has felt the call to construct a new if tentatively unifying foundation for statistical inference, we have the luxury of feeling that we already live in a comfortable (even if not flawless) inferential house. Thus, we come to Aitkin’s book not with a perceived need to rebuild but rather with a view toward strengthening the potential shakiness of the pillars that support our own inferences. A key question when looking at *Statistical Inference* is therefore, apart from trying to understand the real Bayesian meaning of the approach: For the applied problems that interest us, does the proposed new approach achieve better performances than our existing methods? Our answer, to which we arrive after careful thought, is no.

Some of the problems we have studied include estimating public opinion within subgroups of the population; estimating the properties of electoral systems; population toxicokinetics; and assessing risks from home radon exposure. In these social and environmental science problems, we have not found the need to compute posterior probabilities of models or to perform the sorts of hypothesis tests described in Aitkin’s book. In addition, these are fields in which prior information is important, application areas in which we would prefer not to rely on data alone and not to use noninformative prior distributions as recommended by Aitkin. We can well believe that his methods might be useful in existing problems in which prior information is weak and where researchers are interested in comparing discrete hypotheses. Such problems do not arise in our own work, which is really all we can say regarding the potential applicability of the methods being discussed here.

As an evaluation of the ideas found in *Statistical Inference*, the criticisms found in this review are inherently limited. We do not claim that Aitkin’s approach is wrong (or biased, incoherent, inefficient, etc.), merely that it does not seem to apply to our problems and that it does not fit within our inferential methodology. Statistical methods do not, and most likely never will, form a seamless logical structure. It may thus very well be that the approach of comparing posterior distributions of likelihoods could be useful for some actual applications, and perhaps Aitkin’s book will inspire future researchers to demonstrate this.

*Statistical Inference* begins with a crisp review of frequentist, likelihood and Bayesian approaches to inference and then proceeds to the main event: the “integrated Bayes/likelihood approach” described in Chapter 2. Much of the remaining methodological material appears in Chapters 4 (“Unified analysis of finite populations”) and 7 (“Goodness
of fit and model diagnostics”). The remaining chapters apply Aitkin’s principles to various pocket-sized examples. In the present article, we first discuss on the basic ideas in Chapter 2, then consider the applicability of Aitkin’s ideas and examples to our applied research.

2 A small change in the paradigm

“This quite small change to standard Bayesian analysis allows a very general approach to a wide range of apparently different inference problems; a particular advantage of the approach is that it can use the same noninformative priors.”
Statistical Inference, page xiii

The “quite small change” advocated by Statistical Inference consists in considering the likelihood function as a generic function of the parameter $L(\theta, x)$ that can be considered a posteriori (that is, with a distribution induced by $\theta \sim \pi(\theta|x)$), hence allowing for (posterior) cdf, mean, variance and quantiles. In particular, the central tool for model fit is the “posterior cdf” of the likelihood,

$$F(v) = P^\pi(L(\theta, x) > z|x).$$

As argued by the author (Chapter 2, page 21), this “small change” in perspective has several appealing features:

- the approach is general and allows to resolve the difficulties with the Bayesian processing of point null hypotheses
- the approach allows for the use of generic noninformative (improper) priors
- the approach handles more naturally the “vexed question of model fit”
- the approach is “simple.”

We however dispute the magnitude of the change and show below why, in our opinion, this shift in paradigm constitutes a new branch of statistical inference, differing from Bayesian analysis on many points. Using priors and posteriors is no guarantee that inference is Bayesian (Seidenfeld, 1992). As noted above, we view Aitkin’s key departure from Bayesian principles to be his willingness to use models that make nonsensical predictions about quantities of interest. The practical advantage of the likelihood/Bayesian approach may be convenience (although the evidence presented in his book does not convince us; consider the labor required to work with the simple examples in this book, compared to the relative ease of handling much more complicated and interesting applied problems in Carlin and Louis [2008] using fully Bayesian inference), but the drawback is that the method pushes the user and the statistician away from progress in model building.

