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THE ISBA NEWSLETTER

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Following worldwide enthusiasm, positive votes at Bayesian Conferences in Rio De Janiero, St. Paul, Valencia, and Cambridge, and letters of support from Brazil, the United Kingdom, Spain, Japan, China, Taiwan, Hong Kong, Switzerland, Poland, New Zealand, the United States, Israel, South Africa, India, Canada, Mexico, Belgium, Italy, and Holland, the acting board announces the formation of ISBA. This world organization seeks to benefit international society by the advancement of Bayesian statistics, science, and analysis in the natural, biological, and social sciences, engineering, industry, medicine, law, government and education, and by the development and interface of inferential and decision-making procedures in all areas.

The current acting members of the temporary board are Arnold Zellner (U. of Chicago) and Jose Bernardo (Generalidad Valenciana Presidencia), Co-presidents, Michel Mouchart (Universite Catholique de Louvain), Secretary; Gordon Kaufmann (MIT), Treasurer; Thomas Leonard (U. of Wisconsin-Madison), Newsletter Editor, and Constitutional Advisor; James Press (UC Riverside), Intersocietal Representative; Dennis Lindley (Somerset, England), Hajime Wago, (U. of Toyama), Duncan Fong (Penn State U.), Jacques Dreze (U. of Louvain), Yoel Haitovsky (Hebrew U. of Jerusalem), John Hsu (UC Santa Barbara), Dale Poirier (U. of Toronto), Herman Van Dijk (Erasmus U. Rotterdam), Enrique de Alba (ITAM, Mexico City), Luis Perichi (Univ. Simon Bolivar, Caracas), and J. K. Ghosh (Indian Statistical Institute), International Advisors; and Seymour Geisser (U. of Minnesota), Chairman of the Council of Sciences. A more permanent board will be elected by postal ballot prior to the first annual meeting. Until then, all volunteers are invited to help the Board; a variety of suggestions for regional activities have already been received.

The initial services of ISBA will include a quarterly newsletter and bulletin, including news, articles, and research abstracts, the development of an international E-mail network, a Scientific Council, reduced rates for the Annual Conference, intersocietal activities and local sections, accessibility to groups with interests in particular areas of science, a consultants service, a joint research facilitation program, and accessibility to international conferences around the world. It is however, envisioned that many more objectives and services will be developed, by the charter membership, a ten person democratically elected Constitutional Committee, and the first democratically elected Board.

ANNOUNCEMENTS

- There are currently about 200 Charter Members of ISBA. The first *Annual Meeting* of the ISBA will take place on August 7, 1993, in San Francisco, California, during the first *International Meeting* of ISBA, which will be held August 6-7, 1993, prior to the Joint Statistical Meetings, August 8-13, 1993.

- The International Meeting, Friday 6, August - Saturday 7, August, 1993 will also include a cocktail party, a banquet, and presentation of papers. Local arrangements will be announced later. For further information please contact Professor Arnold Zellner, ISBA Conference Organizer, Graduate School of Business, University of Chicago, 1101 E. 58th St., Chicago, IL 60637, U.S.A.

- Treasurer Gordon Kaufmann has arranged incorporation of ISBA as a non-profit corporation.

- Call for papers: If you are interested in presenting a paper at the first International Meeting, then please send an abstract to Professor Robert E. McCulloch, ISBA Conference Program Chair, Graduate School of Business, University of Chicago, 1101 E. 58th St., Chicago, IL 60637, U.S.A.

- Conference proceedings: It is planned to publish the papers and their discussions in a Conference Volume.

- Council of Sciences: If you are interested in serving on the Council of Sciences, then please contact Professor Seymour Geisser, Chairman, ISBA Council of Sciences, School of Statistics, University of Minnesota, 206 Church St., SE, 270 Vincent Hall, Minneapolis, MI 55455, U.S.A.

- Secretary Michel Mouchart has circulated the temporary board concerning his plans for the formal mechanisms and workings of the Society. Please send all suggestions to Professor Michel Mouchart, CORE, University of Louvain, Voie du Roman Pays 34, B-1348 Louvain-LaNeuve, Belgium.

