Assessing the uncertainty in statistical evidence with the possibility of model misspecification using a non-parametric bootstrap

Mark L. Taper\textsuperscript{1,3*}, Subhash R. Lele\textsuperscript{2*}, José M. Ponciano\textsuperscript{3}, Brian Dennis\textsuperscript{4}

\textsuperscript{1}Montana State University, Department of Ecology, Bozeman MT, 59717, USA.
\textsuperscript{2}University of Alberta, Department of Mathematical and Statistical Sciences, Edmonton, AB, T6G2G1, Canada.
\textsuperscript{3}University of Florida, Department of Biology, Gainesville FL, 32611-8525, USA.
\textsuperscript{4}University of Idaho, Department of Fish and Wildlife Sciences and Department of Statistical Science, Moscow ID 83844-1136, USA

* Correspondence:
Mark L. Taper
MarkLTaper@gmail.com

Keywords: evidential statistics, post-data inference, Schwarz’ information criterion, calibrated bootstrap, reliability, evidential confidence intervals

Abstract

Empirical evidence, e.g. observed likelihood ratio, is an estimator of the difference of the divergences between two competing models (or, model sets) and the true generating mechanism. It is unclear how to use such empirical evidence in scientific practice. Scientists usually want to know ‘how often would I get this level of evidence’. The answer to this question depends on the true generating
mechanism along with the models under consideration. In many situations, having observed the data, we can approximate the true generating mechanism non-parametrically by assuming far less structure than the parametric models being compared. We use a resampling method based on the non-parametric estimate of the true generating mechanism to estimate a confidence interval for the empirical evidence that is robust to model misspecification. Such a confidence interval tells us how variable the empirical evidence would be if the experiment (or observational study) were to be replicated. In our simulations, variability in empirical evidence appears to be substantial and hence using empirical evidence without a measure of uncertainty is, in fact, treacherous in practice. We divide the decision space in six different categories: Strong and secure (SS), Strong but insecure (SI), Weak and secure (WS), Weak and insecure (WI), Misleading and insecure (MI) and Misleading and secure (MS) based on the confidence interval. We illustrate the use of these categories for model selection in the context of regression. We show that instead of the three categories: Strong, Weak and Misleading, as suggested by Royall (1997), the six categories decision process leads to smaller errors in model selection and hence in scientific conclusions.

1 Introduction:

In one of the most influential scientific papers of the 20th century, Neyman and Pearson (1933) developed a decision procedure to choose between two competing hypotheses about how the data were generated. Any such decision, inevitably, leads to errors. In the Neyman-Pearson (NP) formulation, they considered one of the hypotheses as special and called it the null hypothesis. The decision procedure they developed was such that the probability of rejecting a null hypothesis, when it is true, was fixed a priori at a small value (e.g. to protect from unnecessarily rocking the status quo)
and the probability of rejecting the alternative hypothesis, when it is true, was minimized. These
errors are commonly called the Type I error and Type II error. As a consequence of fixing the Type I
error a priori, the cut-off point at which the null hypothesis is rejected increases as a function of the
sample size, thus making rejecting the null hypothesis harder and harder to reject. It also implies that
no matter how large a sample size is, the probability of making an error does not converge to 0. This
seems wrong scientifically; as we have more information, the probabilities of making wrong
decisions should go to zero.

Royall (1997), following Barnard (1949), Birnbaum (1962), Hacking (1965), Edwards (1972) and, of
course, Fisher (1922), quantified strength of evidence for comparing two simple models using the
likelihood ratio ($LR$).

We represent one observation as the random variable $X$ with $g(x)$ being the probability density
function representing the true, data-generating process and $f(x)$ being the probability density
function of an approximating model. The likelihood function under the generating process, for $n$
independent and identically distributed (iid) observations $x_1, x_2, ..., x_n$ is given as

$$L_g = g(x_1)g(x_2)\cdots g(x_n).$$

Whereas, the likelihood under an approximating model it is

$$L_f = f(x_1)f(x_2)\cdots f(x_n).$$

When there are multiple approximating models (e.g. $f_1(x)$ and $f_2(x)$),

we write the likelihoods as $L_1$ and $L_2$ (see Dennis et al. submitted). The $LR$ is

$$LR = \frac{L_1}{L_2} = \frac{f_1(x_1)f_1(x_2)\cdots f_1(x_n)}{f_2(x_1)f_2(x_2)\cdots f_2(x_n)}.$$ 