1 One might argue that, in practice, almost all Bayesians are subject to our criticism of “using models that make nonsensical predictions.” For example, Gelman et al. (2003) is full of noninformative priors.
We envision Bayesian data analysis as comprising three steps: (1) model building, (2) inference, (3) model checking. In particular, we view steps (2) and (3) as separate. Inference works well, with many exciting developments coming on line soon, handling complex models, leading to lots of applications, and a partial integration with classical approaches (as in the empirical Bayes work of Efron and Morris, 1975 or more recently the similarities between hierarchical Bayes and frequentist false discovery rates discussed by Efron, 2010), causal inference, machine learning, and other aims and methods of statistical inference.

Even in the face of all this progress on inference, model checking remains a bit of an anomaly, with the three leading Bayesian approaches being Bayes factors, posterior predictive checks, and comparisons of models based on prediction error. Unfortunately, as Aitkin points out, none of these model checking methods work completely smoothly: Bayes factors depend on aspects of a model that are untestable and are commonly assigned arbitrarily; posterior predictive checks are, in general, “conservative” in the sense of producing p-values whose probability distributions are concentrated near 0.5; and prediction error measures (which include cross-validation and the deviance information (DIC) criterion of Spiegelhalter et al., 2002) require the user to divide data into test and validation sets. The setting is even bleaker when trying to incorporate noninformative priors (Gelman et al., 2003; Robert, 2001) and new proposals are clearly of interest.

“A persistent criticism of the posterior likelihood approach (...) has been based on the claim that these approaches are ‘using the data twice,’ or are ‘violating temporal coherence.’ Statistical Inference, page 48

“Using the data twice” is not our main reservation about the method—because “using the data twice” is not a more clearly defined concept than “Occam’s razor.” One could just as well argue that the Bayes factor also uses the data twice, once in the numerator and once in the denominator. Instead, what we cannot fathom is how the “posterior” distribution of the likelihood function is justified from a Bayesian perspective. Statistical Inference stays away from decision-theory (as stated on page xiv) so there is no derivation based on a loss function or such. Our difficulty with the integrated likelihood idea is (a) that the likelihood function does not exist a priori and (b) that it requires a joint distribution to be properly defined in the case of model comparison. The case for (a) is arguable, as Aitkin would presumably contest that there exists a joint distribution on the likelihood, even though the case of an improper prior stands out (see below). We still see the notion of a posterior probability that the likelihood ratio is larger than 1 as meaningless. The case for (b) is more clear-cut in that when considering two models, a Bayesian analysis does need a joint distribution on the two sets of parameters to reach a decision, even though in the end only one set will be used.

Our criticism here, though, is not of noninformative priors in general but of nonsensical predictions about quantities of interest. In particular, noninformative priors can often (but not always!) give reasonable inferences about parameters \(\theta\) within a model, even while giving meaningless values for marginal likelihoods that are needed for Bayesian model comparison. It does when interest shifts from \(\Pr(\theta|x, H)\) to \(\Pr(H|x)\) that the Bayesian must set aside our noninformative \(p(\theta|H)\) and, perhaps reluctantly, set up an informative model.
As detailed below, this point is related with the introduction of pseudo-priors by Carlin and Chib (1995) who needed arbitrary defined distributions on the parameters that do not exist.