- All people interested in Bayesian analysis and its scientific and societal implications, though not necessarily of Bayesian ideology, are invited to become Charter Members of ISBA. Please complete attached form and return with your annual membership fee, by 1st. February, 1993, to Professor Gordon M. Kaufmann, School of Management, Room 53 - 375, MIT, Cambridge, MA 02139, U.S.A. The fee is $25 U.S. dollars, or equivalent. Special discounts may be available upon request, for example from countries with low average incomes.

NAME:

AFFILIATION:

MAILING ADDRESS: APDO. POST.

E-MAIL ADDRESS (If available):

I wish to become a Charter Member of ISBA, and enclose my first annual membership fee (valid until 1st. February, 1994) of (please check):

$25 US Dollars: XX Foreign Currency Equivalent (please state amount):______
A requested reduced amount of: ______ Please state reason for request:

I wish to be nominated for the Constitutional Committee (YES/NO).
XX

I wish to be nominated for the democratically elected Board (YES/NO).
XX

I am particularly interested in collaborations and activities in the following areas:
SIGNATURE:

(Please mail to Professor Gordon Kaufmann)

From the Acting Newsletter Editor:

I believe that ISBA's success will depend upon how well we are able to communicate, and that our newsletter provides our primary medium for communication. We would therefore welcome letters or light hearted articles concerning: (a) News about members, (b) Details about activities, (c) Suggestions for the Society, (d) Your views on Bayesian analysis, and its interaction with Science, and (e) Ways to make the Society truly international.

All letters or articles should either be mailed or E-mailed (leonard@stat.wisc.edu) to me. I would like to invite anybody interested to become a co-newsletter editor, and it would be good if we could find regional editiors, e.g., for Europe, Asia, Africa, South America, and Australasia.

It is planned to publish the Newsletter quarterly, but I would need to find other people to work with, before guaranteeing this frequency of publication. The Newsletter is currently prepared by my secretary, on Latex, and reproduced by a printer in Madison. Volunteers to help collect material are urgently requested!

I would like to welcome José Bernardo and Arnold Zellner as founding co-presidents. Both José and Arnold have been particularly kind to me during my career, and they have also helped and encouraged many other Bayesians.

I obtained my Ph.D. in 1973, with Dennis Lindley, and with lots of advice from Adrian Smith, I worked with Tony O'Hagan, Jeff Harrison, and Jim Smith at the University of Warwick in the 1970's, attended the first Valencia Conference in 1979, and organized conferences in Madison in 1981 and 1984. I am therefore very excited about our International Society.

Welcome to ISBA - by José M. Bernardo* Co-President ISBA

The First Bayesian European Conference I am aware of took place in Fontainebleu, France, almost 17 years ago, in June 1976. I attended that Conference as a young postgraduate – I had obtained my Ph.D. with Dennis Lindley only some months before – and I found the experience extremely stimulating. For the first time, I was interested in virtually all the talks at a conference!

A couple of years later one could sense that there was a general agreement amont Bayesian statisticians on the probable benefits of organizing a Bayesian World Conference. The opportunity arose when I was introduced to the Education Minister of Spain as the youngest Spanish professor at the time – I had just been appointed to chair the Department of Biostatisitcs at the University of Valencia – and he asked whether I had any requests . . .
I did!

Immediately, Morrie DeGroot, Dennis Lindley, Adrian Smith and myself, teamed together to organize the First Valencia International Conference on Bayesian Statistics, which took place in Las Fuentes in June 1979. The Conference was a success, and all participants agreed to try to make it a periodic event.

At that time, Spain had adopted a federal political system and the State of Valencia accepted the responsibility of funding World Bayesian Conferences every four years. Thus, Valencia 2 was, again, held in Las Fuentes in September 1983, Valencia 3 in Altea in June 1987, and Valencia 4 in Peñíscola in April 1991.

Although, during all this period, the Valencia Meetings in Europe and the Meetings organized by Arnold Zellner in the United Stated did maintain mailing lists and provided meeting opportunities for people interested in Bayesian Statistics, by 1991 there was a general feeling that a world-wide Bayesian Society should be organized which would take over, and certainly expand, these responsibelities.

