THE ISBA BULLETIN

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A Report from the Program Council

by Edward I. George
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The principal role of the ISBA Program Council over the past year has been to oversee the planning and implementation of ISBA 2000, and to make recommendations to the Executive Committee concerning ISBA sponsorship or co-sponsorship of proposed meetings.

To begin with, I am delighted to report that our Sixth World Meeting, ISBA 2000, which just took place in Hersonissos-Heraklion, Crete on May 28 - June 1, 2000, was a tremendous success. Ideally situated at glorious luxury hotels, an impressive scientific program of 126 talks and 108 posters was presented. The Bayesian spirit of hard work and hard play was ever present, and a productive and fun time was had by all. The bulk of the planning and implementation of ISBA 2000 was carried out by three committees - the Program Committee (Mike West, Chair), the Finance Committee (Alicia Carriquiry and Stephen Fienberg , Co-Chairs) and the Local Organizing Committee (George Kokolakis, Chair). These committees, especially the chairs, did a superb job, and their tireless efforts are most gratefully acknowledged. A refereed proceedings volume is in the works, and will be published and distributed by Eurostat, who co-sponsored the meeting. In addition to all this generous support, Eurostat will also distribute a complimentary copy to all current ISBA members. Submission information for this volume is now available at the ISBA website.

Moving on to other meetings, the Program Council evaluated a proposal from Dale Poirier for ISBA sponsorship of a meeting on Bayesian Applications in the Behavioral Sciences to be held in Laguna Beach, California on April 5-8, 2001. The Program Council made a positive recommendation to the Executive Committee who then formally approved it. I am pleased to announce that this meeting has now been officially designated as an ISBA 2001 North American Regional Meeting. Interested participants should consult `www.sossci.uci.edu/bayesian` for details.

The Program Council has also received a number of inquiries about potential ISBA involvement in other meetings, but at a lesser level than sponsorship or co-sponsorship. To accomodate such requests in the future, the Board of Directors recently approved a proposal that will allow for ISBA to officially endorse meetings with Bayesian content that is of direct interest to ISBA members. I am pleased to announce that our first ISBA Endorsed Meeting will be the Second Workshop on Bayesian Inference in Stochastic Process to be held in Varenna, Italy on May 31-June 2, 2001. The organizers are Susie Bayarri, Guido Consonni, David Higdon, Pietro Muliere, Sonia Petrone, David Rios Insua, Fabrizio Ruggeri, Mike West and Mike Wiper.

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Interested participants should consult www.iami.mi.cnr.it/conferences/varenna.html for details. I believe that such endorsements will not only bring promising meetings to the attention of ISBA members, but will also help to increase the overall visibility of ISBA. Future requests for ISBA endorsements will be reviewed by the Program Council and submitted to the Executive Committee and Board of Directors for formal approval. Information about all ISBA sponsored, co-sponsored and endorsed meetings will be made available on the ISBA website.

Finally, I would like to encourage all ISBA members to be more proactive in seeking out, and bringing to the attention of the Program Council, meetings that might be appropriate for ISBA sponsorship, co-sponsorship and endorsement. As Phil Dawid wrote in the March 2000 issue of the ISBA Newsletter, the popularity of Bayesian methods is rapidly increasing across many groups outside of Statistics. ISBA should and can play a vital role in bringing these groups together.

ISBA Nominating Committee 2000

At the recent ISBA 2000 World Meeting in Crete, the Board appointed this year's Nominating Committee. Its task is to put forward to the membership two or more nominees for each vacancy that will arise as from 1 January 2001, namely: President-Elect, Executive Secretary (to replace Michael Evans), four members of the Board of Directors (to replace Alan Gelfand, Jay Kadane, Rob Kass and Luis Pericchi).

The composition of the Nominating Committee is as follows: John Geweke (Chair, Iowa), Caitlin Buck (Cardiff), Merlise Clyde (Duke), Petros Dellaportas (Athens), Pilar Iglesias (Chile), Peter Müller (Duke), Donna Pauler (Harvard) and Luis Pericchi (Venezuela).

If any member of ISBA wishes to make any suggestions as to suitable nominees, please do so to John Geweke (john-geweke@uiowa.edu) as soon as possible, and in any event not later than July, 23rd.

The ISBA Website

by Mike Evans
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The ISBA website www.bayesian.org is a resource for ISBA members and more generally for the public who wish to learn about Bayesian inference. Several initiatives are being undertaken to make the site more useful. Part of this will be an expansion of the Bayesian Links section of the site. If you know of links that would be useful additions please send them to the webmaster. Also the website contains a section called Some Books by ISBA Members. This is a steadily growing list of links to descriptions and advertisements for currently available books published by members of ISBA. If you have a publication that you would like to be included in this list please send the relevant details to the webmaster.