In the specific case of an improper prior, Aitkin’s approach cannot be validated in a probability setting for the reason that there is no joint probability on \((\theta, x)\). Obviously, one could always advance that the whole issue is irrelevant since improper priors do not stand within probability. However, improper priors do stand within the Bayesian framework, as demonstrated for instance by Hartigan (1983) and it is easy to give those priors a proper meaning. When the data are made of \(n\) iid observations \(x^n = (x_1, \ldots, x_n)\) from \(f_\theta\) and an improper prior \(\pi\) is used on \(\theta\), we can consider a training sample \((\text{Smith and Spiegelhalter} 1982)\) \(x^{(l)}\), with \((l) \subset \{1, \ldots, n\}\) such that

\[
\int f(x^{(l)}|\theta) d\pi(\theta) < \infty \quad (l \leq n).
\]

If we construct a probability distribution on \(\theta\) by

\[
\pi_{x^{(l)}}(\theta) \propto \pi(\theta)f(x^{(l)}|\theta),
\]

the posterior distribution associated with this distribution and the remainder of the sample \(x^{(-l)}\) is given by

\[
\pi_{x^{(l)}}(\theta|x^{(-l)}) \propto \pi(\theta)f(x^n|\theta), \quad x^{(-l)} = \{x_i, i \notin (l)\}.
\]

This distribution is independent from the choice of the training sample; it only depends on the likelihood of the whole data \(x^n\) and it therefore leads to a non ambiguous posterior distribution on \(\theta\). However, as is well known, this construction does not lead to produce a joint distribution on \((x^n, \theta)\), which would be required to give a meaning to Aitkin’s integrated likelihood. Therefore, his approach cannot cover the case of improper priors within a probabilistic framework and thus fails to solve the very difficulty with noninformative priors it aimed at solving.

3 Posterior probability on the posterior probabilities

“The p-value is equal to the posterior probability that the likelihood ratio, for null hypothesis to alternative, is greater than 1.” Statistical Inference, page 42

“The posterior probability is \(p\) that the posterior probability of \(H_0\) is greater than 0.5.” Statistical Inference, page 43

Those two equivalent statements show that it is difficult to give a Bayesian interpretation to Aitkin’s method, since the two “posterior probabilities” quoted above are incompatible. Indeed, a fundamental Bayesian property is that the posterior probability of an event related with the parameters of the model is not a random quantity but a
number. To consider the “posterior probability of the posterior probability” means we are exiting the Bayesian domain, both from logical and philosophical viewpoints.

Are we interested in taking this exit? Only if the new approach had practical advantages, a point to which we return later in this review.

In Chapter 2, Aitkin exposes his (foundational) reasons for choosing this new approach by integrated Bayes/likelihood. His criticism of Bayes factors is based on several points:

(i). “Have we really eliminated the uncertainty about the model parameters by integration? The integrated likelihood (...) is the expected value of the likelihood. But what of the prior variance of the likelihood?” (page 47).

(ii). “Any expectation with respect to the prior implies that the data has not yet been observed (...) So the “integrated likelihood” is the joint distribution of random variables drawn by a two-stage process. (...) The marginal distribution of these random variables is not the same as the distribution of Y (...) and does not bear on the question of the value of θ in that population” (page 47).

(iii). “We cannot use an improper prior to compute the integrated likelihood. This eliminate the usual improper noninformative priors widely used in posterior inference.” (page 47).

(iv). “Any parameters in the priors (...) will affect the value of the integrated likelihood and this effect does not disappear with increasing sample size” (page 47).

(v). “The Bayes factor is equal to the posterior mean of the likelihood ratio between the models” [meaning under the full model posterior] (page 48).

(vi). “The Bayes factor diverges as the prior becomes diffuse. (...) This property of the Bayes factor has been known since the Lindley/Bartlett paradox of 1957.”

The representation (i) of the “integrated” (or marginal) likelihood as an expectation under the prior is unassailable and is for instance used as a starting point for motivating the nested sampling method [Skilling 2006, Chopin and Robert 2010]. This does not imply that the extension to the variance or to any other moment has a similar meaning within the Bayesian paradigm. While the difficulty (iii) with improper priors is real, and while the impact of the prior modelling (iv) may have a lingering effect, the other points can be easily rejected on the ground that the posterior distribution of the likelihood is meaningless. This argument is anticipated by Aitkin who protests on pages 48-49 that, given point (v), the posterior distribution must be “meaningful,” since the posterior mean is “meaningful” (!), but the interpretation of the Bayes factor as a “posterior mean” is only an interpretation of an existing integral, it does not give any validation to the analysis. (It could as well be considered as a prior mean, despite depending on the observation x, as in the nested sampling perspective.) One could just as well take (ii) above as an argument against the integrated likelihood/Bayes perspective.
4 Products of posteriors