Notice that the NP formulation also uses the $LR$ to make decisions. However, there is a crucial
difference between the NP approach and Royall’s approach. In the NP approach (e.g. Casella and
Berger, 2002), the cut-off point, the value of the $LR$, beyond which the null hypothesis is rejected, is
determined based on the control of the error probabilities. On the contrary, Royall chooses the cut-off point, denoted by k, a priori. If the LR is larger than k, it is considered strong evidence for $f_2$; if it is smaller than $1/k$, it is considered strong evidence for $f_1$ and if it is between $1/k$ and k, it is considered weak evidence, indicating evidence is insufficient to distinguish the two models. This leads to possible errors. For example, suppose $f_1$ is the true model but we observe $LR < 1/k$, thus leading us to conclude that there is strong evidence for $f_2$. This is called strong misleading evidence. For a full description of different errors and their probabilities, see e.g. Dennis et al. (submitted). In general, we would like to have these error probabilities small. Royall (1997, 2000) calculates various error probabilities and their global upper bounds, asymptotic expressions etc. for this fixed cut-off point. In contrast to the NP approach, the error probabilities converge to 0 as the sample size increases (Royall 2000, his figure 2). However, similar to the NP approach, these error probabilities are properties of the procedure. According to Royall (1997), these error probabilities are useful pre-data and only for designing an experiment. Post-data, these error probabilities do not have any meaning (although see a somewhat contradictory statement in Royall 2004 page 124). One could speculate as to Royall’s reasons for this assertion, but Royall was never explicit in print. We have shown theoretically (Dennis et al. submitted) that when the true generating process is not in the set of models being compared (i.e. there is model misspecification) then the LR evidence does not contain all of information on the probability of errors—not even as a transformation (see Sellke et al., 2001). We are not the first to notice this lacuna, in fact, it was the failure of the evidential approach to include measures of confidence that led Birnbaum, an early (1962) strong supporter of the evidential concept to later strongly reject it (1970, 1972). The purpose of this paper is to demonstrate one practical method of inserting the concept of confidence into evidential analysis.

Lele (2004) considered a wider class of functions than LR that may quantify the strength of evidence. He called it the class of evidence functions. Broadly speaking, evidence functions are to the LR as
generalized likelihood ratio tests are to likelihood ratio tests. While the $LR$ is constrained to representing evidence comparing simple models (i.e. models with pre-specified parameters), evidence functions can compare models where parameters need to be estimated and even for more general composite hypotheses. Evidence functions are monotonic functions of the difference between data based estimates of the statistical divergences between each of two models and the true generating process. Evidence functions are required to meet a number of desiderata designed to strengthen their inferential properties. Lists of these desiderata can be found in Taper and Ponciano (2016), Jerde et al. (submitted), Taper and Lele (2011) and Lele (2004). The $LR$ and consistent information criteria are particular cases of evidence functions where the statistical divergence is the Kullback-Leibler divergence (see Taper and Ponciano (2016) and Dennis et al. submitted).

Throughout the above discussion and derivation of different properties of evidence functions, it is assumed that the true data generating process is in the model set. However, it is well known that all models are only an approximation to the true data generating mechanism. Dennis et al. (submitted) explore the effects on the various error probabilities when the true data generating mechanism is outside the model set. They call it an evidential approach under model mis-specification.

Of course, it is common practice in applied fields to test for model misspecification (e.g. Cook and Weisberg 1982 for regression diagnostics). An alternative approach is to make inferences that are robust to model mis-specification. It is obvious that given the data, not only one can compute the $LR$ but also obtain a non-parametric estimate of the data generating mechanism. The non-parametric estimator of the true data generating mechanism is still only an approximation to the true data generating mechanism because it still depends on some assumptions, but it is just a little bit more flexible than fully parametric models in the model set. Moreover, as the sample size increases, the model space for the non-parametric estimator also widens, making it a good proxy for the true data generating mechanism. An earlier paper by Taper and Lele (2011) made a suggestion of looking at
the empirical evidence, the observed likelihood ratio, as an estimator of the true difference between the KL divergences between the two models and the data generating mechanism. They, then, suggest using non-parametric bootstrap to obtain confidence intervals for the strength of evidence. Our goal in this paper is to use such non-parametric estimator of the true data generating mechanism to make evidential model selection decisions that are robust against model mis-specification.

In the next section, we outline the bootstrap methodology to obtain a confidence interval for the strength of evidence. More importantly, we show how to interpret and use this confidence interval for making model selection decisions. In section 3, we present the simulation set up that we use to illustrate the ideas about utilizing the confidence intervals. In section 4, we discuss the results of the simulation study. Section 5 presents some general comments along with a discussion of pre- vs post-data uncertainty quantification in the context of bootstrap confidence intervals for the strength of evidence.

2    Confidence interval for the true strength of evidence

2.1    Defining the “true strength of evidence”

Quantification of the strength of evidence for one hypothesis vis-à-vis an alternative is an inferential statement. An inferential statement becomes a “statistical” inferential statement only when one quantifies uncertainty associated with it (Cox 1958, Lele submitted). There are different ways to quantify uncertainty. One based on aleatory probability (also known as frequentist probability) as proposed by Neyman (1937) used in the Classical frequentist inference and the other based on credal probability (also known by the names subjective, inductive, and epistemic probability) as practiced by the Bayesian inference. In the following, we propose to use the aleatory probability in the
Classical frequentist sense: If the experiment is replicated, what could be the different values of the strength of evidence? This immediately raises the question of conditionality and which experiment is being replicated as is described in Lele (submitted). One could potentially use the Bayesian approach, select a prior distribution on the strength of evidence and obtain a credible interval. In that case, one has to answer the question: Which prior distribution? The choice is difficult and hence is termed ‘Sophie’s choice’ by Lele (submitted). We, as scientists, are more comfortable in answering the question which experiment could potentially be replicated by the skeptics and hence we use the aleatory probability in the following.

Let us consider a simple situation of an experiment with independent, identically distributed random variables. Suppose there are two competing models that purportedly generated the data, with a potential for neither of them being true. Given a sample of size $n$, we can quantify the strength of evidence for one model vis-à-vis the alternative by using the likelihood ratio. The log of this observed likelihood ratio is an estimate of $n$ times the difference of the Kullback-Leibler divergence of one model from the generating process and the Kullback-Leibler divergence of another from the generating process (see Taper, 2004; Dennis et al. submitted).