Largely due to the personal efforts of Arnold Zellner and Tom Leonard, such a Society, ISBA, is now a reality and, next August in San Francisco, we expect to be able to hand over to its first democratically elected Board a working mechanism which should "benefit international society with the advancement of Bayesian analysis".

We will soon have a set of newsltters, a bulletin, and e-mail network, annual conferences and, generally speaking, a permanent common forum for discussion. To succeed, the new Society will need the combined efforts of all of us. Welcome on board!

I want to conclude by thanking all those who have helped during this transition period in keeping alive some form of coordination between people interested in Bayesian Statistics. The list is very long, but I would like particularly to recall the key role played by our late friend, Morrie DeGroot.

With the future in mind, let's meet in San Francisco!

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The International Society for Bayesian Analysis (ISBA): Objective and Future

by Arnold Zellner, Co-President ISBA

ISBA came into existence in 1992 with world-wide support. At Bayesian research meetings in Rio de Janeiro, Minneapolis-St. Paul, Valencia, and Cambridge, the proposal to create an international Bayesian organization was enthusiastically approved. Since then a call for charter members has met with a favorable response. ISBA currently has members form all over the world and is growing.

The following are some of the major objectives of ISBA as I see them at present.

(1) A prime objective of ISBA is to promote the further development and application of Bayesisan inference and decision techniques.

(2) A second objective is to educate students and practitioners with respect to Bayesian analysis and its uses.

(3) Since Bayesian analysis is employed in many field of study and application, another objective of ISBA is to provide a forum for workers from all the sciences and fields of application so that Bayesians can learn about devleopments in many areas and help unify the inference and decision methods utilized in the sciences and field of application. This effort will also involve interaction with philosophers of science.

(4) Computational techniques are an essential aspect of many Bayesian analyses and thus ISBA will attempt to facilitate the creation, description, evaluation and dissemination of Bayesian computer programs.

(5) To provide information regarding the development and application of Bayesian analysis, ISBA will sponsor international and regional meetings and publications.

(6) Since Bayesian analysis is important in many areas, ISBA will promote public recognition of noteworthy achievements by extending honors and awards to outstanding individuals and organizations.

In connection with (5), the first international meeting of ISBA will be hald on Friday and Saturday, August 6-7, 1993 in San Francisco, California just before the annual American Statistical Association and other statistical societies' meetings, August 9-13, 1993. Contributed paper and invited paper sessions are planned with some of the latter dealing with Bayesian analysis in various sciences and fields, e.g., biology, physics, astronomy, economics, psychology, law, medicine, industry, etc. A banquet to celebrate the founding of ISBA is scheduled for Friday evening, August 6, 1993. Also, tours and other social activities are planned.

Please plan to participate in the first international meeting of ISBA at which the above objectives and others as well as plans for the future will be discussed at an open public meeting. See you in San Francisco, if not before!

BAYESIAN ANALYSIS, AN OVERVIEW

Thomas Leonard

Consider a scientific experiment E which results in a vector y of numerical observations. Then the scientist might wish to assume that y was generated by some random process. Most philosophies of Statistics are based upon the presumption that y is the numerical realization of a random vector Y, which possesses some probability distribution P, defined on all events in the sample space S. This proposal is perhaps much more open to debate, than the existence of a prior distribution, discussed below. The existence of a sampling distribution is often best justified by frequency arguments, i.e., by letting P denote the limit of an empirical distribution, for realizations of y, observed during a large number of identical repetitions of E. However, a prior distribution can be based on current information.

Since P is typically unknown, it is frequently of interest to infer sensible choices of P, given the information provided by one observed y, together with the scientific background associated with E. Related problems include the prediction, given y, of a future observation z, and making real-life decisions which might be affected by knowledge of P.

A Bayesian is a scientist who represents his uncertainty in P by a probability distribution, defined over a set of different possible choices of P, and then uses probability theory to draw conclusions about P, from the corresponding conditional probability distribution, given the observed y, defined over the same set of possible choices of P. The first of these probability distributions is referred to as the *prior distribution*, since its specification must be based upon the information available, prior to observing y. The above conditional distribution, given y, is known as the *posterior distribution*. The posterior distribution typically helps the scientist to make a sensible choice of P, and hence to describe the random process underlying the observed data. This provides a rational approach to Scientific Inference, under the somewhat controversial presumption that probability theory defines rational behavior.