In the case of unrelated models to be compared, the fundamental argument against using posterior distributions of the likelihoods and of related terms is that the approach leads to parallel simulations from the posteriors under each model. The book recommends that models be compared via the distribution of the likelihood ratio values,

\[ \frac{L_i(\theta_i|x)}{L_k(\theta_k|x)}, \]

where the \( \theta_i \)'s and \( \theta_k \)'s are drawn from the respective posteriors. This choice is similar to Scott’s (2002) and to Congdon’s (2006) mistaken solutions analyzed in Robert and Marin (2008), in that MCMC runs are run for each model separately and the samples are gathered together to produce either the posterior expectation (in Scott’s case) or the posterior distribution (for the current paper) of

\[ \frac{\rho_i L(\theta_i|x)}{\sum_k \rho_k L(\theta_k|x)}, \]

which do not correspond to genuine Bayesian solutions (see Robert and Marin, 2008). Again, this is not as much because the dataset \( x \) is used repeatedly in this process (since reversible MCMC produces as well separate samples from the different posteriors) as the fundamental lack of a common joint distribution that is needed in the Bayesian framework. This means, e.g., that the integrated likelihood/Bayes technology is producing samples from the product of the posteriors (a product that clearly is not defined in a Bayesian framework) instead of using pseudo-priors as in Carlin and Chib (1995), i.e. of considering a joint posterior on \((\theta_1, \theta_2)\), which is \[ p_1 m_1(x) \pi_1(\theta_1|x) \pi_2(\theta_2) + p_2 m_2(x) \pi_2(\theta_2|x) \pi_1(\theta_1). \] (1)

This makes a difference in the outcome, as illustrated in Figure 1, which compares the distribution of the likelihood ratio under the true posterior and under the product of posteriors, when assessing the fit of a Poisson model against the fit of a binomial model with \( m = 5 \) trials, for the observation \( x = 3 \). The joint simulation produces a much more supportive argument in favor of the binomial model, when compared with the product of the posteriors. (Again, this is inherently the flaw found in the reasoning leading to the Scott, 2002 and Congdon, 2006 methods for approximating Bayes factors.)

A Bayesian version of Aitkin’s proposal can be constructed based on the following loss function that evaluates the estimation of the model index \( j \) based on the values of the parameters under both models and on the observation \( x \):

\[ L(\delta, (j, \theta_j, \theta_{-j})) = \mathbb{I}_{\delta=1} \mathbb{I}_{f_j(x|\theta_j) > f_1(x|\theta_1)} + \mathbb{I}_{\delta=2} \mathbb{I}_{f_j(x|\theta_j) < f_1(x|\theta_1)}. \] (2)

Here \( \delta = j \) means that model \( j \) is chosen, and \( f_j(.|\theta_j) \) denotes the likelihood under model \( j \). Under this loss, the Bayes solution is

\[ \delta^\pi(x) = \begin{cases} 1 & \text{if } \mathbb{P}^\pi [f_2(x|\theta_2) < f_1(x|\theta_1)|x] > 1/2 \\ 2 & \text{otherwise}, \end{cases} \]
which depends on the joint posterior distribution \( \pi_{M_1|X} \) on \((\theta_1, \theta_2)\), thus differs from Aitkin’s solution. We have

\[
\pi_{\pi_1}(\theta_1|x) \int_{\Theta_2} \pi_{\pi_2}(\theta_2|x, \theta_1) d\pi_2(\theta_2)
\]

where \( l^1 \) and \( l^2 \) denote the respective log-likelihoods and where the probabilities within the integrals are computed under \( \pi_1(\theta_1|x) \) and \( \pi_2(\theta_2|x) \), respectively. (Pseudo-priors as in Carlin and Chib, 1995 could be used instead of the true priors, a requirement when at least one of those priors is improper.)

