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A MESSAGE FROM THE PRESIDENT

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In this issue of the *ISBA Bulletin* I have one procedural item and one research item to share/discuss with you.

On ISBA procedure, several months ago I initiated a discussion, largely by email, among the members of the ISBA Board and Executive about the desirability of beginning (in the near future) the process by which **ISBA Fellows** will be elected, and I also asked for comments on this issue from the general membership. Support for the idea that we should begin this process soon, rather than at some point in the future when ISBA has been around longer (we're 10 years old now) and/or when ISBA has more members (we have about 500 now), was strong enough that I'd like to encourage us to move ahead now toward the establishment of a process by which ISBA Fellows are regularly chosen. Jay Kadane, Chair of the ISBA Constitution and Bylaws Committee, tells me that—since the ISBA constitution does not address the question of Fellows—there's no legal bar to doing this, and he suggests that the right way forward is to create a new bylaw (which requires two consecutive votes by the Board) on the process of choosing ISBA Fellows. In the coming months the Board and Executive will be discussing a variety of procedural questions related to this issue (How many Fellows per year? How should they be nominated? How exactly will it be decided who is chosen as an ISBA Fellow in any given year? What sort of written criteria are appropriate to guide the nomination process? Will ISBA membership be required? If so, for how long before someone can become a Fellow?). I welcome your comments, by email or otherwise, on the best way forward.

On research, I'd also welcome your views on the role that *calibration* should play in Bayesian work. It seems clear to me that as a statistician using Bayesian methods and ideas to help people solve real-world problems, I need to pay attention to how often I get the right answer (at least in situations where the right answer becomes clear after a suitable passage of time), because if I consistently give my consulting clients bad advice I won't be invited back for new consulting jobs after awhile :-). In a paper published in *Statistical Science* back in 1988 I evoked the image of all of us eventually dying, going to Heaven, and being met by St. Peter at the pearly gates; he takes each of us aside and says "Let's see, you were a statistician, you're probably wondering about your personal scorecard. Remember back in 1992 when you were helping those people in that drug company to decide whether the new drug was better on average than the old one by a large enough amount to be practically relevant, and you concluded that they were about equally good? Well, you were wrong; the new drug *was* better by a wide enough margin to really make a difference for a lot of people." And so on. I don't know about you, but personally if I ever did have such an interview with St. Peter I'd like to have used methods in my lifetime that would lead to a good scorecard in Heaven, and this means to me as a Bayesian that in addition to trying to be *coherent* in my inferences I'd also like to be *well-calibrated*.

There is a curious literature on the subject of Bayesian calibration, which alternately seems to imply that this goal is on the one hand (a) trivial and on the other hand (b) impossible. In an interesting series of personal communications Dennis Lindley has offered a third description to me, that calibration is (c) irrelevant, because the most straightforward ways to formalize the idea require us to think about a series of replications of the phenomenon under study under identical conditions, and Dennis points out that precisely identical replications rarely, if ever, occur. Somehow I find this point of view unsatisfying, and it seems inescapable to me that my consulting clients would also be dissatisfied if I altogether ignored the issue of how often I get the right answer :-).

One possible way forward in situations where the truth of the final assertion is unknowable (or at least will not be known until your Heavenly interview, if any :-) is to pay close attention to *out-of-sample predictive calibration*: in other words, to (1) construct predictive distributions (based on your favorite

model structure(s)) for observables that were not used in the fitting process and (2) compare the actual observables with their predictive distributions in some meaningful way. Step (1) can be performed using any of several methods, including leave-one-out-style jackknifing of the data and/or the cross-validation approach of random partitioning of the data into subsets for modeling and validation purposes. The log scoring rule, perhaps first discussed by Jack Good and since used to good effect by many people, including Alan Gelfand and colleagues, is one possible basis for step (2), although it's not always easy to decide when the results with this metric are "good enough"; another possibility, when the predictive distributions are approximately Gaussian, is to construct predictive z–scores and compare them to the expected behavior of random draws from the standard normal distribution.

All of this can be given a plausible justification with Bayesian decision theory, but it also has a frequentist ring to it (after all, we're essentially using relative-frequency ideas to keep score). Given that the two main theories for how probability should be interpreted are like prize fighters who have been butting heads in the ring for 350 years, and given that both are still standing (although the Bayesian boxer seems rather less fatigued than his frequentist counterpart at present, and may be winning on points in recent decades :-), it may well be that our job as statistical scientists is not to choose one paradigm exclusively but to develop a fusion of the two approaches that emphasizes their strengths and de-emphasizes their weaknesses, and one possible fusion (given the discussion above) is to reason in a Bayesian way when carrying out our inferences and predictions and use relative-frequency ideas to evaluate their quality, through devices like out-of-sample predictive calibration. That way, we can be both internally consistent (through coherence considerations) and in good touch with the world (through calibration monitoring). Such a fusion would overcome the non-Bayesian objection, to the purely Bayesian paradigm ,that there is nothing in the Bayesian story by itself to prevent you from inserting very strong prior information that is retrospectively seen by you and others to have been out of step with reality, thereby obtaining a worse answer from the Bayesian machinery than you would have gotten if you had "relied only on the data." And such a fusion might go some distance toward encouraging our non-Bayesian colleagues to join us on the Bayesian bus :-), particularly when they see that Bayesian methods can lead to a better frequentist scorecard than non-Bayesian approaches.