We denote by $\Delta K$ the difference of the KL divergences of $f_1(x)$ or $f_2(x)$, from $g(x)$:

$$\Delta K_{1,2} = K(g, f_2) - K(g, f_1).$$

We will call $n \cdot \Delta K_{1,2}$ the “true strength of evidence” for model 1 over model 2. We will refer to the model in the divergence being subtracted as the reference model and to the model in the divergence subtracted from as the alternative model. If the reference model is closer to the generating process than is the alternative model, the “true strength of evidence” will be positive. And, on the other hand, if the reference model is farther from the generating process than is the alternative model, the “true
strength of evidence” will be negative. We will sometimes refer to the “true strength of evidence” as the expected evidence.

2.2 Variability in observed evidence

One problem facing the use of observed evidence, i.e. evidence calculated from a realized data set, in scientific inference is that evidence is highly variable. Evidence is a sum, and its expectation and variance grow linearly with sample size. However, as the standard deviation of evidence only grows as root $n$, inferences do become more secure as sample sizes increase (Dennis et al. submitted). (see Figure 1: Convergence simulated distributions of evidence at sample sizes 20, 200, 2,000, 20,000).

Clearly as the sample size increases, one gets relatively closer and closer to the true strength of evidence, at least when the models have no unknown parameters. For composite hypothesis case, where we need to estimate the parameters in the models, we need to adjust this by using a consistent information criterion such as the SIC. For the discussion below, we will use the simpler case of no unknown parameters.

Because of its variability, the reliability of evidence does need to be quantified. Reliability, as a pre-data or global quantity $R^*$, is the probability that an experiment or survey will yield evidence that correctly identifies the best model. Post-data we will be interested in our confidence that the observed data has yielded evidence that correctly identifies the best model. We will term this post-data confidence as local reliability and designate it as $R_L$. This then is the question we want to address is: How uncertain is the estimate of the strength of evidence?

2.3 Non-parametric bootstrap confidence intervals for the true strength of evidence
A simple way to answer the uncertainty question is by using non-parametric bootstrap confidence interval for this statistics. In the i.i.d. situation, one can estimate the true data generating mechanism by using the empirical distribution function of the sampled values. By repeatedly sampling from the empirical distribution function, we can mimic repeated sampling from the true data generating mechanism. The basic procedure is well known in statistics (Efron and Tibshirani 1993).

However, as was noticed by Hall (1987) that percentile bootstrap confidence interval does not always have good coverage properties, particularly when the generating process is highly non-normal. To obtain better coverage properties, he developed what is known as a calibrated bootstrap (Efron and Tibshirani 1993, chapter 18). We describe these algorithms in more detail below. The basic idea is bootstrap the bootstrapped data set and based on the coverage properties of the lower level bootstraps adjust the limits of the upper level bootstrap.

Let us assume, for the time being, that we can obtain confidence interval for the true strength of evidence. It is a confidence interval because the true strength of evidence is a real number defined independently of the data. How should we interpret and use such confidence intervals?

2.4 Inferential use of confidence intervals for expected evidence

2.4.1 Desirable properties for confidence intervals for expected evidence

From an a priori design viewpoint an interval can either cover the expected evidence or it can miss it. If the interval fails to cover the expected evidence, it can either be entirely above the expectation (miss high) or entirely below it (miss low) (see Figure 2). Generally, the primary desirable property of a confidence interval is that the proportion of times the intervals contain their target true value (its coverage probability) is close to a priori nominal level. A secondary desirable property is that, given reasonable coverage, the intervals be as short as possible. Finally, it is often, but not always, it is considered desirable if intervals that miss the target value are distributed equally above and below it.
Evidence is one of the cases where an equal distribution of error is not desirable. In this context missing high is superior to missing low. Both types of intervals misrepresent the confidence one should have in the evidence, but the high miss is at least always indicating a correct decision, while a low miss could be supporting an incorrect decision. Of course, this is assuming that the expected evidence is positive, as in Figure 2, if the expected evidence were negative, the desirability of missing high and low would be reversed. Really, we mean that it is better to miss distally from 0 than to miss proximally to 0. However, in this simulation study the evidential comparisons are arranged so that reference model is always the better model, and the language of missing high and low seems less confusing.

<Figure 2 near here>

2.4.2 The use of bootstrap evidence intervals to aid post-data inference.

2.4.2.1 Defining evidential categories

As described in our introduction, Royall (1997) divided evidence into three categories: Strong, Weak and Misleading. These categories were based strictly on the observed evidence. Using both the observed evidence and the confidence interval as a measure of the evidential uncertainty, we divide the decision space in six different categories: Strong and secure (SS), Strong but insecure (SI), Weak and secure (WS), Weak and insecure (WI), Misleading and insecure (MI) and Misleading and secure (MS) based on the confidence interval. We define the evidence for one model over another given the data as strong and secure if the observed evidence is greater than a strong evidence threshold, and the proximal confidence bound is greater than a marginal evidence threshold. Strong and insecure evidence occurs when the observed evidence is greater than the strong evidence threshold and the proximal confidence bound is less than the marginal evidence threshold. Weak but secure evidence occurs when the observed evidence is less than the strong evidence threshold but the proximal
The Merits of Bootstrapping Evidence

confidence bound is greater than the marginal evidence threshold. The misleading and insecure and misleading and secure evidence categories are the same as the strong and insecure and strong and secure categories with the all-important difference that the model identification is not correct.