Suppose, for example, that the scientist has developed k completely specified choices, or hypotheses, or models, for the sampling distribution P. He, however, has no further evidence to distinguish the relative merits of his k possible choices. Then, prior to observing y, it is logical to represent this uncertainty in P by a discrete uniform probability distribution assigning equal probabilities to each of his k choices.

It is now an elementary exercise to calculate the posterior probabilities, i.e., the conditional probabilities that any particular choice of the k models is true, given that Y is observed to equal y (see Press, 1989, p. 26). The posterior probabilities are proportional to the densities, or probability mass functions of Y, when evaluated at the observed y. The rule needed for this calculation is known as Bayes' theorem (see Bayes, 1763, as reported by Press, 1989, pp. 185-229), usually attributed, to my compatriot, the Reverend Thomas Bayes (1702 -1761). The Bayes family tomb can be found in Bunhill Cemetery, Moorgate, London, and a plaque was placed on top of the tomb in the late 1960's commemorating Thomas Bayes' fine contribution to the historical development of probability and statistics. Bayes originally applied the theorem to a problem involving billiards balls. However, the current example provides a more general application, permitting a scientist to select a model or hypothesis, from one of his k prior choices, or to create a new model e.g., the mean of his posterior distribution can permit a very broad specification.

As another example, suppose that the experiment E consists of taking a random sample without replacement, say of size $n = 10,000$, from a much larger population, and observing a vector y of binary responses, where, for each person in the sample, the binary response indicates whether or not the person possesses a particular gene Z. In this case the probability distribution of the corresponding random vector Y can be completely defined, up to the specification of an unknown parameter *i.e.*, the gene frequency, or population proportion, θ, which denotes the proportion of people in the population with gene Z. Up to a close approximation, the corresponding binary responses, possess independent binary distributions, each with the same probability θ, equal to the population proportion. The problem of inferring an appropriate choice of P therefore reduces to the problem of estimating a reasonable value for θ.

The preceding situation provides an example of a "parametric model", and most of the Bayesian literature is concerned with "parametric inference". However, as discussed by Box (1983), it is also important, in general, to consider "statistical modeling" *i.e.*, the reduction of the choice of P to a model involving several real-valued parameters. Except in special circumstances, a choice of parametric model can be very difficult, since it is not always possible to specify a finite number of possible alternatives. It is therefore not always possible to complete practical data analyses by just using applications of Bayes' theorem. Some combination of Inductive Modeling (intuitive thought about the data in relation to its scientific background) and deductive calculation (via a Bayesian analysis) is often needed.

In the context of our gene frequency example, the information in the data about θ, may be summarised by the "*likelihood function*", which may be sketched as a function of the unknown θ. This is just the density, or probability mass function of the y vector, given θ, but with y replaced by the y actually observed. Suppose that $x = 211$ out of the $n = 10000$ in the sample are observed to possess gene Z. Then, assuming exact independence of the binary responses, the likelihood is proportional to a beta curve, with parameters 212 and 9790 (see, Press, p. 40). The likelihood function provides the basis for "conditional inference"; see Birnbaum (1962), and Edwards (1970). This theory is restricted to situations where the model is assumed known, up to the specification of some unknown parameters. However, Birnbaum's key result that the Sufficiency Principle, for a random experiment, together with the Conditionality Principle, imply the Likelihood Principle, invokes the importance of both conditional inference and the likelihood function, when axiomatized by frequentist concepts. The only real substance in Birnbaum's result is provided by the Neyman-Fisher factorization theorem which emphasises the close relationship between the minimal sufficient statistic, and the shape of the likelihood function. This theorem apart, Birnbaum's result is almost tautologous, because of the unusual way in which his proof applies the Sufficiency Principle.

