

Model Assessment Tools for a Model False World

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Abstract

A standard goal of model evaluation and selection is to find a model that approximates the truth well while at the same time is as parsimonious as possible. In this paper we emphasize the point of view that the models under consideration are almost always false, if viewed realistically, and so we should analyze model adequacy from that point of view. We investigate this issue in large samples by looking at a model credibility index, which is designed to serve as one-number summary measure of model adequacy. We define the index to be the maximum sample size at which samples from the model and those from the true data generating mechanism are nearly indistinguishable. We use standard notions from hypothesis testing to make this definition precise. We use data subsampling to estimate the index. We show that the definition leads us to some new ways of viewing models as flawed but useful. The concept is an extension of the work of Davies (1995).^{1 2}

1 Introduction

Our starting point is the famous quotation of G.E.P. Box:

All models are wrong, but some are useful (1976).

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In this article we will take as our initial premise that “All models are wrong”, and see where it leads us. A consequence of model falseness is that for every data generating mechanism, there exists a sample size at which the model failure will become obvious.

Our second premise is that there are occasions when one will want to use, in some fashion, a model that is clearly false, provided that it provides a parsimonious and powerful description of the generating mechanism. Here we wish to emphasize that we are interested in description, not prediction, as there is a smaller advantage to simplicity when the overarching goal is accurate prediction.

In order to explore this question, the key assumption of this paper will be that the sample size under which the data is collected, say n , is sufficiently large that many of the models under investigation are clearly false. This would seem to be a reasonable assumption in the modern data-mining environment. Just the same, we wish to measure the quality of their approximation to the true data generating mechanism to see which ones most economically capture its main features. Later in this paper we will use subsampling from the data as a means of replicating the true data generating mechanism.

It is important to our theme that we are seeking to measure attributes that are completely unrelated to the value of n that generated the data at hand. We emphasize this because the standard tools for model assessment are highly n -dependent. For example, hypothesis testing has played a prominent role in the assessment of the models since the development of Pearson’s chi-squared statistic. Unfortunately, it is based on the false premise that the model is correct, and so for a large enough sample size, we are doomed to reject any fixed model. That is, if we view these tests as answers to the question: “Is this model useful?”, then what we mean by usefulness is clearly related to not just the quality of the model, but also the size of the sample that was used in its assessment. So hypothesis testing does not meet our need directly.

In our approach, we use testing methodology but in an inverted fashion. We treat the null hypothesis as being false, and ask questions about the power of the test statistic as a function of its sample size. We define our new index, called the model credibility index, as the sample size needed to obtain a desirable power. Although the point of view is not new that the power of a test depends on the sample size. it is a novel idea to propose the sample size

as a model evaluation index.

Other standard risk analyses, the basis for AIC, Mallows's C_p , and other methods are n -dependent because the goal there is to assess the quality of prediction using the fitted model. These criteria for model selection depend not just on the model itself, but also on the quality of the parameter estimation, which in turn depends on n .

We hope that our new methods will be thought-provoking because they involve only standard tools of testing and risk assessment, so they could be readily understood (and constructed) by any statistician.

Just the same, we think that our work presents a challenge to the standard statistical train of thought. Statisticians are quite accustomed to taking the “model true” point of view. After all, we have a huge box of statistical tools that are based on the assumption. This can make it hard for statisticians to maintain consistently a “model false, but maybe useful” point of view.

For example, suppose we have a random sample X_1, X_2, \dots, X_n with distribution τ . In traditional model building much is made of the idea of consistency, in the sense of finding the true distribution τ based on the assumption it lies within some narrow set of models. However, this true distribution is very likely to be much too complex to be useful, especially if we consider the discretization, rounding, misrecording, and measurement errors incumbent in real data. (For example, see the discussion of Ghosh and Samanta, 2001, page 1140.) For the duration of this article, at least, we ask the reader to believe in model-falseness, and further believe that usefulness is not necessarily tied to consistency.

In the next subsection we give an informal introduction to our methodology. This will be followed by a more detailed look at the contents of the paper.

1.1 Introducing credibility indices

Davies (2002) gave the following definition:

A probability model P_θ is an adequate approximation for the data set (x_1, \dots, x_n) if “typical” samples $(X_1(\theta), \dots, X_n(\theta))$ of size n generated using P_θ “look like” the real data set (x_1, \dots, x_n) .

This is clearly an n -dependent assessment, but it captures what we consider an important aspect of a good model – that it is good at creating data similar to the observed data.

To illustrate our thinking, let us start with the most prominent statistical assumption, that the data is normally distributed. Surely we might believe that no data is exactly normal in distribution, but that it is often useful and plausible to assume so.

Berkson (1938) described the paradox that a goodness-of-fit test may become embarrassingly powerful whenever the data are extensive.

I believe that an observant statistician who has had any considerable experience with applying the chi-square test repeatedly will agree with my statement that, as a matter of observation, when the numbers in the data are quite large, the P 's tend to come out small. Having observed this, and on reflection, I make the following dogmatic statement, referring for illustration to the normal curve: "If the normal curve is fitted to a body of data representing any real observations whatever of quantities in the physical world, then if the number of observations is extremely large – for instance, on the order of 200,000 – the chi-square P will be small beyond any usual limit of significance."

If this be so, then we have something here that is apt to trouble the conscience of a reflective statistician using the chi-square test. For I suppose it would be agreed by statisticians that a large sample is always better than a small sample. If, then, we know in advance the P that will result from an application of a chi-square test to a large sample there would seem to be no use in doing it on a smaller one. But since the result of the former test is known, it is no test at all!

As a response, Hodges and Lehmann (1954) suggested that the difficulty could be avoided by making distinction between "statistical significance" and "practical significance" in the formulation of the problem. The idea was to construct a larger hypothesis H_1 of distributions about the null H_0 , representing distributions that are close enough to H_0 so that the difference is deemed not practically significant with the data at hand. If one let H_1 play the role

of the null hypothesis, then if the true distribution is an element of H_1 , then one might still wish to use the model H_0 . Liu and Lindsay (2009) expanded upon this idea, but still found difficulty in creating a reasonable set H_1 having a simple interpretation.

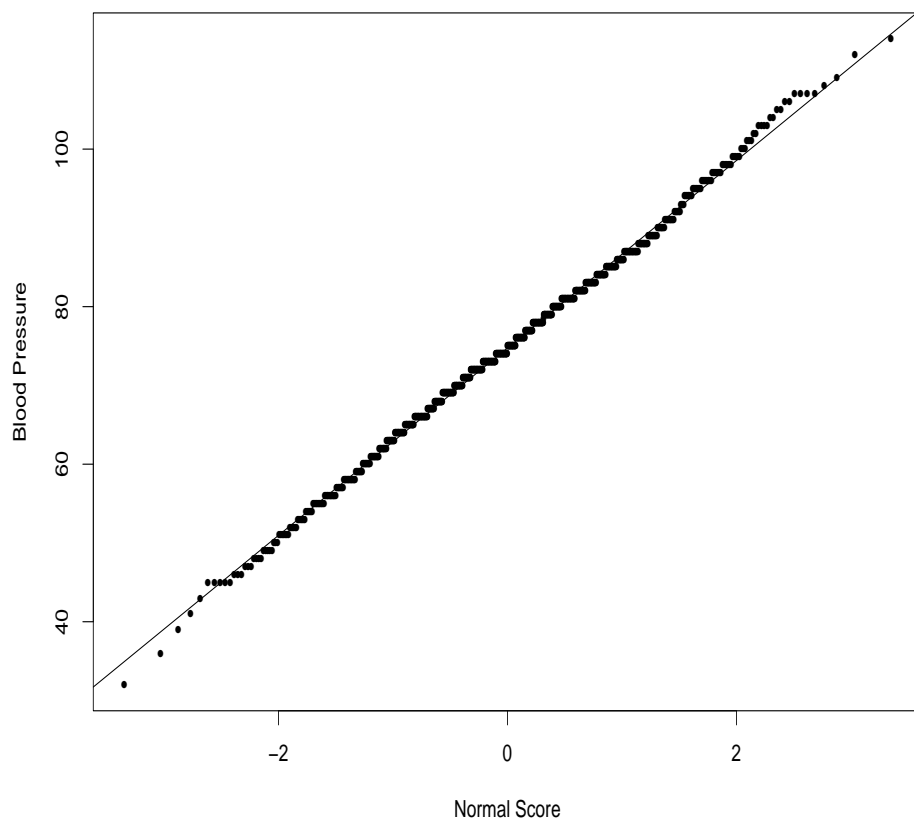
Conducting a goodness-of-fit test involves two choices: the test and the significance level α . Given an alternative, there is a resulting type II error β . We start our development by showing how one can invert goodness-of-fit testing to develop a new measure of model failure. To help fix the idea, we use the following example. The full data set consists of the diastolic and systolic blood pressure data of 10,529 persons aged from 35 to 84. We take only the 1,239 normal females as our data to be analyzed, because the blood pressures of the full sample would likely be better modeled as a mixture of normals. The original data was obtained from the Clinical Trials Research Unit (CTRU) of New Zealand. Central limit theory suggests that such data might be rather normal in distribution. After looking at the QQ plot Figure 1, where there is little deviation from a straight line except at tails, we think many statisticians would be happy using a normal model for such data.