As usual, your comments on these and other research issues would interest me greatly.

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ISBA/SBSS Archive for Abstracts

All authors of statistics papers and speakers giving conference presentations with substantial Bayesian content should consider submitting an abstract of the paper or talk to the ISBA/SBSS Bayesian Abstract Archive. Links to e-prints are encouraged. To submit an abstract, or to search existing abstracts by author, title, or keywords, follow the instructions at the abstract's web site,

`www.isds.duke.edu/isba-sbss/`

THE BAYESIAN CONTRIBUTIONS OF EDMOND LHOSTE

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The contributions of Edmond Lhoste are largely unknown outside of France, and even within that country are not well known. His main contributions were in two areas: (1) the development of distributions that represent little or no prior knowledge, and (2) a sophisticated posterior analysis for normal and binomial populations. His results are similar to those of Haldane (1948) and Jeffreys (1939, 1948, 1961), but they appeared much earlier in 1923. They represent a significant and unique contribution to Bayesian ideas in the early part of the 20-th century.

Introduction

Edmond Lhoste (1880-1948) studied at the Ecole Polytechnique and in 1923 wrote *Le Calcul des probabilités appliqué a l' artillerie, lois de probabilité a priori*. This was a collection of four articles comprising about 92 pages that appeared respectively in the May- August issues of the Revue d' Artillerie. For this review, Lhoste's results from the June and July issues are emphasized, because they contain his original ideas. In the second article, his contributions to Bayesian statistics were substantial and include the development of vague prior distributions that represent little or no knowledge about the mean and variance of a normal distribution and about the probability of success for the binomial distribution. His results were similar to those of Jeffreys (1939, 1948,1961) for the normal distribution and to those of Haldane (1948) for the binomial. In addition to his work with prior information, he developed sophisticated Bayesian inferential procedures for the normal and binomial distributions and applied them to problems in artillery. Many of his contributions were important and novel and have been commonly regarded to be discovered by others.

The Development of Vague Prior Information

In the June article, Lhoste begins with the declaration *Je n'ai pas l'intention 'exposer dans ces quelques articles la théorie classique du calcul des probabilités. Une telle étude serait superflue.* This article consists of developing vague prior distributions for: (1) the mean μ of a normal, (2) the standard deviation σ of a normal, and (3) the binomial parameter θ. For each case, the prior distribution for the relevant parameter is developed, using techniques based on little or no prior knowledge about the parameter. For the mean of a normal population, Lhoste proposed a constant prior density based on something resembling the principle of precise measurement. See Barnett (1999, page 279) for a discussion of this principle. The likelihood function and the prior density are considered density functions of the unknown mean μ, and the ratio of the variance of the likelihood function to the variance of the prior density is considered to be small (because the variance of the prior density is considered to be large relative to that of the likelihood function). This in turn implies the graph of the prior density is flat relative to that of the likelihood function, or as he writes " In other words, if one admits that the ratio is small, the form of the prior function doesn't have influence on the final result and we can practically replace the prior by a constant." With regard to his choice of the prior for the standard deviation , Lhoste reasons that the prior density for σ should be the same as that for its reciprocal, and he lets

$$f(\sigma) \propto 1/\sigma \text{ for } \sigma > 0 \tag{1}$$

be the prior density for the standard deviation. Thus our lack knowledge about should be the same as our lack of knowledge about $1/\sigma$. This is similar to Jeffreys' (1961, page 119) invariance principle that states prior information about σ should be the same as that for any power of σ. To paraphrase Lhoste, "nothing distinguishes, a priori, $1/\sigma$ from σ ; if is indeterminate between two values, then $1/\sigma$ will be equally and in the same fashion be indeterminate between the corresponding values of $1/\sigma$ ". To determine the prior density for the binomial parameter θ , Lhoste used as the prior induced by the prior density for the odds $\gamma = \theta/(1-\theta)$, and since the odds and σ for the normal distribution have the same domain, the positive numbers, the same prior density for the odds is assigned as that for σ of a normal distribution, which leads to

$$f(\theta) \propto 1/[\theta(1-\theta)] \tag{2}$$

as the prior density for θ. As mentioned earlier, these determinations of prior information resemble those used by Jeffreys (1939, 1948,1961) for the mean and variance of a normal population and to Haldane (1948) for the binomial parameter. Haldane, however chose this prior because the posterior mean would be the sample proportion, which has 'good' sampling properties.

Bayesian Inferences

In his July article, Lhoste developed Bayesian inferences for the parameters of normal and binomial populations, using the vague priors he developed in the first article. Several cases are considered: (1) a normal population with standard deviation σ known, but unknown mean μ (*la valeur probable*), the mean known, but standard deviation unknown, (3) both mean and variance unknown, and (4) the unknown binomial parameter . His results can be regarded as the standard posterior inferences found in modern sources such as Box and Tiao (1973). For example, using his vague prior distribution (2.1) for both parameters of the normal, Lhoste derives the marginal posterior distribution of the mean as a t-distribution with n-1 degrees of freedom, and that for the variance as an inverse gamma. For the binomial case using his vague prior density (2.2), he derives the posterior distribution of θ as a beta with parameters x and $n - x$, where x is the number of successes in n trials.