2.4.2.2 Interpreting evidence with intervals post-data

Figure 3 depicts some possible confidence intervals for the expected evidence. We will discuss how one might interpret each. We have adopted the convention that $\Delta SIC_{r,c} \geq 7$ is required for strong evidence for the reference model over an alternative model. $\Delta SIC_{r,c} = 4$ is the threshold for marginal evidence (see Jerde et al. submitted for discussion of the interpretation of thresholds).

In interval 1, the observed evidence, indicated by the filled oval, is strong and the lower bound for the bootstrapped evidence is above the marginal level. This evidence is designated strong and secure—the reference model is strongly indicated as being closer to the generating process than is the alternative and there is little chance that sampling variation would upset this identification. The research may reasonably conclude that no further work is needed regarding this particular contrast.

In interval 2, the observed evidence is strong, but the proximal bound is less than 4. So while the reference model is strongly indicated, it is plausible that this is due to sampling variation. Cautious interpretation is indicated, and if possible more data should be collected. Unlike NP tests, there is no penalty for looking at the data, so collecting more data is an appropriate response.

In interval 3 the evidence is both weak and insecure. The models are not differentiated by the data. The primary decision that the research should make is that more data are needed.

Interval 4 is a reflection of interval 2. The evidence is strong but insecure. In this case, the evidence is misleading, but a researcher with real data would not know this. The researcher can avoid errors by heading the insecure caveat both by cautious interpretation and by seeking more data.
Interval 5 is a researcher’s worst case. The evidence is strong, secure and misleading. The researcher should try to avoid this situation both by experimental design (large sample size) and by analytic design (higher strong and marginal evidence thresholds).

The bounds for interval 6 range from strong evidence for the reference model to strong evidence for the alternative model. This indicates that both models probably have fairly similar divergences from the generating process but are quite different from one another.

For interval 7, the evidence is weak, and the interval length is short. The most likely cause for this configuration is that the two models both have fairly similar distances to the generating process and are fairly similar to one another. In both cases 6 and 7 considerably more data will likely be needed to resolve the situation.

As sample sizes increases towards infinity, the asymptotic mathematics of Dennis et al. (submitted) applies and the probability of any interval other than strong secure (and correct) goes to 0. There is, of course, the pathological case where two models are exactly equidivergent from the true generating process. Were this curiosity ever to occur, then each model would be strongly and secure selected with a probability of 0.5. It is arguable that, even then, no error has occurred, as in each case a model closest to the generating process has been selected.

In the next section, we use simulations to show how confidence interval for the strength of evidence be used to conduct model selection for regression models.

3 Research Plan

3.1 Simple modeling scenario
To examine the uncertainty in evidential assessments and the potential impacts of model misspecification on that uncertainty we need a modeling test bed. This test bed should be simple so as not to distract from the important demonstrations with statistical pyrotechnics. To further enhance accessibility, the test bed should be broadly familiar.

We have chosen a multiple linear regression of a response variable, \( R \), against five predictor variables, \( (U, V, W, X, Y) \) and a quadratic effect \( Q \).

\[
R_i = \beta_0 + \beta_U U_i + \beta_V V_i + \beta_W W_i + \beta_X X_i + \beta_Y Y_i + \beta_Q Q_i + \epsilon_i
\]

Where \( \beta_0 \) is an intercept, the other \( \beta \) are the regression coefficients for the corresponding variables. Finally, \( \epsilon_i \) is an error term. \( U \) and \( V \) are spurious covariates, by which we mean covariates without causal connections to the response variable or to any of the other predictor variables—any correlations are purely chance. \( W \) is a weak covariate, \( X \) and \( Y \) are strong covariates, and \( Q \) is a quadratic curvature for \( Y \).

This, perhaps odd, notation for predictor variables allows us to designate models in a simple yet heuristic fashion. If a variable is in a model then it is indicated by including the variables letter in the name. A variables absence from a model is highlighted with an underscored blank space. \( UVWXYQ \) indicates the saturated model, \( \_\_WXYQ \) indicates the generating model, and \( \_\_WXY\_ \) indicates a model with neither of the spurious covariates or the curvature term.

3.2 Evidence functions

The general notation we use for evidence functions is \( Ev(d,m1,m2) \) this is to be read as the evidence for model \( m1 \) over model \( m2 \) based on data \( d \). Including data in the notation serves as a reminder that evidence always depends on specific data. The \( d \) may be suppressed for compactness if it is clear what data are being used.
The evidence function used in this study for comparing model $m_1$ to model $m_2$ is the difference of Schwarz’ information criterion values: $\Delta SIC(m_1, m_2)$ (Schwarz, 1978). This is our default evidence function for general scientific work because of its tendency not to make overfitting errors (Taper 2004). There may be good reasons to use other evidence functions for specific purposes (Ferguson et al. submitted, Hjort et al. submitted) The SIC evidence function (also known as the BIC) is defined as:

$$\Delta SIC(d, m_1, m_2) = 2\left(\log(L(m_2; d)) - \log(L(m_1; d))\right) + \log(n)(k_2 - k_1),$$

where $L(m, d)$ is the maximized likelihood function, $k_1$ and $k_2$ are the number of estimated parameters including error terms and other variance components. When we speak of evidence in general, or of potential comparisons, we use the $Ev(\cdot, \cdot)$ notation. When we report estimated values we use $\Delta SIC(\cdot, \cdot)$.