On the other hand, suppose we use the Kolmogorov-Smirnov goodness-of-fit test to test the normality assumption. The test statistic is the greatest absolute vertical distance between the empirical distribution function of blood pressures and the hypothetical normal distribution function, evaluated on the 1,239 sample values. The parameters of the normal distribution are estimated from the sample. Normality is strongly rejected (p -value = 0.0016), a fact which we might attribute to the large sample size ($n = 1,239$). That is, at such a sample size, we have power against what appear to be very small deviations from normality. In this example, the normality is rejected although data looks quite normal at the center. For another example where a simple model is rejectable but still useful, see Bissantz, Munk and Scholz (2003).

How can we say this data is very well described by a normal model without saying it is *exactly* normal? Here is one way to use statistical testing to answer the question.

One starts with a goodness-of-fit test method that has desirable operating characteristics. That is, it should be sensitive to important model failures (alternatives) but insensitive to trivial model failures. We discuss this choice in the next subsection.

Figure 1: QQ plot of the Blood Pressure data of 1,239 females



Given a true probability generating mechanism τ , that is not in the model, and a size α test procedure $I\{T_m(X_1, \dots, X_m) > c_m\}$, one can define the power curve $\beta_\tau(m) = P_\tau\{T_m(X_1, \dots, X_m) > c_m\}$. See Figure 2 for such a plot based on the blood pressure data. Here τ is the empirical distribution of the full data set, the test is the Kolmogorov-Smirnov test for normality with $\alpha = 0.05$. As a simple number summary of such a plot, we define the *maximum credible sample size* of the postulated model (here the normal model in the blood pressure population) to be that sample size $N^* = N^*(\tau, \mathcal{M})$ at which we would reject the model \mathcal{M} 50% of time based on a size $\alpha (< 0.5)$ goodness of fit test. We will also call N^* the *model credibility index*. More generally, one could define N_β^* as the sample size needed to attain power β , in which case the index N^* is $N_{0.5}^*$.

Although one might choose other summaries of the power curve, such as $(N_{0.25}^*, N_{0.75}^*)$, we find N^* to be a natural summary. It also creates certain asymptotic simplifications.

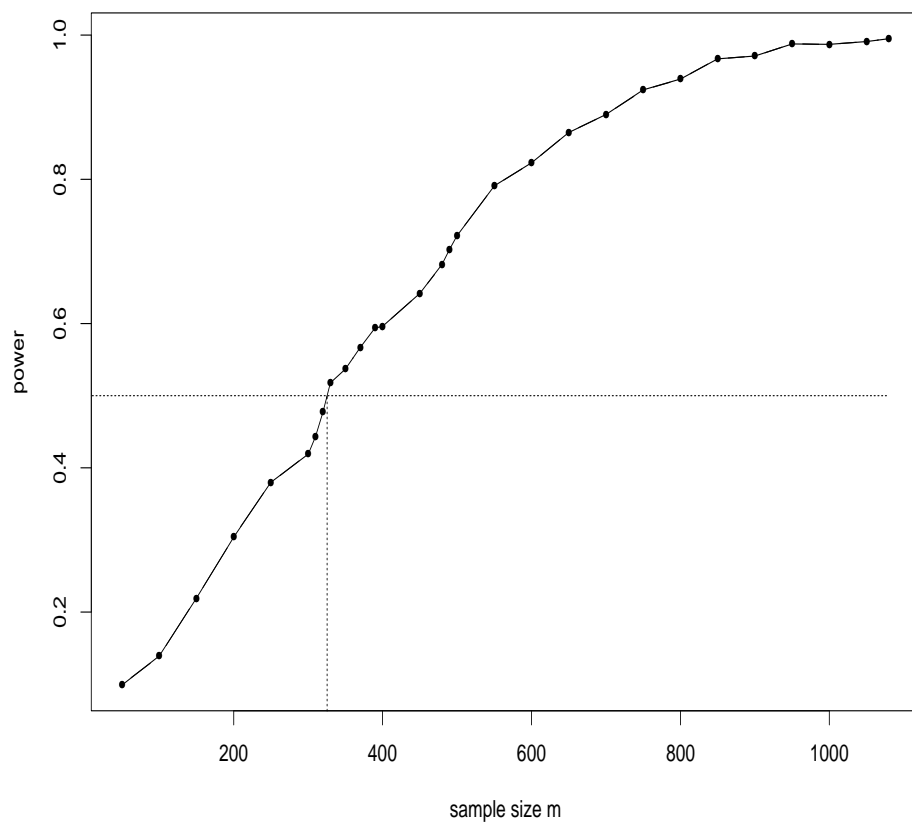
If the model is actually correct, then $N^* = \infty$. However, if the model is false there is some finite sample size at which the power would reach 0.50. Different tests will have different power curves that in turn reveal different inadequacies of the model.

In Figure 2 we assumed that the true distribution τ is random sampling from our set of 1,239 scores, and we determined $\beta(m)$ by simulation. That is, we bootstrapped repeated samples of various hypothetical sizes m from the 1,239 blood pressure values and repeatedly conducted the Kolmogorov-Smirnov test until we found the m that gave power 0.5. For example, in our example we found when $m = 315$, the normality assumption was rejected by the Kolmogorov test approximately 50% of the time (499/1000).

The choice of test size is also arbitrary. Table 1 shows the estimated sample size m when obtaining various power $\beta_\tau(m)$ at different testing significance. The monotone pattern in the table indicates that one would need larger sample size in order to obtain more testing power at a higher test size.

Based on this analysis, it is clear that it would be very hard to detect nonnormality in samples of size 100 from this true distribution ($\beta(100) = 0.13$). To put this another way, the samples of size 100 must “look” very much like samples from a normal distribution, and so one might say that normality is a good descriptor of the sampling mechanism at this sample size.

Figure 2: Plot of test power vs. sample size



Indeed, this descriptive power holds till the sample size approaches 315, when the distinction between normal samples and data mechanism samples must start to become more obvious.

Table 1: m at various test sizes and power levels for blood pressure data

Power $\beta_\tau(m)$	Test Size		
	$\alpha = 0.1$	$\alpha = 0.05$	$\alpha = 0.01$
0.3	115	200	410
0.5	225	315	600
0.7	360	490	795
0.9	540	695	1050

1.2 Role of test statistics

What kind of index is N^* , in a mathematical sense? As we will see later, in a detailed analysis of some standard test statistics, it is inversely proportional to the squared distance measure that was used to construct the test statistic.