Summary

To summarize his work, Lhoste's contributions fall mainly into two main areas: (1) a justification for the choice of prior distributions that convey 'little' information about the parameters of the model, and (2) a modern posterior analysis for the normal and binomial distributions. Our study of Lhoste gives us a better understanding of the history of Bayesian ideas in the early 20th century. Dale's (1991) history of inverse probability can be ammended by adding Lhoste's name to the list of innovative Bayesian thinkers. We now have a better understanding of the significance and originality of Lhoste' impact on the field.

Acknowledgement

I wish to thank Monsieur Maurice Dumas who initiated this study by contacting me some time ago in making me aware of the many contributions of Edmond Lhoste. The author is indebted to Ana Langevin Broemeling for the English translation of *Le Calcul des probabilités applique a l'artillerie, lois de probabilité a priori* by Edmond Lhoste. Also, I would like to thank Christian Robert, and Denis Bayart, who provided me with valuable information about Maurice Dumas and his articles that referenced Lhoste.

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THOUGHTS ON BIOSTATISTICS

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We see the Students' Corner as a service for Ph.D. students involved in Bayesian statistics, giving them the opportunity to talk as well as to listen. The last issue was about talking and, therefore, featured many abstracts of recent Ph.D. thesis. This issue is about listening, since we feature what we believe to be a truly interesting interview with Professor Gianpaolo Scalia Tomba from the University of Tor Vergata, Rome. He is definitely an expert in biostatistics and so is the "right" person with whom to talk about a well established application field of Statistics which is of great interest nowadays due to the recent developments in genomics.

Professor Scalia Tomba obtained his degree in Mathematics and a Ph.D. in Mathematical Statistics from the Stockholm University in Sweden. He then spent a few years as a medical statistician at the Karolinska Institute, which resides in Stockholm and is one of Europe's largest medical universities. In the past ten years, he has been working within the Italian academia as an associate professor and enjoys an on-going collaboration with the Italian National Institute of Health on many projects such as "reconstructing the HIV epidemic in Italy", "counting individuals in semi-anonymous records with replicates", "vaccination strategies against infectious childhood diseases" and "generation of alarm signals based on spontaneous notifications of adverse reactions to drugs".

Dear Professor Scalia Tomba, first of all thank you for being here with us. As an appetizer, can you tell us about your experience in Sweden before and after having obtained your Ph.D.? Sweden was, and still is, a good place to be a student. While I was studying for my Ph.D., I was able to do a lot of teaching assistance and statistical consultancy work; after I got my Ph.D., I was able to go on doing more or less the same things, but with a better salary.

When and why did you decide to work in biostatistics? Obtaining a Ph.D. is like learning to drive a car: it's not really useful if you don't have somewhere you want to go. I decided to stay within the "world of research" and to pursue mainly biostatistical problems because I felt that working with problems with a "real world" connection was rewarding ("I'm doing something useful to others...") but also theoretically interesting, sometimes. The focus on biostatistics, rather than on engineering or economics applications, just happened. I believe Chance to be an important actor in human decisions.

On which projects did you work and for how long? Because of the many years spent doing statistical consulting, I have worked on many strange projects, like "optimizing time tables and routes in urban bus lines", "optimal sequential testing of batches of wine bottles", "new winning combinations in Bingo", "statistics of accidents incurred by emergency vehicles" or "road accidents involving elks", but my main areas have been models and statistical methods for infectious diseases (almost two decades, now, and still going on) and, for some years, analysis of mutation data in animals and humans.

What about interacting with non-statistical scientists? It requires an effort. Having studied a subject for a long time tends to shape your way of thinking and also to make you consider certain ideas and notions as self-evident; unfortunately, people having studied different areas have different world views and "self-evident truths" and, therefore, essentially do not understand each other. The remedy is, I believe, to try to be "as a child" and ask, as soon as you suspect you do not understand; but, as with children, this strategy can be tiresome and requires that you find a counterpart that accepts it. However, I believe it to be "bad statistics" to just accept somebody's request to perform certain statistical tests or other operations on a data set, without trying to understand what the underlying problem is about.

Is there enough room in biostatistics for methodological research, or standard techniques are preferred? My personal experience is that interesting research problems can be found, extracted and purified from "applied problems"; how easy this is depends on the researcher's "statistical culture" since noticing that the problem under hand contains interesting non-standard features that could lead to something new depends on knowing what has been done before. The standard techniques must be studied and mastered, because they are commonly used by others (and probably by yourself) and because they are the starting point for new points of view and results.