ISBA Logo Competition

by Mike Evans
ISBA Executive Secretary
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The ISBA Board decided at its recent meeting at ISBA 2000 that it would be advantageous if ISBA had a logo. Further it was decided that the acquisition of this logo would be via a competition amongst ISBA members. The winner of this competition will receive a one week vacation for two at the beach resort (www.ntua.gr/ISBA2000/new/accommkrv.html) in Crete where ISBA 2000 was held (accomodation expenses and half-board expenses only and there are some time restrictions on when it can be taken)! Those attending this meeting can attest to the value of this prize as it is indeed a very pleasant and interesting location.

To submit an entry for this competition please send, by October 15, 2000 a .jpeg (.jpg) file to the ISBA webmaster (mevans@utstat.utoronto.ca) or alternatively send a scannable image to:

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Professor Mike Evans
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Dept. of Statistics
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All entries will be posted on the ISBA website and ISBA members will vote electronically to select the winning entry.

A WORD FROM THE EDITOR

by Fabrizio Ruggeri
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As you have already noticed, the Newsletter has changed its name: now it is the ISBA Bulletin. The ISBA Board decided at its recent meeting in Crete that Bulletin was a more adequate name for this publication. In Crete we discussed plans to have a smooth transition to a new Editor when my term (December, 31st 2001) will be over. Our plan is to gradually change all the Associate Editors in the next 12 months so that the new Editor will have a skilled Editorial Board in helping him/her for the first issues. At the same time, I am very sorry because the plan will deprive me of these brilliant people, who, by the way, deserve a rest from their job! Therefore, it is time for you to make suggestions about the future Associate Editors.

In Crete, Val Johnson (ISBA Treasurer) presented the ISBA budget. His figures confirmed that e-mailing of the Bulletin would help ISBA to use money for other activities (e.g. travel grants for young researchers). Therefore, we continue our campaign for e-mailing of the Bulletin (.ps, .ps.gz or .pdf): it is enough to drop a line to `isba@iami.mi.cnr.it`.

SIMON FRENCH

by David Rios Insua
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Given the slight decision analytic flavour that our Editor has given to this issue of the Bulletin, it seems appropriate to interview Professor Simon French, a leading author in the Decision Analysis arena and well known in our Society. Simon completed his studies at Oxford, with a D. Phil. in crystallography. He then moved to the Decision Theory Department at Manchester, then to the School of Computer Studies at Leeds and, then, back to Manchester where he is currently a Professor at the Manchester Business School. Simon's books in Decision Theory and Decision Analysis are widely used. He was the founding editor of the Journal of Multicriteria Decision Analysis and has been consulted in many important projects, including the aftermath of the Chernobyl accident and various risk analysis problems with the UK Health Authority.

We emailed Simon the following questions

`1. What got you hooked on Statistics and Decision Analysis? And why Bayesian?`

I guess it was during my undergraduate days. I read mathematics and during our second year we had a course on regression analysis. I hated it. All I – and my friends on the course – remember is that we 'played' with lots of matrix identities, saw NO data and left believing that Anova was a Russian mathematician! Later I did find my notes useful in understanding the contortions of frequentist statistics; but that was much later and by then I had become hooked on Bayesian ideas. That happened in my third year. There was a young lecturer who had just joined Oxford: Adrian Smith. With Mike Dempster, he taught a course based upon a brand new book by Morrie DeGroot: Optimal Statistical Decisions. Enthusiastic lecturers; great book and sensible ideas: it just all made sense. So I became a Bayesian. DeGroot is decision focused so I entered statistics from the perspective of decision making.

Later when I had finished a doctorate on the application of Bayesian statistical ideas to the analysis of protein crystallography – they worked! – I was looking around for a job. An advert appeared in the Times: "Wanted Lecturer in Decision Theory: no previous experience required". Well I applied and fortunately the other applicants had lots of previous experience. So, fitting the specifications pretty much perfectly, I got the job and joined Doug White in the Department of Decision Theory at Manchester. Over the next few years I studied decision theory and analysis and I guess that confirmed me as a Bayesian decision analyst rather than a Bayesian statistician.