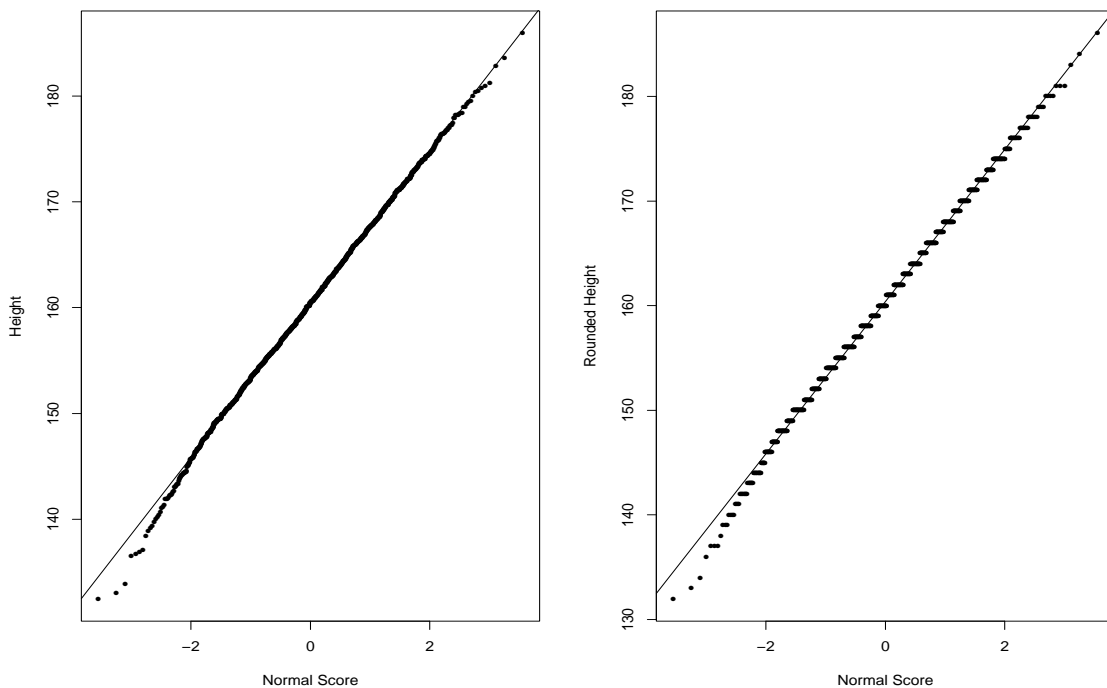
This makes it quite clear that the value of the model credibility index N^* depends strongly on the test statistic that is being used. If we wish N^* to reflect usefulness of the model, then the test statistic must be sensitive to those model failures which we consider most important. Thus the choice of the test must reflect our statistical purposes, as well as which models we consider to be competitors. For example, if we would consider a t-distribution a useful alternative description, having a test sensitivity to tail probabilities would be desirable, say, Anderson-Darling.

Kolmogorov-Smirnov test is a test of normality for large samples. One of its limitation is that it is more sensitive to deviations in the center rather than in the tails. In the blood pressure example, at least the center of data is quite normal (Figure 1). If one is interested of the tail regions, then one should use other tests that are more sensitive to tails. More generally, Claeskens and Hjort(2003) develop model selection tools which can focus on specific aspects of lack of fit.

While trying out other data sets to use in this paper we examined an-

other data set with heights of 2,603 female adults from the data surveys and collection systems of Centers for Disease Control and Prevention (NHANES, 1999-2000). The Kolmogorov-Smirnov test for normality of this set gave a p value greater than 0.10. Although this data set didn't meet Berkson's criterion of 200,000, it was even more normal than the blood pressure set. See Figure 3. We found another interesting thing for this heights data. The original data is coded in centimeters with one decimal accuracy. However, when we rounded the data to integer values, the p -value of the Kolmogorov-Smirnov test became 0.000, leading to a rejection of normality. This illustrates that the Kolmogorov-Smirnov test is sensitive to data coding.

Figure 3: QQ plot of the heights of 2,603 female adults, both original and rounded data



Shapiro-Wilks W statistic (1965) is a well-known goodness of fit test for

the normal distribution. It is attractive because it has a simple, graphical interpretation: one can think of it as the correlation between given data and their corresponding normal scores. Shapiro-Wilks test has good power properties across a wide range of alternative distributions in comparison with other goodness of fit tests (Shapiro, Wilk and Chen, 1968).

For the blood pressure data, normality is also rejected by Shapiro-Wilks W -statistic (p -value=.0043). The credibility index is $N^* = 220$ for the Shapiro test.

The chi-square test, introduced by Pearson in 1900, is the oldest and best known goodness-of-fit test. The idea is to reduce the goodness-of-fit problem to a multinomial setting by grouping data and comparing cell counts. Chi-squared tests can be applied to any type of variable: continuous, discrete, or a combination of these. However grouping the data sacrifices information, especially if the underlying variable is continuous. For the blood pressure data, normality is rejected by the Chi-squared test with p -value = 0.0000; and the credibility index is $N^* = 240$.

In comparing these credibility indices, we recall that— even though N^* has a natural sample size interpretation— it is $\sqrt{N^*}$ that is the more statistically meaningful quantity, as it reflects the standard deviation scale of uncertainty. (This in turn arises, mathematically because N^* is inversely proportional to squared distance, making its root inversely proportional to distance.) For these tests, the root indices were $\sqrt{315} = 17.75$, $\sqrt{220} = 14.83$, and $\sqrt{240} = 15.49$, very similar values, albeit measures of different model fit features.

How might one use the N^* -index? Certainly in any particular data set $N^* = 315$ has its own direct statistical interpretation. And one can use simulation methodology to obtain a better feel for the magnitude of $N^* = 315$; as we do in Section 3.3. More generally, given a specific testing method and type of data set, one could use the N^* values to address the question as to which data set is a better fit to the model and quantify the differences. However, the greatest strength of this methodology is that it creates a universal tool that transcends particular data types and particular testing methods. That in turn raises questions as to whether it is possible to compare N^* -values across different settings in a reasonable way. In particular, one might ask whether an N^* -value is large or small given the number of parameters included in the model. This last question we defer to future research.

1.3 Estimating N^*

To this point we have treated N^* as a population quantity, where the population in our example is a large data set. As such, there is only simulation error in our bootstrap estimation. Inference about N^* when the large data set is itself treated as a sample of size n from a yet large population, so τ is unknown, creates some challenging inference problems. One can, as before, estimate the power curve $\beta_\tau(m)$ by averaging over bootstrap samples of size m , but now the estimator is not unbiased for $\beta_\tau(m)$ unless we use sampling without replacement, a method we will simply call subsampling (See Politis, Romano and Wolf, 1999).

The subsampling framework gives us several tools to tackle inferential questions. In a later section we will show that we have consistent and asymptotically normal estimation of $\beta_\tau(m)$ when m is fixed and $n \rightarrow \infty$. However, in a more realistic scenario in which the sampling fraction $\phi = m/n$ is fixed as $n \rightarrow \infty$, the inverse ratio $\phi^{-1} = n/m$ is shown to be an important measure of the quality of N^* inference. When ϕ^{-1} is small, say 10 or less, then estimator of $\beta_\tau(m)$ has considerable uncertainty.

1.4 Our contents

We have now introduced a measure of the credibility of a model which depends on the hypothesis testing methodology, but it comes with a new interpretation. Note that it is a characteristic of the model, the test statistic and the data generating mechanism, but not the *de facto* sample size n used to estimate it. It is a highly portable statistic, as one can use it in any context where there is a known goodness-of-fit procedure. However, it is also clear that it can only be estimated well when the *de facto* sample size is large enough to make the model in question clearly false.

In this paper we start by discussing how the work of Davies inspired our approach in Section 2, and reviewing briefly other related literature. We then formally define the model credibility index in Section 3. There we also expand upon the normal example so as to compare numerically two-sample and one-sample testing approaches and to compare bootstrapping and subsampling as methods to compute N^* .

In Section 4, we explore the asymptotic properties of the power estimators

associated with the model credibility index. We then in Section 5, examine the structure of the model credibility index in greater detail in the context of likelihood ratio testing in categorical models. We will show how these indices are closely related to Kullback-Leibler discrepancy measures, and give some further numerical examples. Section 7 concludes the paper and propose topics worth of further investigation.