What has been, in your opinion, the main contribution of Bayesians to biostatistics? The same as to Statistics in general. In recent times, to slowly erode the Neyman-Pearson paradigm in its pure form and, by simply existing, to lead the way to some future synthesis. In general, I do not believe in "schools": a very important Italian proverb states "Ascolta il consiglio di tutti, ma poi fai di testa tua" ("Listen to everyone's advice, then do it your own way"). Statistics should be about rational methods of interpreting observations in order to answer well defined questions, and I believe that it is as difficult to find a final format for Statistics as it is to decide what all the components of the above definition really mean.

What kind of background, interests and personal qualities should have someone willing to work in biostatistics? Nothing special, just the general requisites for good science: whatever work you do, try to do it well; do not passively recite the old masters, try to do something yourself; in general, try to understand what you are doing.

Thank you for your stimulating answers. Is there anything in particular you would like to say to young Ph.D. students? Yes...

We will all have to contact directly Professor Scalia Tomba in order to find out what he would like us to know! (Since our call for suggestions went unanswered, this will possibly give us another opportunity to count the number of our readers...).

BAYES FACTORS

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We provide some references on Bayes factors which is used as measure of evidence in hypotheses testing and model selection. Clearly, there is a vast amount of literature on Bayes factors, its application, and on the associated theoretical issues including foundational issues. Unfortunately, it is beyond our scope to give a thorough list of references, and inevitably and unwittingly, we must have missed some important references. Our hope is that the listed review articles would cover such articles, so a reader less familiar with the topic would still not miss such an article.

We have divided the references into three parts, one mainly consisting of general review articles, another consisting of articles where improper priors are used, and the third consisting of articles dealing with computation of Bayes factors.

Introduction and Foundation

1. An excellent article with comprehensive introduction and review of Bayes factors, including interpretation, details on computing, connections to BIC and applications, is available in: "Bayes Factors" by Robert E. Kass and Adrian E. Raftery, J.Amer. Statist. Assoc.(1995), 90, 773-795. This article also contains an extensive bibliography on the use of Bayes factors.

2. Use of Bayes factors with improper priors was a thorny issue due to the indeterminacy resulting from the improper priors. When improper priors are used the Bayes factors are only determined up to a constant (a ratio of two constants). In the past decade, there has been a renewed interest with substantial developments on this topic. An expellant reference that provides reviews of the earlier work as well as the more recent developments with critical comparison of the available approaches is available in "Objective Bayesian methods for model selection: introduction and comparison" [with discussion]. Berger, J. and Pericchi, L. (2001). In 'Model Selection' (P.Lahiri, editor), Institute of Mathematical Statistics Lecture Notes – Monograph Series volume 38.

3. The following article cautions that the Bayes Factor has certain coherency issue in the sense it is not always monotone in the alternative hypotheses, and shows that the posterior probability, which also takes account of prior odds, is coherent. "Bayes Factors: What they are and what they are not", Lavine, M Schervish, M (1999) The American Statistician 53, 119-122.

Bayes factors when the priors are improper

This is an important topic, especially in a multidimensional setup, as proper priors are not easy to elicit, and the use of objective improper priors are therefore desirable. Here we list articles which collectively cover the different approaches to deal with the issue of indeterminacy of Bayes factors when improper priors are used.

1. The following articles uses the idea of imaginary samples to choose some suitable values for the proportionality constants associated with the improper priors. "Bayes factors and choice criteria for linear models", Smith A F M and Spieglehalter D J. (1980), Journal of the Royal Statistical Society, Series B 42 213-220.

The following articles introduce a novel approach, now known as intrinsic Bayes approach to deal with non-uniqueness of the Bayes factor. This approach uses a minimum training sample to update an improper prior to a proper posterior to be used as an "interim" proper prior to calculate the Bayes factor using the rest of the sample. These articles and the references therein cover by far the most thorough treatment of the subject of improper priors and their use with Bayes factors. They investigate the intrinsic Bayes approach from many different angles and convincingly argue for its merits in this conceptually complex area of model selection. In particular, these articles introduce the intrinsic Bayes factor methodology, and illustrate it in many challenging examples. They also introduce the important notion of intrinsic priors as a means giving a Bayesian validation for the intrinsic Bayes factors as they are not fully Bayes.

2. "The intrinsic Bayes factor for model selection and prediction", Berger, J. and Pericchi, L. (1996), J.Amer. Statist. Assoc. , 91, 109-122.

3. "Default Bayes factors for non-nested hypothesis testing", Berger, J. and Mortera, J. (1999), 'J. Amer. Statist. Assoc., 94, 542-554.

4. "Accurate and stable Bayesian model selection: the median intrinsic Bayes factor", Berger, J. and Pericchi, L. (1998), Sankhya B (60), 1-18.

In the following article the authors develop an interesting new notion of developing proper objective priors for use in model selection and testing hypotheses.

5. "Expected posterior prior distributions for model selection", Perez, J.M. and Berger, J. (2002) Biometrika 87.

The following two articles introduce another approach, Fractional Bayes factors, to deal with the issue of non-determinacy of Bayes factors when using improper priors. This approach, in some sense, is equivalent to using a fraction of the likelihood as "prior" information to update the posterior and then use the whole likelihood to calculate the Bayes factor. The first article introduces the approach and its merits. The second one offers a critical evaluation of the approach and compares and contrasts the approach with other similar approaches.