`2. Can you name some of the people and events` `that have influenced you during your career?`

Well I have named Adrian and Doug above. They really did support me and help me explore ideas. When I was doing my doctorate at Oxford, I was in the Laboratory of Molecular Biophysics, working with the real problem owners, not in the Mathematics Department. But Adrian always had time for me and helped me translate the theory of DeGroot's book into real sequential data analysis. Actually another book was very formative on my thinking: Optimisation of Stochastic Systems by Aoki. Later I found that many of us who used Kalman filters to sort out and analyse hierarchical models (no MCMC methods in those days!) had read Aoki and found enlightenment.

I should also mention that the environment in the Laboratory of Molecular Biophysics was amazing for learning 'how to do research'. It was organised into co-operative teams of physicists, biologists, biochemists, computer scientists and me, the mathematician. The days were partitioned into periods of hard work separated by sessions in the coffee bar or pub during which we discussed a whole variety of ideas, some mad enough that they led to very successful research! I have seldom worked in such a multi-disciplinary and supportive an environment since. Although she wasn't the head of the laboratory, I would also mention the example provided by Dorothy Hodgkin, the Nobel Laureate. We didn't have to read her biography to see how devotion to science, family, friends and students shaped her and our lives.

Returning to statistical influences, Dennis Lindley examined my doctorate and took a great interest in my career. Although I did not pass through his group at UCL, I look upon myself as one of his disciples who he sent out into the heathen frequentist world.

The Department of Decision Theory at Manchester again was a marvellous place... although not quite so multi-disciplinary. Doug White, Roger Hartley and Lyn Thomas – the other members, we were a small department! – All mathematicians... I don't know what it was about Doug's leadership. There is very little one can point to and say he did that differently and better than other Heads of Department. But almost all those who joined his groups over the years as lecturers or research fellows are now professors. Somehow he inspired us, me included.

So I guess to all of those people and plenty of others too I owe a great vote of thanks.

`3. You have been involved in many important real problems. Could you summarise some of them for us?`

I think that you are being rather kind. I have only really been involved in one family of 'real' problems which might be called 'important': nuclear and health risks, beginning with my involvement in the International Chernobyl Project in the early 1990's. Before I became involved with all the activities surrounding the aftermath of Chernobyl, I was pretty much a mathematician who had applied the methods of decision analysis in real contexts. I had worked with Larry Phillips on decision conferencing a number of applications in industry and the public sector. I had also worked in a group using decision theoretic ideas to help design public examination processes. But looking back, I always went into those applications with the sort of decision models in my 1986 book very much guiding my thoughts. Then I went out to Chernobyl...

The word *Chernobyl* entered our consciousness in April 1986 and still evokes images of the World's worst nuclear accident. That memory is only partially right: Chernobyl was and still thankfully is the worst nuclear accident we have seen; but its effects stretch far wider than purely radiological consequences. Medical effects directly arising from the radiation, i.e. cancers and genetic effects, are severe, but not nearly severe enough to explain the morbidity among those living in the region. Morbidity in excess of 70% was found in regions of Byelorus in early 1996, and is most likely related to very high levels of stress. While the accident itself and the presence of radioactive contamination are clearly causes of the stress, there are other causes related to poor information management and communication of the risks to the public. Many studies of how the emergency and its aftermath were handled in the (then) Soviet Union, the Eastern Bloc and – before we get too comfortable – across Western Europe show that decision makers at all levels were unprepared and unsupported for the decisions that they needed to take and the need to explain and advise the public on their actions. There was little consistency between the actions taken in different regions and, as this was exposed through the international media, the public soon became skeptical of any advice they received. Thus one of the major lessons learnt from the Chernobyl accident is the importance of a coherent, harmonised and sensitive response to nuclear emergencies, together with clear, honest and consistent information provision to the public.

While decision analysis can help in structuring thoughts about the issues and developing guidance for the decision makers, it only provides a small part of a solution. To ensure that we face up to disasters and public risk issues better than we faced Chernobyl, we need take a much wider view drawing on cultural, social, psychological, economic, and many other perspectives. So since my involvement in the Chernobyl project began, there has been far less emphasis on the mathematical underpinnings of decision analysis within my work and far more on the many multi-disciplinary issues that must be addressed in implementing decision analysis.

Thus over the past decade I have worked very widely within the RODOS project to develop a common, comprehensive decision support system for off-site emergency management, capable of finding broad application across Europe. The RODOS system has been developed by a consortium of over 40 institutes in Europe and the Former Soviet Union and is now being implemented in emergency centres in several countries. RODOS provides decision support at all levels ranging from largely descriptive reports to a detailed evaluation of the benefits and disadvantages of various countermeasure strategies and their ranking according to the societal preferences as perceived by the decision makers. Although I entered the project along with Jim Smith to provide a Bayesian perspective on the forecasting and decision analytic methods embodied in the software, we quickly found that we were addressing a much broader range of issues, from IT and software quality assurance issues, on the one hand, to emergency management processes and political imperatives, on the other. And along the way we had to pick up a fair bit of understanding of atmospheric dispersion, food chain modelling, radiation protection and economics too.