An asymptotic evaluation of the above procedure is possible: consider a sample of size \( n, x^n \). If \( M_1 \) is the “true” model, then \( \pi(M_1|x^n) = 1 + o_p(1) \) and we have

\[
\pi_{\pi_1}(l^1(\theta_1) > l^2(\theta_2)|x^n, \theta_1) = \pi_{\pi_1}(-\chi^2_{p_1} > l^2(\theta_2) - l^2(\hat{\theta}_1)) + O_p(1/\sqrt{n})
\]

\[
= F_{p_1}(\chi^2_{p_1} - l^2(\theta_2)) + O_p(1/\sqrt{n}),
\]

with obvious notations for the corresponding log-likelihoods, \( p_1 \) the dimension of \( \Theta_1 \), \( \hat{\theta}_1 \) the maximum likelihood estimator of \( \theta_1 \), and \( \chi^2_{p_1} \) a chi-square random variable with \( p_1 \) degrees of freedom. Note also that, since \( l^2(\theta_2) \leq l^2(\hat{\theta}_2) \),

\[
l^1(\hat{\theta}_1) - l^2(\theta_2) \geq nKL(f_0, f_{\theta_2}) + O_p(\sqrt{n}),
\]

where \( KL(f, g) \) denotes the Kullback–Leibler divergence and \( \theta^*2 \) denotes the projection of the true model on \( M_2 : \theta^* = \arg\min_{\theta_2} KL(f_0, f_{\theta_2}) \), we have

\[
\pi_{\pi}(f(x^n|\theta_2) < f(x^n|\theta_1)|x^n) = 1 + o_p(1).
\]
By symmetry, the same asymptotic consistency occurs under model \( M_2 \). On the opposite, Aitkin’s approach leads (at least in regular models) to the approximation

\[
P[X^2_{p_2} - X^2_{p_1} > l^2(\hat{\theta}_2) - l^1(\hat{\theta}_1)],
\]

where the \( X^2_{p_2} \) and \( X^2_{p_1} \) random variables are independent, hence producing quite a different result that depends on the asymptotic behavior of the likelihood ratio. Note that for both approaches to be equivalent one would need a pseudo-prior for \( M_2 \) (resp. \( M_1 \) if \( M_2 \) were true) as tight around the maximum likelihood as the posterior \( \pi_2(\theta_2|x^n) \), which would be equivalent to some kind of empirical Bayes type of procedure.

Furthermore, in the case of embedded models, \( M_2 \) and \( M_1 \subset M_2 \), it happens that Aitkin’s approach can be given a probabilistic interpretation. To this effect, we write the parameter under \( M_1 \) as \((\theta_1, \psi_0)\), \( \psi_0 \) being a fixed known quantity, and under \( M_2 \) as \( \theta_2 = (\theta_1, \psi) \), so that comparing \( M_1 \) with \( M_2 \) corresponds to testing the null hypothesis \( \psi = \psi_0 \). Aitkin does not impose a positive prior probability on \( M_1 \), since his prior only bears on \( M_2 \) (in a spirit close to the Savage-Dickey representation, see [Marin and Robert, 2010]). His approach is therefore similar to the inversion of a confidence region into a testing procedure (or vice-versa). Under the model \( M_1 \subset M_2 \), denoting \( l(\theta, \psi) \) the log-likelihood of the bigger model,

\[
P^{\pi} [l(\theta_1, \psi_0) > l(\theta_1, \psi)|x^n] \approx P \left[ X^2_{p_2-p_1} > -l(\hat{\theta}_1(\psi_0), \psi_0) + l(\hat{\theta}_1, \hat{\psi}) \right]
\]