6. "Fractional Bayes factors for model comparison", O'Hagan, A. (1995). J. Roy. Statist. Soc. Ser. B 57 99-138

7. "Properties of intrinsic and fractional Bayes factors", O'Hagan, A. (1997), Test 6, 101-118.

A third approach to Bayes factors in the absence of subjective proper priors is given in the following two papers. Here, the authors find conditions or prior distributions under which the Schwartz approximation is asymptotically a reasonable approximation to Bayes factor, when comparing nested models. They find unit information priors as reasonable reference prior in testing nested hypotheses.

8. "A Reference Bayesian Test for Nested Hypotheses And its Relationship to the Schwarz Criterion", Kass, R. E. and Wasserman, L.A. (1995) J.Amer. Statist. Assoc., 90 p928-934.

9. "Bayes factors for variance components model", Pauler, D.K., Wakefield, J.C., Kass, R.E.(1999), J.Amer. Statist. Assoc., 94, 1242-1253.

Computing the Bayes factor

Methods of computing Bayes factor received much attention after MCMC methods became a common tool for simulating from posterior distributions. Here we list some of the articles that deal with the issue, while references for other articles may be found in the listed articles.

The following two articles use the importance sampling technique to calculate the Bayes factors, and suggest choices for the importance function to improve the accuracy.

1. "Approximate Bayesian inference by the weighted likelihood bootstrap", Newton, M. A. & Rafter, A. E. (1994) , Journal of the Royal Statistical Society, Series B 3, 348.

2. "Bayesian model choice: asymptotics and exact calculations", Gelfand, A. and Dey, D.(1994), Journal of Royal Statistical Society, series B, 56, 501-514.

The following articles make use of the MCMC outputs to calculate the marginal likelihood and the Bayes factors.

3. This articles uses a simulated version of Laplace's method along with bridge sampling method.
"Computing Bayes factors by combining simulation and asymptotic approximations", DiCicio, T. Kass, R, Raftery, A, and Wasserman, L., 1997, Journal of the American Statistical Association, 92 p903-915.

4. This article uses a model indicator into the list of unknown parameters in the models being compared to facilitate the use of MCMC draws in calculating the marginal likelihood. "Bayesian Model Choice via Markov Chain Monte Carlo Methods", Carlin, B. and and Chib, S. (1995), Journal of the Royal Statistical Society B, 57, 473-484.

The following two articles directly uses the MCMC draws from a posterior distribution to calculate the marginal likelihood.

5. "Marginal Likelihood From the Gibbs Output" Chib, S., Journal of the American Statistical Association, (1995), 90, 1313-1321.
6. "Marginal Likelihood from the Metropolis-Hastings Output" by Chib, S. and Jeliazkov, I. Journal of the American Statistical Association, (2001), 96, 270-281.

VARIETY IS IN THE EYE OF THE BEHOLDER, ONE PAIR AT A TIME OR MARGINALLY?

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Dear ISBA readers:

One of the main reasons I made the switch to be a statistician in a Marketing Department, was the hope that on a daily basis I could work on problems that have "real teeth". A perfect example of such a problem presented itself in the most unlikely way, and it's the story I'd like to share in this article.

One day my department chair and friend, Steve Hoch, came into my office with a seemingly innocuous question: "How do you measure the variety of an assortment?" Hmmmmm, I said, tell me more. Specifically what Steve and his co-author Brian Wansink (soon to be my co-authors) wanted to know was imagine that you have an assortment of multivariate objects each made up entirely of categorical features (e.g. name, shape, and color). Can you develop a measure for how much variety is in this assortment? After many thoughts, and many kluge-like solutions, I stumbled upon the most seemingly little and known but barely known facts that the sample variance (the "variety" of an assortment of scalars), that we all love, can be computed as a sum of pairwise distances between the outcomes (Yule, 1900). What this got me thinking about was ok, now I see the forest, but here are the trees:

1. How do I measure the distance between two categorical objects?
2. How do I "sum these up" over all the pairs to come up with a scalar measure of assortment variety?
3. How do I turn this into an estimable model?

The answer to question (1) came to me from a project I had worked on while a graduate student at Harvard statistics. Back then, I was working on a project having to do with the Massachusettes Lottery, and I had to figure out the "distance" between any two lottery tickets, as part of my solution. As I was thinking about the distance between multivariate categorical objects, I realized that this problem was structurally identical to the lottery ticket problem, and in that case, I measured the distance between two lottery tickets by the Hamming distance (Sloane and MacWilliams, 1977), the number of features in which the two objects do not match. So I adopted this framework to our current problem. Note, that in the assortment problem, I had to use a generalized hamming distance in which each attribute (name, shape, color) may be allowed a different weight (other than unity in the simple hamming distance).

The answer to question (2) had a simple strawman; once you compute the distance between any pair of objects, simply add them up across all $\binom{N}{2}$ pairs. The problem with this approach, I felt, was that it ignored spatial proximity in that some objects are adjacent to each other (and hence more likely to be compared), and others are on the opposite side of the assortment (imagine a store shelf with products on opposite ends). Therefore, I extended the simple sum over pairs approach to allow for differential weighting based on the "distance" between the objects.