### 3.3 Evidential comparisons to be studied.

There are of a great number of potential comparisons that could be made given this regression framework. We focused on small that we expected to be informative.

#### 3.3.1 Evidence under correct specification

A model set without misspecification is one that include the generating process, at least as a model form. We still consider it a correct specification if the parameters of the generating model form need to be estimated. In this study the generating model had the form $\_WXYQ$.

The ability of evidence to detect spurious covariates is of interest for several reasons. First, the presence spurious covariates in models will confuse scientific interpretation. Second, the presence of spurious covariates will degrade the precision with which real parameters of interest can be estimated. Third, as discussed in Dennis et al. (submitted) the classical theory of loglikelihood ratios
(Neyman and Pearson, 1933; Wilks, 1938; Wald, 1943) indicates that evidence comparing the
generating model and a model augmented with spurious covariates should have an inverted shifted
chi-square distribution. Such skewed distributions may generate ambiguous evidential results. We
analyze evidence under correct specification for two models including spurious covariates:
\[ Ev(_WXYQ: U_WXYQ) \text{ and } Ev(_WXYQ: UVWXYQ). \]

The SIC with its strong complexity penalty is known to be prejudiced against detecting weak effects
at low sample size. It is of interest to compare the behavior of evidence for models with weak effect
with that for spurious effect. We consider the evidential comparison: \[ Ev(_WXYQ: ___XYQ). \] We
also study the evidential comparison \[ Ev(_WXYQ: _WXY \_). \] to see if there is anything anomalous
about the ability of evidence to detect curvature. Finally, for comparative purposes we include the
evidential detection of strong effects with comparison: \[ Ev(_WXYQ: _W_YQ). \]

### 3.3.2 Evidence under misspecification

We investigate the impact of misspecification on evidential assessment with problems of detecting
weak effects and of detecting curvature. For each problem evidence is evaluated using model sets
that contain several kinds of misspecification. Evidence for the weak effect is sought in comparing
models with the inclusion of two spurious covariates, \[ Ev(UVWXYQ: UV_XYQ); \] in models that are
missing the quadratic curvature element, \[ Ev(_WXY_: ___XYQ); \] and in models that both include the
spurious covariates and miss the curvature term, \[ Ev(UVWXY_: UV_XYQ). \] Similarly, for detecting
curvature we compare models with added spurious covariates, \[ Ev(UVWXYQ: UVWXY \_); \] with a
missing weak effect, \[ Ev(___XYQ: ___XY \_); \] and with both misspecifications, \[ Ev(UV_XYQ:
UV_XY \_). \]

### 3.4 Bootstrapping Methodology
As mentioned above, this is a difficult bootstrapping scenario. We expect both symmetric and highly skewed distributions of the evidence statistic. Depending on the kinds of misspecifications the distributions are expected to be chi-square, non-central chi-square, normal, or some mixture of these distributions (Dennis et al., submitted). To navigate this landscape we use the calibrated bootstrap. This is a flexible after the data adjustment for different distributions. The technique was first proposed by Peter Hall (1986, 1987) and is described Efron and Tibshirani (1993). The calibrated bootstrap is a double level bootstrap that uses coverage of the observed statistic by second level bootstrap to adjust quantiles of 1st level bootstrap to achieve desired coverage. When bootstrapping, rows of data, response and covariates are sampled as units.

Given an observed data set, \( \mathbf{x} \), where the boldface indicating a vector (or matrix), with and estimated statistic(s) of interest, \( \hat{\theta} \), the basic algorithm to obtain percentile bootstrap confidence intervals for any statistics is (See Efron and Tibshirani 1993 page170) as follows: 1) Randomly resample, with replacement the data set B times to create B bootstrap data sets, \( \mathbf{x}_i \), the subscript \( i \) indicates the \( i \)th of B data sets,. 2) For each data set calculate the statistic(s) of interest \( \hat{\theta}_i \). 3) Order the bootstrap estimates and use the confidence points, the 100\( \cdot\alpha \) and the 100\( (1-\alpha) \) percentiles, as the limits for a \( (1-2\alpha) \) per cent interval. In this study, our targeted statistic of interest, \( \hat{\theta} \), is \( \Delta SIC \). 

We implement the calibrated bootstrap as follows: 1) Randomly resample, with replacement the data set B\(_1\) times to create B\(_1\) first level bootstrap data sets, \( \mathbf{x}_i \). 2) For each data set calculate the statistic(s) of interest \( \hat{\theta}_i \). 3) For each first level bootstrap, randomly resample, with replacement, the first level data set, \( \mathbf{x}_i \), B\(_2\) times to create B\(_2\) second level bootstrap data sets, \( \mathbf{x}_{ij} \). 4)
The Merits of Bootstrapping Evidence

Calculate and order within each first level bootstrap, the estimated second level \( \hat{\theta} \). For a grid of \( \lambda \) confidence points (say \( \lambda \in \{0.01, 0.02, \ldots, 0.5\} \)) calculate \( 100 \cdot \lambda \) lower bounds for each of the B first level bootstraps. Select as \( \lambda^* \) the \( \lambda \) whose B lower bounds most closely cover the observed \( \hat{\theta}_0 \) at the desired \( 100 \cdot \alpha \) level. 5) Repeat step 4 to for upper bounds to find \( \lambda_{U}^* \) whose B lower bounds most closely cover the observed \( \hat{\theta}_0 \) at the desired \( 100 \cdot (1-\alpha) \) level. 6) Order the first level bootstrap estimates, \( \hat{\theta}_i \), and use the \( 100 \cdot \lambda_{L}^* \) and the \( 100 \cdot (1- \lambda_{U}^*) \) percentiles as the limits for a \( (1-2\alpha) \) per cent calibrated interval.