\[
\approx 1 - F_{p_2-p_1} [-l(\hat{\theta}_1(\psi_0), \psi_0) + l(\hat{\theta}_1, \hat{\psi})],
\]

which is the approximate \( p \)-value associated with the likelihood ratio test. Therefore, the aim of this approach seems to be, at least for embedded models where the Bernstein–von Mises theorem holds for the posterior distribution, to construct a Bayesian procedure reproducing the \( p \)-value associated with the likelihood ratio test. From a frequentist point of view it is of interest to see that the posterior probability of the likelihood ratio being greater than one is approximately a \( p \)-value, at least in cases when the Bernstein-von Mises theorem holds, in the case of embedded models and under proper priors. This \( p \)-value can then be given a finite-sample meaning (under the above restrictions), however it seems more interesting from a frequentist perspective than from a Bayesian one.\(^3\) From a Bayesian decision-theoretic viewpoint, this is even more dubious, since the loss function (2) is difficult to interpret and to justify.

\(^3\) See Chapter 7 of Gelman et al. (2003) for a fully Bayesian treatment of finite-sample inference.

"Without a specific alternative, the best we can do is to make posterior probability statements about \( \mu \) and transfer these to the posterior distribution of the likelihood ratio." Statistical Inference, page 42

"There cannot be strong evidence in favor of a point null hypothesis against a general alternative hypothesis." Statistical Inference, page 44

Once Statistical Inference has set the principle of using the posterior distribution of the likelihood ratio (or rather of the divergence difference since this is at least symmetric in both hypotheses), there is a whole range of output available including confidence
intervals on the difference, for checking whether or not they contain zero. This is appealing but (a) is not Bayesian for reasons exposed above, (b) is not parameterization invariant, (c) relies once again on an arbitrary confidence level.

Again, we prefer direct Bayesian approaches, recognizing that when Bayes factors are indeterminate, it is a sign that more work is needed in building a joint model.

5 Misrepresentations

We have focused in this review on Aitkin’s proposals rather than on his characterizations of other statistical methods. In a few places, however, we believe that his casual reading of the literature has led to some unfortunate confusion.

On page 22, Aitkin describes Bayesian posterior distributions as “formally a measure of personal uncertainty about the model parameter,” a statement that we believe holds generally only under a definition of “personal” that is so broad as to be meaningless. As we have discussed elsewhere (Gelman, 2008), Bayesian probabilities can be viewed as “subjective” or “personal” but this is not necessary. Or, to put it another way, if you want to label my posterior distribution as “personal” because it is based on my personal choice of prior distribution, you should also label inferences from the proportional hazards model as “personal” because it is based on the user’s choice of the parameterization of Cox (1972); you should also label any linear regression (classical or otherwise) as “personal” as based on the individual’s choice of predictors and assumptions of additivity, linearity, variance function, and error distribution; and so on for all but the very simplest models in existence.

In a nearly century-long tradition in statistics, any probability model is sharply divided into “likelihood” (which is considered to be objective and, in textbook presentations, is often simply given as part of the mathematical specification of the problem) and “prior” (a dangerously subjective entity to which the statistical researcher is encouraged to pour all of his or her pent-up skepticism). This may be a tradition but it has no logical basis. If writers such as Aitkin wish to consider their likelihoods as objective and consider their priors as subjective, that is their privilege. But we would prefer them to restrain themselves when characterizing the models as others. It would be polite to either tentatively accept the objectivity of others’ models or, contrariwise, to gallantly affirm the subjectivity of one’s own choices.

Aitkin also mischaracterizes hierarchical models, writing “It is important not to interpret the prior as in some sense a model for nature [italics in the original] that nature has used a random process to draw a parameter value from a higher distribution of parameter values . . .” On the contrary, that is exactly how we interpret the prior distribution in the ideal case. Admittedly, we do not generally approach this ideal (except in settings such as genetics where the population distribution of parameters has a clear sampling distribution), just as in practice the error terms in our regression models do not capture the true distribution of errors. Despite these imperfections, we believe that it can often be helpful to interpret the prior as a model for the parameter-
generation process and to improve this model where appropriate.