From a Bayesian perspective, it is now only necessary to choose a prior distribution for the unknown θ, since in the current situation the remainder of the sampling model is specified. It is important to appreciate that any probability distribution on the unit interval might be appropriate, depending upon the information possessed by the the scientist before observing y. Students, and even colleagues, often ask me "what is the correct prior distribution for such and such a situation?", and I sometimes answer "any prior can be correct and most are usually wrong". There seems to be a mistaken impression in the Bayesian literature that there is something sacrosanct about a conjugate prior (which is chosen to ensure that the posterior distribution belongs to the same, hopefully simple, family). However, if we consider generalizations to multiparameter (e.g., several unknown population proportions), or non-linear (e.g., logistic regression) problems, then we soon learn that a simple conjugate prior can either be overrestrictive (e.g., an overspecialized covariance structure), or non-existent (e.g., most non-linear regression problems).

In our independent binary situation, the simplest available conjugate family for θ is the beta family, say with parameters a and b (see Press, p. 41). For example, $a = 10$ and $b = 190$ would give a prior mean of 0.05. This specification would be reasonable, if previous direct or indirect information available to the scientist suggested a best guess for θ of 0.05, and that he judged this information to be equivalent in strength to the information possessed by $a + b = 200$ real observations. When $y = 211$ and $n = 10000$, the posterior distribution of θ is now beta, with updated parameters $a + x = 221$ and $b + n - x = 9979$. These yields a posterior mean of 0.0217, which smooths the sample proportion of 0.02 to compensate for the prior information. The posterior distribution can also be used to make probability statements about θ, given y, based upon the incomplete beta function, or to calculate 95% or 99% posterior intervals. However, it is very important to sketch both the likelihood, and the posterior density, since these conveniently summarise the sampling and posterior information. Convenient classes of non-conjugate priors include normal distributions for either the logit or probit transformation of θ. Leonard (1972, 1975) demonstrates how these families conveniently generalise to multiparameter and contingency table situations.

If I asked a student on my Bayesian Decision theory course how Bayesians should handle prior ignorance and the student answered "by using an ignorance prior", then I would grade this answer with a zero. A correct answer is, "by just referring to the likelihood function, since any prior distribution, even when improper or uniform, introduces substantial prior information. However, scientists should rarely find themselves in a situation of prior ignorance, and they should therefore always seek a probabilistic model for the prior information available. Failing this, they could use an appropriately vague prior distribution, in the hope of getting a posterior which approximates the posterior under a more informative prior distribution."

For example, there is no ignorance prior available in the preceding binary situation. However, possible vague priors include a uniform distribution for θ, improper uniform distributions for the logit or probit, and the Jeffreys prior, which is beta, with parameters $a = 0.5$ and $b = 0.5$. Which of these alternatives would I recommend? Here I would be pragmatic and recommend the Jeffreys prior, because of the interesting consequence that the posterior 95% and 99% intervals are also exact 95% and 99% confidence intervals *i.e.*, possess excellent frequency coverage, when E is repeated a large number of times.

This frequency result experienced a mixed reception, when I mentioned it to colleagues at the Sir Harold Jeffreys Anniversary Conference in Chicago a couple of years ago. Indeed many Bayesians seem to be unconcerned about frequency properties. There seems to be a historical misconception that the Bayesian and frequency approaches are diametrically opposed. I take a different view while still maintaining that I am a blueblooded Bayesian. I regard the frequency approach as an important *subset* of the Bayesian approach. Firstly, it is frequently the only way of justifying the assumption concerning the existence of a sampling distribution, as described in the first paragraph of this article. Secondly, in situations where a model is specified, given some unknown parameters, any good frequency based procedure usually turns out to also possess a Bayesian justification under some sensible prior. The Complete Class Theorem (Berger, 1985, p. 532) says that, under wideranging regularity conditions, any estimator or decision making policy which possesses the frequency property of admissibility, must also be valid under the Bayesian paradigm. I believe that Bayesians can get the best of both worlds i.e., they can use their approach, which is based purely on probability theory, and hence free from theoretical counterexample, to construct and create their procedures, and they can then refer to outstanding frequency properties (e.g. the mean squared errors of their estimators, and the U.M.P.U. properties of procedures based upon posterior odds ratios) which show that their approach is also at least as valid as any other approach, in an objective scientific sense, under most sensible choices of the prior distribution.