The use of the bootstrap gives the researcher access to information useful in making evidential inferences. The first level bootstrap yields an estimate of the sampling variability for the evidence, quantiles of the sampling distribution evidence, and an estimate of the local, or post data, reliability of the evidential assessment. The second level bootstrap makes possible a calibration of the quantiles for the sampling distribution of evidence and confidence bounds for the estimate of \( R^* \).

3.5 Testing the procedures

To ascertain whether our proposed protocols are useful we analyze the results of their application to simulated data. We generated data sets with 4 different sizes: Tiny, \( n=20 \), no reasonable scientist would consider this adequate for a regression with 6 covariates. Small, \( n=50 \), many rules of thumb indicate this is a reasonable sample size. Big, \( n=200 \), most scientists would be comfortable with this sample size. And, huge, \( n=2000 \), many scientists would consider this wasted effort. For each data size, 100 data sets were generated using the parameters in table 1. For each data set calibrated nonparametric bootstrapping is performed with bootstrap numbers \( B_1 = B_2 = 100 \).
The quality of the calibrated estimators can be determined by comparison with god's eye truth. The expected evidence and global reliability were estimated from 100,000 simulated data sets. Estimators are compared for MSE and bias.

We ask which estimator of evidence is best. We compare the observed evidence, the mean of 1st level bootstrapped evidence estimate, and the median of 1st level bootstrapped evidence estimates. We also briefly considered the 50th quantile of 1st level BCa bootstrap (Efron, 1987, Efron and Tibshrani, 1993) and 50th quantile of calibrated bootstrap. Neither estimator was pursued because initial work indicated that these last two estimators performed more poorly than the other three.

We investigate the adequacy of the bootstrap intervals of evidence. For the 1st level bootstrap percentiles and the calibrated bounds, we tabulate their coverage, interval length, and the proportion of times that the intervals fall entirely above and entirely below the expected evidences.

Three estimators of R* are compared. These are the observed RL (1st order bootstrap), the mean RL (2nd order bootstrap), and the median RL (2nd order bootstrap). We also look at the coverage of bootstrap bounds on R*.

The R code for the simulations can be found in the file SimulationSingleFile.R. The R code for analyzing the simulations is in the file PostSimulationAnalysis.R. Both of these programs can be found on github (https://github.com/jmponciano/mltaper-bootpaper). These programs can also be found in the supplementary material in the zipped file Rcode.zip. The raw summaries can be found in the supplementary figures S1-S108 and in the supplementary tables S1-S32. All analyses were conducted using R version 3.5.1.

4 Results Summary:

4.1 Best estimate of the expected evidence
The overall best estimator of the expected evidence is the observed evidence. In aggregate the differences between estimators are small, however when inspecting the RMSEs by evidential comparison a striking pattern emerges. For all data sizes tested, for evidential comparisons testing whether to add spurious covariates to the model, the observed evidence has a substantially lower RMSE than the other two estimators. For all other comparisons, the RMSEs are very similar across estimators.

4.2 Quality of evidence quantile estimates

Both 1st level and calibrated quantiles over cover for evidential comparisons not involving spurious covariates. For evidential comparisons involving spurious covariates 1st level quantiles over cover and calibrated intervals substantially under cover. However in these cases, misses for calibrated intervals are almost entirely high (see Figure 2.). Calibrated intervals have overall better coverage, and interval length is on average smaller for calibrated intervals. Again this superiority is due to behavior in the presence of spurious covariates. Calibrated interval length is many times shorter than 1st level interval length for evidence against spurious covariates. For evidence not involving spurious covariates, calibrated intervals were shorter than uncalibrated intervals for the tiny and small data set size, but greater for the big and huge sample sizes. Calibrated interval length is slightly larger than 1st level interval length

The best estimate of R* is 2nd level bootstrap median RL. This superior to the 1st level bootstrap estimate of RL and also to the mean 2nd level bootstrap RL in both RMSE and bias. This superiority increases with sample size. Again, the superiority is primarily due to performance with spurious covariates. It is important to note that RL is highly variable, but despite this variability the bootstrap quantile for RL has good coverage of R*.

5 Synthesis
Inspection of the evidential limits (see figures S1-S108 and tables S1-S4, S9-S12, S17-S20, and S25-S28) leads to a profound observation: The interval length surrounding misleading evidence tends to be greater than for correct evidence. This suggest a way to combine observed evidence with estimated bounds or error rates to make honest inferences.

We suggest a model identification protocol that in our testing makes almost no errors even at very low sample size. A standard evidential analysis divides outcomes into 3 categories, strong evidence for model 1, weak or inconclusive evidence, and strong evidence for model 2. We further subdivide strong evidence into the categories of secure strong evidence, and insecure strong evidence. If the evidence is strong and if the bounds for evidence do not overlap an evidential threshold the evidence is designated secure. Under secure strong evidence it is highly unlikely that the conclusion is in error due to sampling variability. Under insecure strong evidence, an error is more plausible. The fundamental mistake to avoid is accepting strong misleading evidence. (see Figure 5:) In our simulations using 90% intervals (5% tails) and threshold of marginal evidence ($|\Delta SIC| \geq 4$) secure misleading evidence almost never occurs.