6 Contributions of the book

Statistical Inference points out several important facts that are individually known well (but perhaps not well enough!), but by putting them all in one place he foregrounds the difficulty or impossibility of putting all the different approaches to model checking in one place. We all know that the p-value is in no way the posterior probability of a null hypothesis being true; in addition, Bayes factors as generally practiced correspond to no actual probability model. Also, it is well-known that the so-called harmonic mean approach to calculating Bayes factors is inherently unstable, to the extent that in the situations where it does work, it works by implicitly integrating over a space different from that of its nominal model.

Yes, we all know these things, but as is often the case with scientific anomalies, they are associated with such a high level of discomfort that many researchers tend to forget the problems or try to finesse them. It is refreshing to see the anomalies laid out so clearly.

At some points, however, Aitkin disappoints. For example, at the end of Section 7.2, he writes: “In the remaining sections of this chapter, we first consider the posterior predictive p-value and point out difficulties with the posterior predictive distribution which closely parallel those of Bayes factors.” He follows up with a section entitled “The posterior predictive distribution,” which concludes with an example that he writes “should be a matter of serious concern [emphasis in original] to those using posterior predictive distributions for predictive probability statements.”

What is this example of serious concern? It is an imaginary problem in which he observes 1 success in 10 independent trials and then is asked to compute the probability of getting at most 2 successive in 20 more trials from the same process. Statistical Inference assumes a uniform prior distribution on the success probability and yields a predictive probability or 0.447, which, to him, “looks a vastly optimistic and unsound statement.” Here, we think Aitkin should take Bayes a bit more seriously. If you think this predictive probability is unsound, there should be some aspect of the prior distribution or the likelihood that is unsound as well. This is what Good ([1950] called “the device of imaginary results.” We suggest that, rather than abandoning highly effective methods based on predictive distributions, Aitkin should look more carefully at his predictive distributions and either alter his model to fit his intuitions, alter his intuitions to fit his model, or do a bit of both. This is the value of inferential coherence as an ideal.

7 Solving non-problems

Several of the examples in Statistical Inference represent solutions to problems that seem to us to be artificial or conventional tasks with no clear analogy to applied work.
“They are artificial and are expressed in terms of a survey of 100 individuals expressing support (Yes/No) for the president, before and after a presidential address (...) The question of interest is whether there has been a change in support between the surveys (...). We want to assess the evidence for the hypothesis of equality $H_1$ against the alternative hypothesis $H_2$ of a change.” Statistical Inference, page 147

Based on our experience in public opinion research, this is not a real question. Support for any political position is always changing. The real question is how much the support has changed, or perhaps how this change is distributed across the population.

A defender of Aitkin (and of classical hypothesis testing) might respond at this point that, yes, everybody knows that changes are never exactly zero and that we should take a more “grown-up” view of the null hypothesis, not that the change is zero but that it is nearly zero. Unfortunately, the metaphorical interpretation of hypothesis tests has problems similar to the theological doctrines of the Unitarian church. Once you have abandoned literal belief in the Bible, the question soon arises: why follow it at all? Similarly, once one recognizes the inappropriateness of the point null hypothesis, it makes more sense not to try to rehabilitate it or treat it as treasured metaphor but rather to attack our statistical problems directly, in this case by performing inference on the change in opinion in the population.

To be clear: we are not denying the value of hypothesis testing. In this example, we find it completely reasonable to ask whether observed changes are statistically significant, i.e. whether the data are consistent with a null hypothesis of zero change. What we do not find reasonable is the statement that “the question of interest is whether there has been a change in support.”