2 Background

In this section we will review some related work on the conceptual difficulty involved in using models while assuming they are false.

2.1 Distance-based indices of fit

A more standard approach to model-false analysis would be to characterize model fitness by choosing a suitable distance measure, then doing inference on the distance between the true distribution and the model.

In 1954 Hodges and Lehmann proposed using tolerance zones around the null hypothesis. They constructed H_1 as a set of distributions whose distance to H_0 doesn't exceed a specified bound c under a distance measurement. Hodges and Lehmann's analysis was in the context of the chi-squared goodness-of-fit test. They used a weighted Euclidean distance as the distance from a model element to the truth. The usual chi-squared distance is included by choosing appropriate weights.

Hodges and Lehmann didn't give a detailed discussion on how one should choose c . They mentioned that the specification of c would "present problems similar to those encountered in choosing the alternative at which specified power is to be obtained." This quoted statement presents some difficulties in its interpretation and implementation.

Liu and Lindsay (2009) expanded on this tubular model idea, but used two different distances, likelihood for the test statistic and Kullback-Leibler for the tube hypothesis. Their tubular model consisted of all multinomial distributions lying within a distance-based neighborhood of the parametric model of interest. The distance between the true multinomial distribution and the parametric model was used as the index of fit. Liu and Lindsay

developed a likelihood ratio test (LRT) procedure for testing the magnitude of the index.

Goutis and Robert (1998) proposed a Bayesian approach for model selection problem based on the likelihood deviation between two nested models, called the full and restricted models. The full model space was considered to contain the true distribution. The Bayesian approach was implemented by specifying a prior distribution in the full model, possibly an improper prior. Each prior distribution was projected onto the restricted model space and the corresponding minimum distance measure was computed. Therefore the posterior distribution of the distance from a prior distribution to the restricted model can be derived. Bayesian inference was made on the restricted model based on the posterior distribution. For example, one criterion was to reject the restricted model if the posterior probability that the distance was less than a certain bound c was small enough. Other aspects of the posterior distribution could be considered as the testing criteria. When one doesn't have a strong prior belief, several priors could be used to assess the distance between models. The sensitivity of the inference to the priors could be used as a factor in making the model choice.

Dette and Munk (2003) used the Euclidean distance in the problem of testing for a parametric form hypothesis in regression. They assumed that the true model was an unknown nonparametric regression function. Goodness-of-fit was measured by the Euclidean distance between the unknown true regression function and the parametric model.

Dette and Munk first estimated the Euclidean distance under the null hypothesis. To obtain the distance under the alternative, the classical concept of analysis of variance was generalized to the nonparametric setting. Their goodness-of-fit statistic measure could be interpreted as the difference between variance estimators under the null model and the nonparametric model.

The challenge one faces with all approaches that use distances directly, such as those described above, is that it is very difficult to give statistically meaningful interpretations to the numerical values of the distance. The credibility indices we have explored here are, in essence, reciprocals of such distances. However we believe that they are easier to interpret, as they measure the ability of the model to describe samples of various sizes. They are also more universal, having meaning across a wide range of settings.

2.2 Davies

In our search for a reasonable way to measure how well a model describes a data generating mechanism, we came across the work of Davies.

Davies (1995) proposed the idea of judging model adequacy using the concept of data feature. The basic idea is that if samples that are simulated from the model are largely indistinguishable from the real data, then the model should be regarded as adequate. A similar idea is expressed in Donoho (1988) via the statement: “No distribution which produces samples very much like those actually seen should be ruled out *a priori*.”

Davies’ formal theory of data features is very similar to hypothesis testing for goodness of fit, with the test statistics being designed to assess whether the data had the same features as a sample from the model. In common with testing theory (but contrary to us), he measures the adequacy of models from the null-centric convention (i.e. that the model is correct) and does so at the *de facto* sample size.

Another distinction from our approach is that rather than using model-based one-sample test statistics, he would use a nonparametric two-sample test to compare the data not with the model, but with samples from the model. This has the conceptual advantage of being a direct answer to the question “Does this data look like a typical sample from the model?”

The disadvantage to this approach is that it limits the number of testing procedures available for model assessment. We believe that one-sample test is addressing the right question, but it does have more power because it removes sampling uncertainty. An example in the Section 3.3 shows that there would be a substantial change in magnitude of N^* if we used a two-sample approach.

3 Credibility Index

3.1 The formal definition

In constructing these indices, we have used the conventional test size $\alpha = .05$. For a given test, we let $N^* = N^*(\tau, calM)$ be the value of n that gives this test power 0.5 at true distribution τ , when the model is \mathcal{M} . Any test that is consistent for every alternative hypothesis (i.e., an omnibus test of fit)

will give a finite N^* under the false model assumption.

The choice of α here seems like an arbitrary element, but we will see later that it plays a minor role in the comparison of N^* -values. The choice of power 0.5 is also somewhat arbitrary, but there are two strong reasons behind this choice. First, there is the intuitive appeal of the idea that the model decision is 50/50 at this point and so the decision is “up for grabs.” The index is the middle value of the power curve and so provides natural one number summary (Eg. Figure 2). Secondly, this value of the power greatly facilitates the asymptotic analysis, as we will soon see.

In an intuitive sense, the model credibility index $N^*(\tau, \mathcal{M})$ operates reciprocally to distance in the following sense. When a true distribution τ is moved closer to the model, so the distance is reduced, the sample size index should increase because a larger sample size n would be needed for discrimination between τ and \mathcal{M} . Typically goodness-of-fit test statistics are based on distance measures; in these cases the reciprocal connection can be made more precise, as we will soon see.

3.2 Determination of N^*

One attractive thing about the testing index N^* is that it admits an elementary subsampling estimation. This could be carried out in a typical IID setting as follows.

Given a target size α and a data set x_1, \dots, x_n , suppose one would ordinarily conduct the goodness-of-fit test of the model based on an asymptotic critical value. One could then estimate N^* for this test procedure by conducting a nonparametric bootstrap simulation using various sample sizes m to estimate the power $\beta_\tau(m)$, the goal being to find the value of m such that $\beta_\tau(m) = 0.5$. If we let the symbol \hat{F} represent the empirical distribution, we are treating \hat{F} as τ , and calculating $\hat{N}^* = N^*(\hat{F}, \mathcal{M})$. Now assuming the model \mathcal{M} does not include the empirical distribution \hat{F} , the bootstrap sampling distribution is under the alternative, and so the rejection probability, which is the power of the test, should increase in m . The female blood pressure example in Section 1 is an example of the bootstrap determination of N^* .

As we will explain later, there are good reasons to use sampling without

replacement (“subsampling”) instead of with replacement (“bootstrapping”). In subsampling the largest possible value of m is n , and the resulting estimated power $\hat{\beta}(n)$ is 1, if the test rejects and 0, if the test accepts. This reflects our lack of knowledge (in the model false world) about the model’s capacity to explain future samples of size n or larger.

To carry out a subsampling or a bootstrap determination of N^* , one needs to define an efficient algorithm so as to minimize computation time. Obviously, sensible interpolation methods should be used. Moreover, it would be nice to have a good starting value based on asymptotic approximations. See section 5.1 for more on this issue.

3.3 One-sample and two-sample indices

In this section we use a particular simulation model to compare different ways of computing N^* . We start by comparing one- and two- sample credibility indices. In this process, we also learn something more about how to interpret the magnitude of a model credibility index.

Suppose we draw two samples of size m , say one each from a normal and a logistic distribution, where the parameters are chosen to make the distributions as similar as possible. We could measure their similarity by using a two-sample test to see if the samples are detectably different. Doing this repeatedly gives us the power of the two-sample test between the two distributions.

We did this using the two-sample Kolmogorov-Smirnov test, using 1000 samples for each m . Table 2 lists the number of rejections for various sample sizes.