Recently, I have become involved in the establishment of the new Food Standards Agency in the UK. If we are to avoid another BSE ('mad cow') crisis, its operations will similarly have to take such a broad multi-disciplinary approach.

`4. How have nonquantitative people like managers and politicians received Bayesian ideas? What have been the main difficulties you have had explaining these ideas to them?`

With one major exception, I have never found it too difficult to work with managers or politicians and get them to use subjective probabilities, values and utilities. Even in the Chernobyl study with the awesome range of issues faced there, the politicians had no difficulty in addressing the problem via multi-attribute models; indeed, they clearly gained understanding from doing so. It is a trite, often made, but none the less true observation that the only people who find Bayesian ideas difficult are those who have already invested much effort in understanding frequentist or similar contorted thinking on decision and inference! But the exception does give pause for thought.

Within the RODOS project we have run several exercises to work with decision makers on how they would handle an accident. During these we have had very limited success in using probabilities and multi-attribute utilities, particularly in the part of the exercise relating to a threat stage, i.e. when a reactor is clearly behaving badly, but engineers may still avert a disaster. At such a time, one think that a Bayesian analysis using, say, a decision tree would help the emergency managers.

But they rejected it ... and pretty much rejected any explicit analysis. One may argue that the time imperatives are such that they cannot take the time to analyse issues fully: but I also have an uncomfortable feeling that the responsibilities on their shoulders are so onerous and the probability of litigation after the event so high that they eschew any explicit analysis because of the audit trail it creates.

`5. What are in your view, the relations between decision analysis and Bayesian Statistics?`

Is this an opportunity, David, to promote our book??? (Simon French and David Rios Insua *Statistical Decision Theory* to be published in the Autumn by Edward Arnold). In that I think we explore the relationship in some depth. But to be rather more succinct here, I think that in spirit there is little difference. Dennis Lindley remarked at one Valencia meeting that Bayesians should be 'belief and preference analysts'. In Bayesian Statistics you lean more to the former; in decision analysis, more to the latter. But the methodologies and skills that you need are basically the same.

`6. To some extent, ISBA tends to focus on inference and data analytic problems, without considering the wider problem of decision making. Any views on how this could be solved?`

'Get more members involved in real decision making' is one answer. But is there really a problem to be solved? What is wrong with ISBA focusing on inference and data analytic problems, as long as it recognises that in doing so it is only addressing an aspect of real decision making processes. Support of decision making necessarily requires multi-disciplinary approaches. ISBA is fostering one of the component disciplines.

`7. In the field of decision analysis, especially in multicriteria decision making, there are many alternative schools of thought (outranking, AHP and the like). How do you view the coexistence of these groups?`

With a mixture of pleasure and frustration. The field of decision analysis, as any other discipline, will only develop through discussion and criticism: bland groupings of like minded people soon go stale and fail through inbreeding. Thus in many ways I welcome the wide variety of schools of thought. I helped found the Journal of Multi-Criteria Decision Analysis precisely to provide the forum in which the Schools could meet, share ideas and discuss differences and so develop the broader subject. My frustration is that too many of the schools are inward-looking, proud of their 'brand' and unwilling to look for and acknowledge the good points in their competitors thinking.... mind you, I did refer to the 'contorted' thinking of frequentists above! It is easy to become comfortable in the familiar world of ones home school of thought and dismiss others.

`8. Recently, you have been paying attention to new technologies facilitating administration, the political side of e-commerce, a kind of e-government? Do you see any role for Bayes/decision analysis methods in this area?`

One of the strengths of the Bayesian approach is its separation of uncertainty judgements from value judgements. This can be very helpful in structuring societal decisions for e-government. It separates the domain of science from that of the values of the stakeholders. Thus I can see the technology enabling a public consultation process in which individuals can explore how a particular course of action matches their personal values. Suppose, for instance, we consider a facility siting decision. One could put a decision model on the web in which the impacts of the alternative facilities and sites were forecast and then allow the public to evaluate them through sensitivity analysis. Such a process would both foster public understanding of the issues and also act as much more structured public consultation for the authorities.

`9. More generally, what do you feel will be the main trends in decision analysis in the next decade?`

Exciting, I hope!