Figure 2: (a) Hypothetical graph of presidential approval with discrete jumps; (b) actual presidential approval series (for George W. Bush) showing movement at many different time scales. If the approval series looked like the graph on the left, then Aitkin’s “question of interest” of “whether there has been a change in support between the surveys” would be completely reasonable. In the context of actual public opinion data, the question does not make sense; instead, we prefer to think of presidential approval as a continuously-varying process.

All this is application-specific. Suppose public opinion was observed to really be flat, punctuated by occasional changes, as in the left graph in Figure 2. In that case, Aitkin’s...
question of "whether there has been a change" would be well-defined and appropriate, in that we could interpret the null hypothesis of no change as some minimal level of baseline variation.

Real public opinion, however, does not look like baseline noise plus jumps, but rather shows continuous movement on many time scales at once, as can be seen from the right graph in Figure 2 which shows actual presidential approval data. In this example, we do not see Aitkin’s question as at all reasonable. Any attempt to work with a null hypothesis of opinion stability will be inherently arbitrary. It would make much more sense to model opinion as a continuously-varying process.

The statistical problem here is not merely that the null hypothesis of zero change is nonsensical; it is that the null is in no sense a reasonable approximation to any interesting model. The sociological problem is that, from Savage (1954) onward, many Bayesians have felt the need to mimic the classical null-hypothesis testing framework, even where it makes no sense. Aitkin is unfortunately no exception, taking a straightforward statistical question—estimating a time trend in opinion—and re-expressing it as an abstracted hypothesis testing problem that pulls the analyst away from any interesting political questions.

8 Conclusion: Why did we write this review?

“The posterior has a non-integrable spike at zero. This is equivalent to assigning zero prior probability to these unobserved values.” Statistical Inference, page 98

A skeptical (or even not so skeptical) reader might at this point ask, Why did we bother to write a detailed review of a somewhat obscure statistical method that we do not even like? Our motivation surely was not to protect the world from a dangerous idea; if anything, we suspect our review will interest some readers who otherwise would not have heard about the approach (as previously illustrated by Robert, 2010).

In 1970, a book such as Statistical Inference could have had a large influence in statistics. As Aitkin notes in his preface, there was a resurgence of interest in the foundations of statistics around that time, with Lindley, Dempster, Barnard, and others writing about the intersections between classical and Bayesian inference (going beyond the long-understood results of asymptotic equivalence) and researchers such as Akaike and Mallows beginning to integrate model-based and predictive approaches to inference. A glance at the influential text of Cox and Hinkley (1974) reveals that theoretical statistics at that time was focused on inference from independent data from specified sampling distributions (possibly after discarding information, as in rank-based tests), and “likelihood” was central to all these discussions.

Forty years on, a book on likelihood inference is more of a niche item. Partly this is simply part of the growth of the field—with the proliferation of books, journals, and online publications, it is much more difficult for any single book to gain prominence. More than that, though, we think statistical theory has moved away from iid analysis, toward more complex, structured problems.
We respect Aitkin’s decision to focus on toy problems and datasets—it is a long tradition to understand foundations through simple examples, and we have done so ourselves on occasion—but we doubt that many statistical modelers will be inclined to abandon their existing methods that work so well on complex models and switch to an unproven approach that is motivated by its theoretical performance on simple cases.

That said, the foundational problems that Statistical Inference discusses are indeed important and they have not yet been resolved. As models get larger, the problem of “nuisance parameters” is revealed to be not a mere nuisance but rather a central fact in all methods of statistical inference. As noted above, Aitkin makes valuable points—known, but not well-enough known—about the difficulties of Bayes factors, pure likelihood, and other superficially attractive approaches to model comparison. We believe it is a natural continuation of this work to point out the problems of the integrated likelihood approach as well.

For now, we recommend model expansion, Bayes factors where reasonable, cross-validation, and predictive model checking based on graphics rather than p-values. We recognize that each of these approaches has loose ends. But, as practical idealists, we consider inferential challenges to be opportunities for model improvement rather than motivations for a new theory of noninformative priors.

9 References