Finally, the way in which this became an estimable model, was the coup-de-grâçe. As it turns out, all you need to compute this metric is to take your set of N objects, each with M categorical features, and count the number of pairs that differ by 0 attributes, 1 attribute, 2 attributes, $\ldots, M$ attributes. Then you simply regress the dependent variable, which in our case was 1-10 Likert scale stated assortment variety, on the number of pairs that differ by 0,1,2,$\ldots, M-1$ attributes (leave one out to identify the model). Thus, this metric which started out as a big forest, turned into a regression problem with interesting regressors.

It is also interesting to note, and see the work of Tversky for earlier references on this, that it is important to look at these regression coefficients as they represent the psychological impact of a distance of size 0, size 1, size 2, ..., size M on the variety. What we found fascinating is that there are diminishing marginal turns to distinctions; that is a distance of size 2 between two objects adds less than twice a distance of size 1. In fact, the square root function fits particularly well.

All is well that ends well, right? Well not exactly. This paper appeared in one of the top quantitative Marketing journals, Marketing Science, in 1999, and seemingly was well-placed in the literature. About a year later, I was asked to be a reviewer on a Marketing Science paper that was a follow-up to our paper that utilized, what was claimed to be, a superior empirical approach to measuring the variety of an assortment. That is, compute the marginal entropy and pairwise associations, not at the object level, but at the margins. This paper, by Rik Pieters and his student Erika Van Herpen, collected new data and fit better than our model (albeit they used a simple version of our model where every object had the same importance, and relative distance between objects was ignored).

Nonetheless, it made me re-think the problem. Finally, after prodding by Steve, we reconciled the issue in the following ways. (1) We fit the Pieters and van Herpen model on our data, and it did not fit better or worse than others. (2) I went back to the combinatoric drawing board and figured out a unifying framework for both models. I am happy to say that both papers, Pieters and Van Herpen, and our new paper Variety Perceptions Redux, are appearing in the next issue of Marketing Science.

In summary, you never know where a good problem is going to come from. I have never asked Steve or Brian this but I am sure this was one of those questions Steve wanted a five minute answer to. However, the more I thought about it, the more I realized we had something here. Secondly, one can only hope that smart colleagues at other schools (in our case Pieters and Van Halpern) spend the time reading your work and questioning its foundation. Finally, despite my desire to write a scathing retort of their findings, my wise co-authors instead suggested, why don't we all be friends and just find something unifying that we can all live with. It led to a much improved response.

So is the variety of an assortment a pairwise process or something on the margin? The answer is it depends. Some people are drill-down thinkers and do something akin to pairwise processing. Others are more holistic thinkers and purely scan the assortment. The wonderful thing is that we have just opened up an area for future empirical work, and I hope others continue to look at the variety assortment problem as one that is fundamental in Marketing. From a Bayesian perspective, the minute we state that "variety is in the eye of the beholder" and that "it depends" the Bayesian's mouth should begin to water as this tastes of heterogeneity, and understanding when one model is likely to hold over another. Certainly an area in which Bayesian model exploration would have a lot to say.

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A REVIEW OF BAYESIAN ANALYSIS SOFTWARE PRESENTED IN THE LAST THREE YEARS IN THE ISBA BULLETIN

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In looking back over the last three years of ISBA bulletins, we noticed that there have been a large number of interesting software reviews. We felt that it might be helpful to write a brief article which summarizes the content of these reviews, and which can serve as a reference point for the reviews which have been published in the ISBA bulletin so far. Of course, many other contributions and updates have been made that we will fail to mention here; our immediate purpose is simply to summarize the information featured in past ISBA bulletins and make it easily available in one issue.

Statistical software development is a growing area of interest and research. The featured software includes routines/packages which were written for both highly specialized and more general purpose applications. They also differ in terms of "prerequisite" software needed to use them, the required operating system, and the degree of programming sophistication required of the user. One feature that all of these routines/packages have in common is that all of them may be used without cost in an educational or a research environment.

One article which may be of particular interest to those researchers just beginning their journeys into MCMC is that published about a year ago in the ISBA Bulletin (Vol. 8, No. 3). Carlin, Holmes, Rue, Smith, and Wilson discussed how to perform MCMC simulations using lower level programming languages (Fortran, C, C++) and using packages such as Matlab/Octave, Splus/R, and WinBugs.

This section is intended to offer the reader the latest innovations in software for Bayesian analysis, and, to this purpose, we hope that this article has proven useful as a baseline. Let us take this opportunity to invite those of you who have developed or utilized new software tools to share your experiences with all of us. Of course, the same applies to those of you who are familiar with updates and additions to the software mentioned in the chart above. We encourage you to contact editor Hedibert Lopes (hedibert@im.ufrj.br) or either of the writers of this article.