Does Bayesian research have an excellent track record when compared with other approaches to applied statistics? Yes, and I cite five main areas:

(A) THE SIMULTANEOUS ESTIMATION OF SEVERAL PARAMETERS: James and Stein(1961) created substantial impact, by proving the remarkable result that the unifom minimum variance unbiased estimators for several parameters, can possess very inferior mean squared error properties, when we move to three or more dimensions. In the consequent literature (e.g., Lindley and Smith, 1972) it is shown that the most reasonable alternatives are Bayesian shrinkage estimators based upon hierarchical exchangeable priors, and their generalizations. Hence, when considering several parameters, the Bayesian approach yields immense practical advantages, which are not quite as evident when attention is restricted to single parameter problems. Since 1972, many of the computational techniques have been updated (e.g., Leonard and Hsu, 1992).

(B) MARGINAL INFERENCE: For non-linear models it is often of interest to summarise the sampling or posterior information regarding either one of several parameters, or some function of the parameters. The only substantive finite sample marginalization procedure as yet proposed from a non-Bayesian perspective is "profile likelihood" due to Kalbfleisch and Sprott (1969). However, Bayesians can, at least in principle, just refer to the marginal posterior density of the parameter of interest. Until the 1980's the calculation of the marginal density was often prohibitively difficult, as multi-dimensional numerical integrations were required. However, these can now be simulated e.g., by importance sampling (see Zellner and Rossi, 1984) or very closely approximated by Laplacian methods (e.g., Leonard et al, 1982, 1989, and the KTK approximation they reference, due to Kass, Tierney, and Kadane). Moreover, under a uniform prior the Laplacian approximation to the marginal density of any particular parameter is equal to the profile likelihood, but divided by an important determinant term. In many examples the ordinary profile likelihood is much too thick in the tails.

(C) NON-PARAMETRIC FUNCTION SMOOTHING AND DENSITY ESTIMATION. Grace Wahba has pioneered an approach for smoothing regression functions, initiated by Kimeldorf and Wahba (1970) who demonstrated that the posterior mean value function under particular priors on Hilbert Space, are also splines. Similar approaches may be used to smooth probability mass functions, or densities, without assuming that they are constrained to belong to any particular parametric family. This provides a formal paradigm permitting Bayesians to model the distribution P, as defined in the first paragragh of this article. Wahba and Wendelberger (1980) applied these ideas to the area of meteorolegy, an area also noted for basing prior distributions upon large previous data bases. Contributors to density smoothing include Whittle, Good, Lenk, and myself, and these have yielded substantial applications, e.g., the smoothing of actuarial and survival data.

(D) EXPERIMENTAL DESIGN: Pilz (1991) reviews a large literature which demonstrates, that experimental design can only be sensibly considered in the presence of prior information. The Bayesian area has achieved an excellent trackrecord as the only reasonable paradigm for formalising the design area; see, for example, the contributions by O'Hagan and Chaloner.

(E) TIME SERIES AND FORECASTING: Harrison and Stevens (1976) pioneered the applications of Bayesian Hierarchical Models for forecasting, further developing the models and updating procedures proposed in the Engineering and Control theory literature by Kalman, Astrom, and Mehra. These procedures are quite sensibly formulated, and reduce to Box-Jenkins forecasts, in special cases. They can also handle discontinuous change, and provide an excellent Bayesian success story for scientists and engineers, working both inside and outside the usual mainstream of Bayesian Statistics. Michael West has also published a series of papers in this area. My own experience is confined to developing a computer package, with Harrison, in the 1970's,for predicting World Sales of fibres, for Imperial Chemical Industries. There are some severe practical problems, as the forecasts are highly sensitive to the choices of prior parameter, and modest changes in these choices can yield a broad range of forecast.

Let me now address the key question: "Why should a scientist consider assuming a prior distribution for his unknown parameters"? I have the following reasons:

(a) To incorporate any prior information which can be represented by a probability distribution.

(b) To create useful statistical procedures, for example smoothing estimators, depending upon some (first stage) prior parameters, which can however be evaluated with the help of the data, either by assigning a second stage to the prior, or, as an approximation to a fully hierarchical Bayesian procedure, directly from the data, using the ideas of parametric empirical Bayes,reviewed by Morris (1983).