Suppose we let the *model credibility index* N^* be the value of n that gives power 0.50. In this example, $N^* \approx 2650$. We found it quite striking that the normal and logistic models would be so poorly discriminated on the basis of this test.

A one-sample version of this index could be created by fixing the normal density as the null hypothesis, and investigating the power of the one-sample Kolmogorov-Smirnov using logistic samples. As seen in Table 3, this test is considerably more powerful than the two-sample one.

Note that this analysis also shows that N^* , when the model is normal and

Table 2: Power of the Kolmogorov-Smirnov test (two-sample method) to detect the difference between normal and logistic distributions at selected sample sizes

m	Rejection Proportion
100	0.044
500	0.116
1000	0.169
2000	0.361
$\hat{N}^*=2650$	0.513
4000	0.768
6000	0.907

Table 3: Power of the Kolmogorov-Smirnov test (one-sample method) to detect the difference between normal and logistic distributions at selected sample sizes

m	Rejection Proportion
100	0.126
400	0.435
450	0.479
$\hat{N}^*=485$	0.500
500	0.518
1000	0.824
2500	1.000

the true distribution is logistic, is about 485, and so logistic samples are closer to normality than is the blood pressure data set.

Finally we use this example to compare the bias and deviation of \hat{N}^* when estimated by bootstrap simulation with \hat{N}^* when estimated by subsampling simulation. Consider a large data set of size n from the logistic distribution. We let m be fixed and simulate the powers of one-sample Kolmogorov test for normality by bootstrapping and by subsampling. We take 500 data sets from logistic distribution at each size n . The simulated average and standard deviation of power are in Table 4 for $m = 485$ and $n = 1,000, 10,000$ and $100,000$.

The true power for the infinite population is approximately 0.5. The results show that as the empirical data size n gets larger and larger, the simulated power gets closer and closer to the true value. Although the standard deviations are almost the same for bootstrap method and the subsampling method, the simulated power by bootstrap is much more biased for small n . With the bootstrap method, sample size 485 is estimated to have 0.66 power when n is 1000. That again indicates that estimation of N^* by bootstrap tends to have a downwards bias.

Table 4: Simulated Power of Normality test for finite population from logistic.

n	Bootstrap		subsampling		$\phi^{-1} = \frac{n}{m}$	subsampling
	Mean	Deviation	Mean	Deviation		EISS
1,000	0.659	0.1753	0.493	0.2472	$\frac{1000}{485} = 2.06$	4.14
10,000	0.517	0.0710	0.499	0.0735	$\frac{10000}{485} = 20.6$	44.3
100,000	0.503	0.0278	0.501	0.0252	$\frac{100000}{485} = 206.2$	393.7

The reader should note the large standard deviation when $n = 1000$. The last two columns will be discussed later in the context of understanding how well one can estimate power nonparametrically.

4 Asymptotic issues in power estimation

In this section we examine the asymptotic properties, as $n \rightarrow \infty$, when one estimates the power curve $\beta_\tau(m)$ by subsampling or bootstrapping.

Suppose our test statistic is $T_n = T_n(x_1, \dots, x_n)$, symmetric in its arguments. Suppose our test procedure is to reject H_0 when $\{T_n(X_1, \dots, X_n) > c_\alpha\}$, where c_α is an asymptotic critical value for the test. The object of interest is

$$\beta(m) = P_\tau\{T_m(X_1, \dots, X_n) > c_\alpha\}.$$

When the null hypothesis is true (i.e., includes τ), we have $P_\tau\{T_n(X_1, \dots, X_n) > c_\alpha\} \rightarrow \alpha$ as $n \rightarrow \infty$.

We will derive asymptotic results for subsampling based estimation of $\beta(m)$, with side notes on the effect of using bootstrap sampling instead. Notice that $I\{T_m(X_{s_1}, \dots, X_{s_m}) > c_\alpha\}$, for any set of distinct integers a_1, \dots, a_m , is an unbiased estimator of $\beta(m)$. Let $S = \{s_1, \dots, s_m\}$ be a subset of m distinct integers sampled from $\{1, \dots, n\}$, and let $X_S = (X_{s_1}, \dots, X_{s_m})$. Finally, let $K_m(X_S) = I\{T_m(X_{a_1}, \dots, X_{a_m}) > c_\alpha\}$. We can construct a U statistic estimator of $\beta(m)$ by

$$U_{comp}(X) = \frac{1}{\binom{n}{m}} \sum_{S \in \mathbb{S}} K_m(X_S)$$

where \mathbb{S} is the set of all distinct subsets of $\{1, \dots, n\}$ of size m . We can also write this as an expectation :

$$U_{comp}(X) = E[K_m(X_S) | X_1, \dots, X_n]. \quad (4.1)$$

Here the expectation is over samples of m integers without replacement from $\{1, \dots, n\}$, with $X = (X_1, \dots, X_n)$ fixed.

We will call this the complete U statistic; in practice we are unlikely to use it because of the $\binom{n}{m}$ calculations required. The approximation we consider will replace this exact expectation with a subsampling estimator created by randomly sampling S . Another possible computational shortcut would be to use a statistical design for the selection of a subset of S (Blom, 1976). We will focus here on the properties of U_{comp} itself, corresponding to an ideal infinite subsampling scheme. In this setting, we can think of the estimator obtained by bootstrap subsampling as being the corresponding V -statistic estimator of β .

4.1 Fixed m asymptotics

We can now make some observations about the consistency of this form of estimation. The answer depends on the asymptotic setting. If we assume that m is held fixed as $n \rightarrow \infty$, *fixed m asymptotics*, then we can apply the following standard U statistic theory, and obtain consistency and asymptotic normality for the estimation of $\beta(m)$ as follows.

The exact and asymptotic variance of U_{comp} is described in Theorem 4.1 (Lehmann, 1999)

Theorem 4.1. *If $\text{Var}[K_m(x_1, \dots, x_i, X_{i+1}, \dots, X_m)] = \sigma_i^2$, then*

(1) *The variance of the U -statistic is equal to*

$$\text{Var}(U_{comp}) = \sum_{i=1}^m \binom{m}{i} \binom{n-m}{m-i} \sigma_i^2 / \binom{n}{m}.$$

(2) *If $\sigma_1^2 > 0$ and $\sigma_i^2 < \infty$ for all $i = 1, \dots, m$, then*

$$\text{Var}(\sqrt{n}U_{comp}) \rightarrow m^2\sigma_1^2.$$

Theorem 4.2 gives the asymptotic normal property of U_{comp} .

Theorem 4.2. (1) *If $0 < \sigma_1^2 < \infty$, then as $n \rightarrow \infty$,*

$$\sqrt{n}(U_{comp} - \beta) \xrightarrow{d} N(0, m^2\sigma_1^2);$$

(2) *If $\sigma_i^2 < \infty$ for all $i = 1, \dots, m$, then*

$$\frac{U_{comp} - \beta}{\sqrt{\text{Var}(U_{comp})}} \xrightarrow{d} N(0, 1).$$

Because for us K_m is an indicator function, the condition that $\sigma_i^2 < \infty$ for all i is obviously satisfied. When m is fixed, these limiting distribution results hold for the bootstrap estimator of $\beta(m)$ because it is the corresponding V -statistic.

4.2 Fixed sampling ratio asymptotics

Unfortunately using fixed m asymptotics is incredibly optimistic in our setting, as we wish to be able to estimate $\beta(m)$ for m as close to n as possible.

The more realistic asymptotics we will use to study this case will consider sequences in n in which $m = m_n$ is some fixed fraction ϕ of n , which we call *fixed ratio asymptotics*. In this setting the target value $\beta_\tau(m_n)$ will be changing in n , going to 1, and so we also need to consider local alternative sequences τ_n .