Software Description	*Authors* Reference	Execution software	Platform	generic/ specific	Review Date
R A good and flexible statistics package	*R Project authors* www.r-project.org	operating system	any	generic	03/01
CADGED Cluster Analysis of Gene Expression Dynamics	*Sebastiani, Ramoni* www.genomethods.org/ caged	operating system	windows	specific	06/02
[*B*/*D*] Bayes Linear Programming Language	*Wooff, Goldstein* fourier.dur.ac.uk/ stats/bd/	operating system	DOS unix/windows	specific	12/99
JavaBayes *System Networks* Manipulates graphical statistical models	*Gagliardi* www.cs.cmu.edu/ ∼javabayes	Web Browser	unix/windows	generic	12/00
BCal MCMC for Radiocarbon Calibration	*Christen, James, Buck* c.e.buck@sheffield.ac.uk	Web Browser	unix/windows	specific	06/00
BRCAPRO A model for Genetic Counseling of woman at high risk of breast and ovarian cancer	*Parmigiani et. al.* *Berry et. al.* biosun01.biostat. jhsph.edu/∼gparmigi/ brcapro.html/	C compiler	unix	specific	12/01
Flexible *Bayesian Modeling* for regression and classification	*Neal* www.cs.toronto.edu/ ∼radford/ fbm.software.html	C compiler	unix	specific	03/00
A library for *Regenerative* *MCMC Simulation*	*Brockwell* lib.stat.cmu.edu/ general	C/C++ compiler	unix/windows	specific	03/02
Bayesian Model *Averaging*	*Raftery, Volinsky, Hoeting* www.research.att.com/ ∼volinsky/bma.html	S-Plus	unix/windows	generic	06/99
BOA Convergence diagnostics	*Smith* www.public-health. uiowa.edu/boa	S-Plus/R	unix/windows	generic	09/99
BayesX Estimation of semiparametric regression	*Lang, Brezger* www.stat.uni-muenchen .de/∼lang/bayesx/ bayesx.html	S-Plus	windows	specific	06/01
TVAR Time-Varying Autoregressive models	*Prado, West* www.isds.duke.edu/ ∼ mw/tvar.html	Matlab or Fortran 90 compiler S-Plus	and unix/windows	specific	03/99
BVS Variable selection in multivariate regression	*Vannucci* stat.tamu.edu/ ∼mvannucci/ webpages/codes.html	Matlab	unix/windows	specific	09/00

NEWS FROM THE WORLD

by Gabriel Huerta
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✻ *denotes an ISBA activity*

➤ Events

Erratum

The event **Combining Probability and Logic**, that appeared on the June 2002 issue of the bulletin, is not concerned with Gnedenko. Moreover, the url given was also incorrect. The correct url is http://www.kcl.ac.uk/progic. Thanks very much, Jon Williamson.

✻ **Third Workshop on Bayesian Inference in Stochastic Processes (BISP03)** *15-17 May 2003, Hotel Sol Galza, La Manga, Spain*

The workshop follows the previous ones in Madrid (1998) and Varenna (2001) and is aimed to encourage discussion and promote further research in the field of Bayesian inference in stochastic processes and on the use of stochastic processes for Bayesian inference.

The workshop is endorsed by ISBA (International Society for Bayesian Analysis) and funded by Universidad de Murcia and Universidad Rey Juan Carlos (Madrid). The cooperation of other institutions will be announced shortly on the conference web page. People interested in presenting a paper at the conference are kindly invited to send a DETAILED abstract by DECEMBER, 15th, 2002, to Juan Antonio Cano Sanchez (bisp03@upct.es) specifying the preferred format for their presentation (oral/poster).
www.upct.es/bisp03

ISIPTA '03 3rd International Symposium on Imprecise Probabilities and Their Applications *July 14-17, 2003,Lugano, Switzerland*

The ISIPTA meetings are one of the primary international forums to present and discuss new results on the theory and applications of imprecise probabilities. Imprecise probability has a wide scope, being a generic term for the many mathematical or statistical models which measure chance or uncertainty without sharp numerical probabilities. These models include belief functions, Choquet capacities, comparative probability orderings, convex sets of probability measures, fuzzy measures, interval-valued probabilities, possibility measures, plausibility measures, and upper and lower expectations or previsions. Imprecise probability models are needed in inference problems where the relevant information is scarce, vague or conflicting, and in decision problems where preferences may also be incomplete. Although the third symposium will be open to contributions on all aspects of imprecise probability, three main themes will be emphasised: inference, algorithms and computational complexity, real applications.
www.sipta.org/ isipta03/themes.html

✻ **First IMS-ISBA Joint Meeting** *July 24-27, 2003, San Juan, Puerto Rico, USA*

The 1st joint statistical meeting of IMS (Institute of Mathematical Statistics) and ISBA (International Society for Bayesian Analysis) will be held in Isla Verde (San Juan, Puerto Rico, USA). The meeting will evolve around three main topics of interest to both IMS and ISBA members: Causal-Graphical Modeling; Spatial Statistics, and Analysis of Extremes. The format of the meeting includes overview lectures in the topics, invited talks, and poster sessions.
www.cnnet.clu.edu/math/IMS-ISBA-PR2003/

✻ **Practical Bayesian Statistics 5** *July 28-31, 2003, Open University in Milton Keynes, UK*

The 5th Practical Bayesian Statistics conference will be hosted by the Department of Statistics at the Open University in Milton Keynes, UK.