6 Discussion

In this study secure misleading evidence only occurs twice both at the intermediate sample size of 200. No instance of secure misleading evidence occur at tiny, small or huge data sizes. This is an expression of Royall’s bump function (Royall, 2000; Dennis et al. submitted) under model misspecification. We find it remarkable that while there are no cases of secure misleading evidence at the tiny data size there are several cases of strong and secure correct evidence. Apparently, strong inferences on model form can be drawn from very small data sets—occasionally, and if the stars align (see Figure 1). Note that caution still needs to be used when interpreting small data sets. Effect size estimates are likely to be strongly biased.
We have seen that the bootstrap bounds on evidence can be very important in understanding the inferences that can be made with the data in hand. They can also be used to help a research decide if it will be profitable to continue collecting data. It is unlikely that under continued sampling the total evidence will fall outside of those bounds. If no evidence within those bounds, if secure, would change your conclusions then continued sampling is not likely to be profitable. This clear advice on the utility of collecting more data is not found in either classical NP tests nor fully in Royall’s evidential trichotomy.

Surprisingly, in this study, model misspecification has had only a minor effect on model selection. Dennis et al. (submitted) only indicates that model misspecification can lead to an increased probability of misleading evidence. More careful retrospective consideration of the geometry of model misspecification in regression leads us to believe that it is the inclusion of dependence relationships among predictor variables (often called collinearity) that will lead to an increase in misleading evidence. As Bollen (1989, p. 59) says: “Collinearity generally increases the standard errors of the coefficients of the collinear variables (other things equal). The increased standard errors means that we have greater uncertainty in the inferences that we make about the parameters.” We expect that this increased uncertainty will translate into increased uncertainty in model selection. In subsequent research will expand our analysis to include collinearity and confirm that our protocol also improves inferences in these cases.

In this era, there is a crisis of confidence in science. The reasons for this are manifold. But, a portion of this lack of confidence in science is due to problems in statistics. This portion is fixable. Classical statistics relies fundamentally on estimated error rates as its warrant to reliable scientific inference. When experiments and observations fail to be replicable at the rates implied by their reported
significance levels or test sizes, the foundations of scientific inference are literally\(^1\) broken. This is the replication crisis facing science today. We believe that the replication crisis itself, at least in part, stems from abuses of classical statistics induced by the deficiencies of classical statistics.

The work horse of modern ecological statistics is the Neyman-Pearson test. However this test is strongly lopsided. The Null hypothesis is either rejected or it is not, but if it is not rejected, this does not constitute evidence for the Null. Papers that fail to reject the Null essentially say nothing, as a consequence, reviewers and consequently journals tend to reject papers without “significant” results. This by itself creates a selection bias whereby “significant” results are over-represented in the literature. It also creates motivation for researchers to either consciously or unconsciously jigger their results to get them over the critical threshold further increasing the selection bias for speciously significant results. Second, there is a widespread underappreciation of the potential impact of model misspecification on the NP-test. Under model misspecification, the nominal error rate can underestimate the true error rate, and sometimes greatly (Dennis et al. submitted). This creates another source of bias towards speciously significant results.

The error structure of evidence is inherently better for the practice of science. First, you can have evidence for either of the two models compared. Second, having the category of weak evidence creates a legitimate means for researchers to discuss suggestive results without the need to overrate the strength of evidence implied. For example, in Jerde et al. (submitted) weak evidence is found for a temperature sensitivity in the scaling of metabolic rate with body mass. A Neyman-Pearson analysis would tend to suppress this result by saying that it was not a significant result. In contrast, the canonical recommendation in an evidential analysis is that such a model should be retained in the

\(^1\) Contrary to common usage, by “literally” we do mean literally and not figuratively.
researchers thinking. In fact, one of the authors of Jerde et al. now plans to pursue this temperature sensitivity as part of her dissertation research.

By breaking the evidential assessment into 6 cases as opposed to 3 we have created the opportunity for more honest and yet more nuanced inference. This division also maintains the separation of evidence and error rates as distinct but jointly informative inferential quantities.

7  Conflict of Interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

8  Author Contributions

MLT an SRL originally conceived of this project. All authors contributed to the design of the study. MLT wrote the simulation code. MLT wrote the first draft of the manuscript. All authors contributed to the revision of the manuscript and approved its final version.

9  Funding

This work was not funded from any source.

10  Acknowledgments
Figure 1: Convergence of the evidential comparison. $E_v(...) \ (\text{see section 3.1 for model definitions})$ as sample sizes increases. For each sample size 20 data sets are generated. Each data set is bootstrapped 1000 times, the evidence for the contrast calculated, and kernel density estimates of the distribution of the bootstrapped evidence plotted. The heavy dashed line buried in the plots is the distribution of evidence for 20,000 simulated data sets. Data sizes shown are 20, 200,
Comparison of the four panels demonstrates that while the mean and variance for evidence increases with sample size, the coefficient of variation decreases.
Figure 2:

Attributes of bootstrap evidence intervals from an *a priori* design viewpoint. An interval can either cover the expected evidence or it can miss it. If the interval fails to cover the expected evidence, it can either be entirely above the expectation (miss high) or entirely below it (miss low). In this simulation study the reference model is always the better model. In this context missing high is superior to missing low. Both types of intervals misrepresent the confidence one should have in the evidence, but the high miss is at least always indicating a correct decision, while a low miss could be supporting an incorrect decision.
Figure 3:

Possible types of evidence confidence intervals to aid post-data inference. We have adopted the convention that $\Delta SIC_{r,c} \geq 7$ is required for strong evidence for strong evidence for the reference model over a contrasting model. $\Delta SIC_{r,c} = 4$ is the threshold for marginal evidence. The dotted lines at 7 and -7 demark the strong evidence threshold. The short dashed lines at 4 and -4 indicate the marginal evidence threshold. The line at 0 indicates completely equivocal evidence. The dark ovals indicate the observed evidence. Bars show the confidence intervals for expected evidence. The labels of SS, SI, WS, WI, MI, and MS indicate strong and secure, strong and insecure, weak and secure, weak and insecure, misleading and insecure, and misleading and secure evidence respectively.
12 Tables

| Variable | $\beta_i$ | Definition        |
|----------|-----------|-------------------|
| Intercept| 1         |                   |
| $U$      | 0         | $\sim N(0,1)$    |
| $V$      | 0         | $\sim N(0,1)$    |
| $W$      | 0.25      | $\sim N(0,1)$    |
| $X$      | 0.5       | $\sim N(0,1)$    |
| $Y$      | 0.5       | $\sim N(0,1)$    |
| $Q$      | -0.15     | $Q_i = Y_i^2$     |
| $\varepsilon$ | 1   | $\sim N(0,1)$    |

Table 1: Predictor variables, coefficients, and definitions

13 References

Birnbaum, A. 1962. On Foundations of Statistical-Inference. Journal of the American Statistical Association 57:269-84.

Birnbaum, A. 1970. Statistical Methods in Scientific Inference. Nature 225:1033.

Birnbaum, A. 1972. More on Concepts of Statistical Evidence. Journal of the American Statistical Association 67:858-861.
The Merits of Bootstrapping Evidence

Bollen, K. A. 1989. Structural Equations with Latent Variables. Wiley, New York.

Casella, G., and R. L. Berger. 2002. Statistical Inference. 2nd edition. Cengage Learning.

Cook, R. D., and S. Weisberg. 1982. Residuals and Influence in Regression Chapman and Hall.

Edwards, A. W. F. 1972. Likelihood. Cambridge University Press, Cambridge.

Efron, B., and R. Tibshirani. 1993. An Introduction to the Bootstrap. Chapman and Hall, London, UK.

Fisher, R. A. 1922. On the mathematical foundations of theoretical statistics. Philosophical Transactions of the Royal Society of London, Series A 222:309-368.

Hacking, I. 1965. Logic of statistical inference. Cambridge University Press., Cambridge.

Hall, P. 1986. On the Bootstrap and Confidence-Intervals. Annals of Statistics 14:1431-1452.

Hall, P. 1987. On the Bootstrap and Likelihood-Based Confidence-Regions. Biometrika 74:481-493.

Lele, S. R. 2004. Evidence Functions and the Optimality of the Law of Likelihood.in M. L. Taper and S. R. Lele, editors. The Nature of Scientific Evidence: Statistical, Philosophical and Empirical Considerations. The University of Chicago Press, Chicago.

Neyman, J. Outline of a Theory of Statistical Estimation Based on the Classical Theory of Probability. Philosophical Transactions of the Royal Society of London. Series A, Mathematical and Physical Sciences 236:333-380.

Neyman, J., and E. S. Pearson. 1933. On the Problem of the Most Efficient Tests of Statistical Hypotheses. Philosophical Transactions of the Royal Society of London. Series A, 231:289-337.

Royall, R. M. 1997. Statistical Evidence: A likelihood paradigm. Chapman & Hall, London.

Royall, R. M. 2000. On the Probability of Observing Misleading Statistical Evidence. Journal of the American Statistical Association 95:760-780.
Royall, R. M. 2004. The Likelihood Paradigm for Statistical Evidence. Pages 119-152 in M. L. Taper and S. R. Lele, editors. The Nature of Scientific Evidence: Statistical, Philosophical and Empirical Considerations. The University of Chicago Press, Chicago.

Schwarz, G. 1978. Estimating the dimension of a model. Annals of Statistics 6:461-464.

Sellke, T., M. J. Bayarri, and J. O. Berger. 2001. Calibration of p values for testing precise null hypotheses. American Statistician 55:62-71.

Taper, M. L. 2004. Model identification from many candidates. Pages 448-524 in M. L. Taper and S. R. Lele, editors. The Nature of Scientific Evidence: Statistical, Philosophical and Empirical Considerations. The University of Chicago Press, Chicago.

Taper, M. L., and S. R. Lele. 2011. Evidence, Evidence Functions, and Error Probabilities. Pages 513-532 in P. S. Bandyopadhyay and M. R. Forster, editors. Philosophy of Statistics. Elsevier, Oxford.

Taper, M. L., and J. M. Ponciano. 2016. Evidential statistics as a statistical modern synthesis to support 21st century science. Population Ecology 58:9-29.

Wald, A. 1943. Tests of statistical hypothesis concerning several parameters when the number of observations is large. Trans Amer Math Soc 54:426–482.

Wilks, S. S. 1938. The large-sample distribution of the likelihood ratio for testing composite hypotheses. The Annals of Mathematical Statistics 9:60-62.