To study this, we first derive some properties of $\text{Var}(U_{\text{comp}}(m))$. For any two independent samples S_1 and S_2 of size m from $\{1, 2, \dots, n\}$, let $|S_1 \cap S_2| = O(S_1, S_2)$ be the number of common elements. We will call $O(S_1, S_2)$ the *sample overlap*. It has a hypergeometric distribution, so it is an elementary calculation to show that $E\{O(S_1, S_2)\}/m = m/n = \phi$. That is, the sampling fraction ϕ is also the mean fractional overlap between subsamples. We can then write

$$E(U_{\text{comp}}^2(m)) = \sum_{k=0}^m E[K_m(S_1)K_m(S_2)|O(S_1, S_2) = k] \Pr[O(S_1, S_2) = k]. \quad (4.2)$$

As we will show below, the U-statistic can suffer a severe degradation in variance, relative to the fixed m asymptotics, if the mean overlap ϕ in the indices is too large. (Note that ϕ goes to zero in fixed m asymptotics, so the overlap mean goes to zero.) As a way to measure the overlap effect, we define an *equivalent independent sample size* (EISS) measure using the formula:

$$\text{Var}(U_{\text{comp}}(m)) = \frac{\text{Var}(K_m(X_1, \dots, X_m))}{\text{EISS}}.$$

For our indicator kernel K_m this gives the formula

$$\text{Var}(U_{\text{comp}}(m)) = \frac{\beta_\tau(m)(1 - \beta_\tau(m))}{\text{EISS}}$$

and so we can think of EISS as being the sample size we would need to conduct an IID experiment with equivalent accuracy in estimating $\beta(m)$.

From a standard U statistic inequality (Blom, 1976, p 574), we have

$$\text{Var}(U_{\text{comp}}) \leq \frac{\text{Var}(I\{T_m(X(S)) > c_\alpha\})}{n/m} = \frac{\beta_\tau(m)(1 - \beta_\tau(m))}{n/m}. \quad (4.3)$$

As a consequence we are guaranteed consistent estimation of $\beta_{\tau_n}(m_n)$, along any sequence of alternatives τ_n , when $\phi = m_n/n$ goes to zero. We note that bootstrap resampling does not have this strong guarantee of consistency, as general results require m^2/n to go to zero (Politis et al, 1999).

This inequality also implies that $EISS \geq \phi^{-1} = n/m$. That is ϕ^{-1} gives us a lower bound for EISS for $\beta(m)$ inference. For example, a sampling fraction of $\phi = 1/25$ is guaranteed to provide at least as accurate estimation of $p = \beta(m)$ as would 25 draws from a Bernoulli distribution with success probability p . As we will later see, this inequality can also be thought of as an approximation when ϕ^{-1} is small, helping to give one the proper degree of pessimism about N^* inference in this case.

4.3 Local alternatives: a closer look

To more closely examine this approximation, we consider certain local alternatives τ_n to the null hypothesis. We will assume now that the test statistic at hand admits a standard local asymptotic analysis under alternatives of the form $\tau_n = F_0 + n^{-1/2}cg(x)$, for fixed $g(x)$, positive c , and null element F_0 . In this setting one can typically show that $\beta_{\tau_n}(n) \rightarrow \beta_{loc}(c)$ as $n \rightarrow \infty$, where the local alternative power curve $\beta_{loc}(c)$ is a continuous increasing function of c . Eg., for Pearson's Chi square test, the local analysis leads to a noncentral Chi-square distribution. (See Ferguson, 1996, p 63.) To find the local power along the sequence τ_n when a different sample size used, say $m_n = \phi n$, we can rewrite the alternative as

$$\tau_n = F_0 + m_n^{-1/2}\phi^{1/2}cg(x).$$

The sample size changes the scaling factor from c to $\phi^{1/2}c$. Hence the asymptotic power approximation for samples of size m_n from τ_n is $\beta_{loc}(\phi^{1/2}c)$. Assuming that c is chosen so that $\beta_{loc}(c) > 1/2$, there will be a fraction $\phi_{.5}$ such that $\beta_{loc}(\phi_{.5}^{1/2}c) = 1/2$. That is, if we choose $\phi = \phi_{.5}$, we have $\beta_{\tau_n}(m_n) \rightarrow .5$, for $m_n = \phi_{.5}n$. As a consequence the true N^* value for the τ_n sequence grows proportionally to n , namely $\phi_{.5} \times n$.

Since $\phi = m/n$ is fixed, our proceeding result about the consistency of U_{comp} is not operative. In fact, in local alternative settings, the estimator is generally not consistent. However, it is possible to obtain useful understanding of how the variance changes as a function of ϕ , and so examine its role in estimation. Returning to the formula

$$E(U_{comp}^2(m)) = \sum_{k=0}^m E[K_m(S_1)K_m(S_2)|O(S_1, S_2) = k] \Pr [O(S_1, S_2) = k].$$

the second term on the right has the elementary calculation

$$\Pr [O(S_1, S_2) = k] = \frac{\binom{m}{k} \binom{n-m}{m-k}}{\binom{n}{m}}.$$

This hypergeometric distribution has mean $\phi_m = (m/n) \cdot m$ and variance bounded above by $m\phi(1 - \phi)$, the corresponding binomial variance. Hence $O(S_1, S_2)/m$, the fractional overlap, converges in probability to ϕ in our asymptotic setting.

For this reason, it is reasonable to approximate the terms

$$E [K_m(S_1)K_m(S_2)|O(S_1, S_2) = k_n]$$

along a sequence of k 's for which the samples have a fixed fractional overlap, say $k_n = am_n = a\phi n$, in order to approximate the important terms in the variance.

Although such a task is dependent on the structure of the test statistic, we think it is worthwhile to illustrate here how these calculations could be carried out. We consider a test statistic which is asymptotically chi-squared distributed, with degrees of freedom d under the null hypothesis and is asymptotically non-central chi-squared, with non-centrality parameter δ under the local alternatives sequence.

If we let $G(t) = \Pr\{\chi_{d-1}^2 > t\}$, then for fixed overlap fraction a , then under standard local asymptotic calculations,

$$E [K_m(S_1)K_m(S_2)|O(S_1, S_2) = k_n] \rightarrow A$$

where A can be calculated as the expectation of

$$\begin{aligned} & G \left(\frac{1}{1-a} c_\alpha - \left(X + \sqrt{(Z\sqrt{\frac{a}{1-a}} + \sqrt{\frac{1}{1-a}}\delta)^2 + \frac{a}{1-a}W} \right)^2 \right) \times \\ & G \left(\frac{1}{1-a} c_\alpha - \left(Y + \sqrt{(Z\sqrt{\frac{a}{1-a}} + \sqrt{\frac{1}{1-a}}\delta)^2 + \frac{a}{1-a}W} \right)^2 \right) \end{aligned} \quad (4.4)$$

where X, Y, Z are independent normal variables and W is independently χ_{d-1}^2 .

In Table 5 we show some calculations from this formula for $d = 25$, where δ is chosen as 3.67 so as to obtain asymptotic power .5. The critical value is $c_{.05} = 37.66$.

Table 5: Simulated EISS for various sampling fraction ϕ .

ϕ^{-1}	EISS	ϕ^{-1}	EISS	ϕ^{-1}	EISS
2	4.2	15	52.9	50	231.6
3	7.4	20	74.6	60	294.1
4	10.7	25	97.7	75	398.0
5	14.1	30	122.0	80	435.6
10	32.6	40	174.3	100	601.7

We note several features here. First, ϕ^{-1} is relatively conservative, but for small values does provide the right caution. Here $\phi^{-1} = 10$ gives an EISS of 32.6, something like a bare, minimum needed for N^* inference. If we compare this table with the values from the simulation in Table 4, we see that in the latter, EISS was about $2 \times \phi^{-1}$ across a larger range of sampling fractions, and so did not show the steady improvement found in Table 5.

5 Credibility in categorical data models

Our setting for analyzing the mathematical features of credibility indices more carefully will be likelihood ratio tests in categorical models.