The meeting is co-sponsored by ISBA (International Society for Bayesian Analysis) and by The Faculty of Mathematics and Computing at the Open University.

Over the last couple of decades, Bayesian statistics has been experiencing very rapid growth, and this is particularly marked in the number and sophistication of the applications being reported. The Royal Statistical Society, and the Institute of Statisticians before it, organised an extremely successful series of conferences on the theme of "Practical Bayesian Statistics". Practical Bayesian Statistics 5 will continue this series and is intended as a forum for the presentation of recent developments in practical Bayesian statistics. At this conference we would like to give particular emphasis to practical Bayesian work which is not merely "illustrative" of general models and methods, but also follows through the practical aspects of the task, for example, in terms of using genuine prior information, thorough model criticism and technology transfer.

The conference will include both invited and contributed talks, and poster sessions. Deadline for submission of abstracts: 1 February 2003.
`http://mcs.open.ac.uk/Statistics/PBS5/`

➤ Call for Papers

Applied Stochastic Models in Business and Industry *Call for papers for a special issue on "Innovative statistical models in the European businesses and industries",* promoted by the ENBIS (European Network for Business and Industrial Statistics) Working Group on General Statistical Modelling. Authors can submit their manuscripts to one of the Guest Editors: Wolfgang Polasek (`polasek@ihs.ac.at`); Fabrizio Ruggeri (`fabrizio@iami.mi.cnr.it`). Deadlines: November, 15, 2002: Abstract to the Guest Editors, December, 31, 2002: Paper to the Guest Editors.
`www.nr.no/enbis`

➤ Miscellanea

Journal of the Iranian Statistical Society

The Journal of the Iranian Statistical Society (JIRSS) is a new English-language periodical for peer reviewed original research work in Probability, Mathematical Statistics, and Statistical Methods. Its premier issue appeared in October, 2002. The goal of this journal is to provide a first-rate international forum for significant theoretical developments and innovative applications of contemporary probability and statistics.

The international editorial board of JIRSS invites the submission of manuscripts. Of special interest are articles in: applied probability, asymptotic theory, Bayesian statistics, biostatistics & survival analysis, computational statistics, design of experiments, distribution theory, environmental statistics, inference for stochastic processes, linear & nonlinear models, longitudinal data analysis, multivariate analysis, nonparametric & semi parametric inference, probability models, reliability & quality control, spatial statistics, statistical methods in bioinformatics, statistical decision theory, survey sampling, stochastic processes, and time series analysis. This list of topics is not intended to be exhaustive.

Authors can submit their manuscripts to one of the Associate Editors listed on the JIRSS website. Alternatively, papers can be directly submitted to:

Professor Ahmad Parsian,
Editor, Journal of the Iranian Statistical Society,
School of Mathematics,
Isfahan University of Technology,
Isfahan 84156, Iran.
E-mail: jirss@cc.iut.ac.ir

Guidelines for submission of manuscripts are available at the JIRSS website.
`http://www.iss.ir/jirss`

Call for Research Student Applications

The 3-day workshop on the "Statistical Analysis of Gene expression data", 11-14 July 2003, Wye College Conference Center, Kent, UK. Organised by Sylvia Richardson (Imperial College) and Phil Brown (University of Kent)

This workshop, sponsored by EPSRC and the Royal Statistical Society (RSS), has a limited number of places to support research students, funded by the RSS. The workshop aims to foster statistical research at the interface between new methodological developments and the biological and experimental context. Some of the topics to be covered are: – Data transformation and normalisation, – Differential gene expression, – Profile clustering and pattern recognition, – Discrimination and clinical profiling, – Experimental design, Implementation and algorithmic issues.

Around fifteen leading international researchers have already agreed to participate and we expect to invite around 35-40 participants in total, plus around 5 PhD students. Applications from research students are now invited for these places.

If you are interested in participating in such a workshop, could you send a one-page application by email stating: (i) your research interests and experience; (ii) whether you would be willing to present a poster and on what topic, and in addition (iii) a separate emailed reference is requested from your supervisor justifying your attendance to Phil Brown (Philip.J.Brown@ukc.ac.uk), cc Sylvia Richardson (sylvia.richardson@ic.ac.uk), as soon as possible and in any case before the 15-12-02.

We will finalise the invitation list during January 2003. If your application is successful, your accommodation and meal costs will be fully covered for the duration of the workshop. If travel costs are an insuperable deterrent to your attendance please also highlight this in your application and we will see if other funds can be found.

The workshop will be held at the conference center of Imperial College situated in Wye (between Ashford and Canterbury, Kent), with easy access from London and on the Eurostar high speed rail route to Belgium from nearby Ashford. The workshop will stop at lunchtime on the 14th July so that participants of the RSS conference on "Statistical genetics and Bioinformatics" in Limburgs can easily travel there.

Information on the workshop can be found at `www.med.ic.ac.uk/divisions/60/BGX/bgx/july2003workshop.html`

SUGGESTIONS

PLEASE, FEEL COMPLETELY FREE TO SEND US SUGGESTIONS THAT MIGHT IMPROVE THE QUALITY OF THE BULLETIN

`hedibert@im.ufrj.br`