(c) To be able to calculate predictive distributions for future observations.

(d) To virtually guarantee acceptable finite sample frequency properties.

(e) To be able to think about his prior distribution in a rational or coherent fashion, by reference to one of the many axiom systems for subjective probability. While some systems interprete a probability in terms of the expected winnings for a monetary bet, the system described by De Groot (1970, pp. 69-82) is free from concepts of utility or money. This encourages the expert to calibrate his probability with the results of an objective auxiliary experiment e.g., a spinning pointer.

(f) If a formal decision needs to be made, then the scientist can then simply minimize the posterior expectation of an appropriately specified loss function (or equivalently choose the decision function minimizing the frequency-based risk, when averaged with respect to the prior).

(g) To input enough information to be able to make a well-defined finite sample conditional inference, hence avoiding the need to resort to ad hoc devices like profile or partial likelihood, fiducial or structural inference, asymptotics, or the bootstrap, which fail to work simply because insufficient assumptions are made.

Perhaps (e) and (f) merit further discussion. If a scientist decides not to be Bayesian, and also does not inadvertently "act like a Bayesian", when summarising his post-data information, then he must, under any axiom system for subjective probability, be breaking one of the axioms. The philosophical question then arises as to whether a scientist, or politician, or sociologist, or judge, must feel compelled to act like a Bayesian in order to remain rational or coherent? My answer is a very qualified Yes; only if we are in a situation (e.g., analysing a specified model with unknown parameters, or handling a very trivial law case) where probability theory can be used to cope with the situation, and rationality is defined to be "adherence with the laws of probability and conditional probability".

While most axiom systems for subjective probability comprise useful descriptions of Bayesian behavior, they are generally at least as strong as the Kolmogorov axioms of probability, e.g., the countable additivity property, and therefore cannot be fairly used to compell a reluctant scientist to satisfy the laws of probability, or to brand him as irrational or incoherent. For example, if the sampling distribution P is unspecified, any inductive, intuitive, or ad hoc process should be permissible in order to discover either an appropriate P, or a finite set of possible hypotheses for P. While some of us have attempted a more formal approach, it stretches credulity to expect every scientist to develop a prior distribution across the space of all sampling distributions, or to expect a judge to be able to specify a probability distribution across the space of all possible truths, in a complex legal case.

Since the pioneering work by Meginness (1976), it is no longer obvious that Economic decisions should follow the expected utility hypothesis, or that the Savage axioms are at all convincing, or that Bayesians should make Bayes decisions, i.e., minimize the posterior expectation of their assumed loss function. Allais' paradox convinces me against the former.Nevertheless,Bayes decisions are valuable because they possess good long run frequency properties. However, while parametric inference can be completed formally, by reference to the posterior distribution, many decisions are perhaps more sensibly made by relating the posterior inference to the decision maker's real-life feelings and experience, and then permitting him to make an applied human judgement. I prefer this to the overconstrained sense of rationality created by minimizing the posterior expectation of a possibly artificial loss function, without seeking to optimise other summaries of the posterior distribution of the loss function. I am for example very cautious about current applications of Bayesian Decision Theory in Business and Economics (see Cyert and De Groot, 1987).

It is important for our younger researchers not to lose sight of the history, philosophy, and inbuilt integrity of our approach. We now need to more fully emphasise the duality between a flexible Bayesian rationality, and the ability to make scientific discoveries, based upon numerical data collected with the purpose of updating our prior scientific knowledge i.e., to emphasise the creativity which can be catalysed by Bayesian ideas, and confirmed by outstanding frequency properties.

Your comments on my article are invited. I agree with both I.J. Good and Don Rubin on the need for a Bayes/non-Bayes compromize, but also think that it is important to unify our diversity of ideas on the Bayesian approach. Arnold Zellner's Presidential Address in the March 1992 issue of JASA is well worth reading.

The following highly selective biobliography is by no means complete, but it is intended to help the reader to achieve a quick introduction to the subject area. My thanks to John Hsu and Richard A. Johnson for reviewing this article for me, and making a number of valuable suggestions.

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