5.1 Asymptotic Approximations

We derive two approximations to N^* here, focusing on the likelihood ratio test in multinomial models. Here the data will be an IID sample from a multinomial distribution, as summarized by the counts $n(t)$ in the cells $t = 1, \dots, T$. The cell proportions will be denoted $d(t) = n(t)/n$, which represent the empirical distribution \mathbf{d} of the data. The model \mathcal{M} will have elements $F_\theta(t)$ representing a parametric model for the multinomial cells— for example, a log-linear model. The testing statistic will be the likelihood ratio, and we will assume that the test statistics have the standard asymptotic chi-squared distributions under the null models.

In this context we can derive a simple asymptotic version of the testing index and show that it is proportional to a reciprocal squared distance. This

in turn leads to an elementary consistent estimator of the asymptotic index. This estimator has two important uses: it can be used for a preliminary value of the index for bootstrap or subsampling testing. It can also itself be bootstrapped or subsampled, which then provides a simple way to assess the variability of the estimated index.

The *likelihood deviation* between a multinomial distribution \mathbf{p} and a model element \mathbf{F}_θ is defined as $L^2(p, F_\theta) = \sum p(t) \log(p(t)/F_\theta(t))$. This is a version of the Kullback-Leibler distance; we call it the likelihood deviation to clarify the asymmetric role of \mathbf{p} and \mathbf{F} . Technically it operates as a squared distance, which is why we use the superscript 2. We also define the likelihood deviation from a multinomial distribution \mathbf{p} to the model \mathcal{M} to be

$$L^2(\mathbf{p}, \mathcal{M}) = \inf_{\theta} L^2(\mathbf{p}, \mathbf{F}_\theta). \quad (5.1)$$

For the true sample distribution $\boldsymbol{\tau}$, if the infimum is attained at a particular θ , it will be denoted θ_τ , and the model element that approximates $\boldsymbol{\tau}$ is therefore denoted \mathbf{F}_{θ_τ} .

In the likelihood ratio test, one rejects the null hypothesis $H_0: \boldsymbol{\tau} \in \mathcal{M}$ at asymptotic size α , if the likelihood ratio test statistic is large enough, that is

$$2nL^2(\mathbf{d}, \mathcal{M}) \geq \chi_{df}^2(\alpha),$$

where $\chi_{df}^2(\alpha)$ is the upper $1 - \alpha$ quantile of chi-squared distribution with df = the degrees of freedom. The power of the test at sample size n when $\mathbf{d}_n \sim \boldsymbol{\tau} \notin \mathcal{M}$ is

$$P_{\boldsymbol{\tau}} \{2nL^2(\mathbf{d}_n, \mathcal{M}) \geq \chi_{df}^2(\alpha)\}.$$

Our goal is to determine the sample size N^* at which the testing power for the alternative $\boldsymbol{\tau} \notin \mathcal{M}$ is 0.5. That is,

$$P_{\boldsymbol{\tau}} \{2N^*L^2(\mathbf{d}_{N^*}, \mathcal{M}) \geq \chi_{df}^2(\alpha)\} = 0.5.$$

Our first approximation to N^* uses the fact that when the model is false, the centered likelihood ratio statistic has, asymptotically, a centered normal distribution. The approximation, as derived in the Appendix, is

$$N_{asy}^*(\boldsymbol{\tau}) = \frac{\chi_{df}^2(\alpha)}{2L^2(\boldsymbol{\tau}, \mathcal{M})}. \quad (5.2)$$

Here our choice of the power .5 greatly simplifies the expression. other choices for N_{β}^* would depend on the limiting variance for the normal distribution.

Our second approximation is a bit more sophisticated. We consider local alternatives that approach the null as the sample size goes to infinity. This gives a noncentral chi-square approximation:

$$N_{asy2}^*(\tau) = \frac{(\delta^*)^2}{X^2(\tau, \mathcal{M})}. \quad (5.3)$$

In equation (5.3), $X^2(\tau, \mathcal{M})$ is the Pearson chi-square distance,

$$X^2(\tau, F) = \sum \frac{(\tau - F)^2}{F},$$

and $(\delta^*)^2$ is the noncentrality parameter that satisfies

$$P \left\{ \chi_{df}^{\prime 2}((\delta^*)^2) > \chi_{df}^2(\alpha) \right\} = 0.5, \quad (5.4)$$

where $\chi_{df}^{\prime 2}(\delta^2)$ is a noncentral χ^2 distribution with degrees of freedom df and noncentrality parameter $(\delta^*)^2$. One can generalize this approximation by changing the righthand side of (5.4) to a chosen power level. See the Appendix for more details.

The second approximation should be more accurate than the first for situations when τ is close to the model. Notice that both approximations (5.2) and (5.3) show an inverse relationship to squared distance. Moreover, we can see that α plays a role only in the numerator of the approximation. Given two models with the same testing degrees of freedom, the ratio of approximate N^* values does not depend on α .

Another useful feature of N_{asy}^* arises in confidence assessment. One could form asymptotic confidence intervals for $N^*(\tau)$ by bootstrapping \hat{N}^* , but this requires double bootstrapping, an expensive possibility. But bootstrapping $N_{asy}^*(\mathbf{d})$ is relatively inexpensive and it can give a useful picture of the uncertainty involved. More rigorous methods of using subsampling to estimate standard errors are under investigation by the authors.

5.2 Numerical Examples

We next assess model credibility for the data in Tables 6 and 7. Table 6, considered earlier by Snee (1974), is a 4×4 table cross-classifying eye color

and hair color. The sample size $n = 592$ is somewhat large but the table does have some small entries. The Pearson statistic for the independence model is $X^2 = 138.290$ on 9 degrees of freedom, and the likelihood ratio statistic is $L^2 = 146.444$. The model would be rejected on the basis of these quantities.

Table 6: Cross-classification of eye color and hair color (size $n = 592$)

<i>Eye color</i>	<i>Hair color</i>			
	<i>Black</i>	<i>Brunette</i>	<i>Red</i>	<i>Blonde</i>
Brown	68	119	26	7
Blue	20	84	17	94
Hazel	15	54	14	10
Green	5	29	14	16

We tested the independence model for data in Table 6, where the degrees of freedom are 9. We then apply the two approximations, (5.2) and (5.3), to obtain the starting value for $N^*(\mathbf{d})$, which are $N_{asy}^*(\mathbf{d}) = 34$ and $N_{asy2}^*(\mathbf{d}) = 37$.

We further refine the preliminary value by bootstrap. Given the target size $\alpha = 0.05$, we took various sample sizes m , then generated $B = 1000$ bootstrap samples \mathbf{d}_b^* from Multinomial(m, \mathbf{d}), with margins not fixed. We then conducted the size α likelihood ratio test, and recorded the fraction of rejections, $\# \left\{ 2nL^2(\mathbf{d}_b^*, \mathcal{M}) \geq \chi_{df}^2(\alpha) \right\} / B$. The estimate of $N^*(\tau)$, $N^*(\mathbf{d})$, would be that sample size that gives rejection fraction 50%. See Table 8 for the numbers, as well as a comparison of bootstrap and subsampling in this example.

In this case $N^*(\mathbf{d}) = 32$, which is very close to the first asymptotic value of 34. A 95% bootstrap interval for $N_{asy}^*(\tau)$ was found to be (25, 43). Note that $\phi^{-1} = 592/32 = 18.5$, suggests that inference about N^* is reasonable.

Diaconis and Efron (1985), in addressing the same problem posed by this paper, suggested a different way of generating an assessment of this particular data set. They compared the observed X^2 -value with those of all possible 4×4 tables with $n = 592$. They found that, among all 4×4 tables with $n = 592$ (margins not fixed), approximately 10% have X^2 -values less than

138.29. They concluded that the given 4×4 table does not lie particularly close to independence.

Our second example, Table 7, originally published in Cramér (1946), is a 5×4 table cross-classifying number of children by annual income levels. The sample size is $n = 25,263$, which is very large. The goodness-of-fit statistics are $X^2 = 568.566$ and $L^2 = 569.420$ on 12 degrees of freedom. The χ^2 -statistics have extremely small p -values leading to rejection using the conventional criteria.

Diaconis and Efron (1985) used this example as well. They found that, among all 5×4 tables with $n = 25,263$ (margins not fixed), the proportion of those having X^2 less than 568.576 is 2.1×10^{-7} . They concluded that the observed table is extremely close to independence, which is dramatically opposite from the conclusion drawn from the χ^2 -values.

Table 7: Cross-classification of number of children by annual income (size $n = 25,263$)

<i>No. of children</i>	<i>Annual income</i>			
	0 – 1	1 – 2	2 – 3	3+
0	2161	3577	2184	1636
1	2755	5081	2222	1052
2	936	1753	640	306
3	225	419	96	38
4+	39	98	31	14

The credibility index for Table 7 was calculated as follows. The starting estimate values of $N_{asy}^*(\mathbf{d})$ for the data in Table 7 was 470 and its bootstrap range was (386, 548), while $N_{asy2}^*(\mathbf{d}) = 439$. We refined the estimate to $N^*(\mathbf{d}) = 425$ using the bootstrap procedure (margins not fixed). Here the closeness of the model and sample explains why N_{asy2}^* worked better as a bootstrap starting value. Note that $\phi^{-1} = 25263/425 = 59.4$, suggesting that inference on N^* is reasonable. See Table 8 for more details.

It is clear that Table 7 lies much closer to the independence model than Table 6. Using the credibility index as a guide, we would say that the row-

column independence model is credible only for samples of size $N = 32$ or smaller for the population represented by Table 6. Table 7 is credible for samples that are more than ten times as large.

The magnitude of the ratio for the Efron-Diaconis statistics is on a completely different scale, being 4.8×10^5 . Of course, the statistics involved are quite different in interpretation. The Efron-Diaconis statistic and our index are not asking the usual questions for contingency tables. The Efron-Diaconis statistic seems to ask “is this table surprisingly close to independence?” It is calculated by assuming that prior to data collection, every possible table of that sample size was equally likely. We ask instead, “does this table come from a population that generates samples that look independent, even for large n ?”

Table 8: Summary of sample sizes and the corresponding power for data in Tables 6 and 7

Power for Table 6			Power for Table 7		
m	Bootstrap subsampling		m	Bootstrap subsampling	
34	.676	0.568	470	.578	0.548
$\hat{N}^* = \mathbf{32}$.505	0.497	450	.544	0.529
31	.512	0.484	430	.505	0.507
30	.481	0.474	$\hat{N}^* = \mathbf{425}$.495	0.500
29	.480	0.467	400	.482	0.479

6 Discussion

The statistical community is currently facing an enormous challenge (and opportunity) that arises from the new data generating capacity of science and engineering (Lindsay *et al.*, 2005). This paper has been concerned with the question: “How should we reconcile our parametric modeling tools with the fact that in a truly large data set, parametric models are either clearly false or are too complex to be concise descriptors of the key data features?” We have tackled one small part of this problem, assessing the quality of a model’s fit

while assuming it is false. We have done so by modifying hypothesis testing methods so that they can be used from a model false perspective.

If model credibility indices are a good idea, then many questions remain. For example, can we design the test procedures, and the corresponding N^* values, that would reassure us about the robustness of using a standard model-based statistical procedure? Is there a good way to use N^* quantify, in an absolute sense, what it means for a model to be a surprisingly good fit to a set of data, as in saying that a data set is “highly normal”? The theoretical development of this idea might involve comparison of the credibility of the chosen model with a randomly selected model with the same number of parameters.

Another issue regards the comparison of N^* values in models across differing numbers of parameters. One possibility is to create an index that adjusts for the number of parameters, such as $N^*/(\# \text{ parameters})$. The form of such an index then could depend on how we might “expect” N^* to grow when the number of parameters grows, given a sequence of arbitrary models.

Although we recognize that the ideas presented here are only a beginning, we hope the reader has found them to be stimulating.

A Appendix: Two approximations to N^*

A.1 Approximation through normal distribution

We can obtain a quick-and-dirty approximation using the fact that—when the model is false—the centered likelihood ratio statistic has, asymptotically, a centered normal distribution.

Lemma A.1. *If $\{n(t)\}$ are a multinomial sample of size n from a fixed distribution τ not in \mathcal{M} , then as $n \rightarrow \infty$,*

$$\sqrt{n} (L^2(\mathbf{d}_n, \mathcal{M}) - L^2(\boldsymbol{\tau}, \mathcal{M})) \longrightarrow N(0, \sigma^2)$$

provided that the asymptotic variance σ^2 is not zero or infinity.

The lemma is just the maximum likelihood within von Mises’ framework (Serfling (1980), page 211). Freitag and Munk (2005) have a bootstrap variant, which is an interesting extension of the Lemma.

Note that this lemma applies to bootstrap sampling from the empirical distribution $d(t)$ (treating it as τ) whenever the data $d(t)$ is not perfectly fit by the model. Now the value of N that we seek satisfies

$$P \left\{ \sqrt{N}L^2(\mathbf{d}_N, \mathcal{M}) - \sqrt{N}L^2(\tau, \mathcal{M}) \geq \frac{1}{2\sqrt{N}}\chi_{df}^2(\alpha) - \sqrt{N}L^2(\tau, \mathcal{M}) \right\} = 0.5.$$

Since the left hand term is asymptotically normal with mean zero, this suggests that we need N to solve

$$\frac{1}{2\sqrt{N}}\chi_{df}^2(\alpha) - \sqrt{N}L^2(\tau, \mathcal{M}) = 0.$$

Note that this calculation is independent of the unknown σ^2 due to the choice of power 0.50. It gives us the approximation

$$N_{asy}^*(\tau) = \frac{\chi_{df}^2(\alpha)}{2L^2(\tau, \mathcal{M})}. \quad (\text{A.1})$$

Thus the asymptotic version of N^* is inversely proportional to the squared likelihood deviation.

Of course, our argument was somewhat specious: one cannot simultaneously let N go to infinity and solve for finite N . Regardless, N_{asy}^* provides an elementary and useful approximation to the index N^* , both its theoretical value (sampling under τ) and the estimator (sampling under \mathbf{d}).

A.2 Second approximation to N^* using noncentral chi-square distribution

One could construct more sophisticated asymptotic approximations of N^* . One method would be based on using “local alternatives”; that is, based on letting the alternatives approach the null, as $n \rightarrow \infty$, obtaining non-central chi-square approximations.

We imagine a sequence of true alternatives with $\tau_m = (1 - m^{-1/2})F + m^{-1/2}g$, where F is a model element and g is some fixed alternative not depending on m . Therefore the likelihood ratio test statistics $2mL^2(d_m, F) \rightarrow \chi_{df}^{\prime 2}(\delta^2)$ as $m \rightarrow \infty$ under τ_m , where $\delta^2 = X^2(g, F)$, the Pearson chi-squared distance, $\sum(g - F)^2/F$, and $\chi_{df}^{\prime 2}(\delta^2)$ is a noncentral chi-square distribution with degrees of freedom df and noncentrality parameter δ^2 (Agresti, 2002).

Therefore one can obtain the power as a function of m at a fixed g , based on the sequence of τ_m . However, what we want is the power at a particular τ , which we can approximate by inventing a different g for each m . At the targeted m ,

$$\tau = \tau_m = (1 - m^{-1/2})F + m^{-1/2}g_m,$$

implies

$$g_m = F + m^{1/2}(\tau - F).$$

This gives the corresponding noncentrality parameter

$$\delta^2 = \sum \frac{(g_m - F)^2}{F} = mX^2(\tau, F).$$

We then get the power at τ for large n being approximately

$$P \left\{ \chi_{df}^{\prime 2}(\delta^2) > \chi_{df}^2(\alpha) \right\}.$$

One can find the noncentrality parameter $(\delta^*)^2(df)$ such that

$$P \left\{ \chi_{df}^{\prime 2}((\delta^*)^2) > \chi_{df}^2(\alpha) \right\} = 0.5,$$

then N^* can be approximated by

$$N_{asy2}^* = \frac{(\delta^*)^2(df)}{X^2(\tau, F)}. \quad (\text{A.2})$$

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