WHAT IS A BAYESIAN?

by Dennis Lindley
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When I began to study statistics in 1943 the term 'Bayesian' hardly existed; 'Bayes', yes, we had his theorem, but not the adjective. If there was a distinction then, and it was not an important one, it was between direct and inverse probability. In illustration, if p is the probability of success in each of n trials, independent given p, the probability of r successes in n trials is found by the binomial distribution. This is an example of a direct probability but if, in contrast, we ask, having observed r successes in n trials, what can we say about p, then this is a problem in inverse probability, where the first solution was offered by Bayes. Notice the distinction here is between problem types, rather than between methodologies for solving the same problem and, as a result, the two types of probability existed comfortably together, and writers after Bayes could shift from one to the other without embarrassment.

The situation changed in the 1920's when Fisher began to produce brilliant solutions to inverse problems using direct probability, and over the inter-war years introduced concepts like sufficiency and likelihood, and techniques like analysis of variance, all without use of the inversion ideas inherent in Bayes's theorem; indeed, he laid emphasis on the freedom of these methods from contamination with a prior. With a little help from Neyman and Pearson to keep us mathematicians happy, this was the state of statistical affairs at the end of the second world war. At this point several of us were unhappy with the situation, not because of the Fisherian methods, which seemed of great practical importance, but because they lacked the cohesion we expected of a mathematical discipline, where there was a set of axioms from which theorems could be proved. Our hope was that a set of axioms could be found so that the theorems would include the mixed collection of results that we had acquired from the masters and that thereby we would have both a satisfactory explanation of these ideas and, more importantly, it would provide a methodology whereby new theorems and useful implementations, could be found. I recall at the time thinking that statistics ought to be like Newtonian mechanics, over which I had laboured to acquire my first degree, with Newton's laws and the theorems which flowed from them.

Some progress was soon made by Wald (Statistical Decision Function, 1950) but his concept of loss was unclear and the one application he took of the minimax estimator of the binomial mean, the p of the first paragraph, was almost absurd. The real advance came with Savage (The Foundations of Statistics, 1954) who, in his first seven chapters, accomplished everything we mathematicians had hoped for. Here were the postulates, clearly stated in the end papers, and from them theorems derived, leading to the constructive methodology that we needed in the maximisation of expected utility, MEU. Savage was a true scholar who took proper notice of the work of others, learning significantly from them, and he found that surprisingly others had been there before him. Two contributors stood out: Ramsey, who in 1926, had trod similar ground. His "concepts of probability and utility are essentially the same as those presented in this book" is Savage's own description; and de Finetti, who, beginning in 1937, had developed a theory of personal probability and who would work with Savage in the following years to develop the ideas and who later produced the definitive work on the topic (Theory of Probability, 1974/75). The earlier books by Jeffreys (Theory of Probability, 1939) and Good (Probability and the weighing of evidence, 1950) were also relevant, though the mathematical rigour of the former did not match with the concept of rigour (or should it be rigor) of Chicago mathematics.

Here there appeared to be salvation, we mathematicians turned statisticians had got what we wanted; Savage's statistics to place alongside Newton's mechanics. But by 1971 the dream had shattered and Savage was to write in his second edition, more honestly than most scientists can manage, that his attempt in the second part of his book to justify the ideas of Fisher and others, which he termed frequentist, had failed and he "reluctantly admits that justification has not been found". What had gone wrong? Having produced MEU as the constructive device for producing statistical methods, we tried to apply it to standard problems, finding sometimes that it agreed, as in the use of sufficient statistics, but more often finding that it did not, for example in the use of the tail area in a significance test. (Interestingly Jeffreys had pointed this out in 1939 but none of us had fully appreciated what he was saying. This is especially ridiculous in my case since I had attended Jeffreys's lectures in Cambridge in 1947; the only excuse I can offer, apart from my own stupidity, is that he was a bad lecturer. But that is not valid since his book is, at least seen through today's eyes, lucid and still worth reading.)

The real crunch came in 1962 with Birnbaum's derivation of the likelihood principle from postulates that the frequentists had accepted, and continue to accept, even when their logical consequences are pointed out to them. Now the likelihood principle is a trivial consequence of Savage's argument and yet is violated by almost all frequentist methods. This is true even of maximum likelihood estimation, the estimate adhering to the principle but its associated standard error not. Incidentally the principle had been recognised as early as 1947 by Barnard and even appears in Fisher's writings in 1955 but I think it is fair to say that its importance had not been fully appreciated until Birnbaum's paper. Now there was a real difficulty; the principle followed from Savage's postulates and therefore was integral to MEU, but it also followed from frequentist ideas yet was not used by them. This made frequentist ideas logically inconsistent and therefore, by mathematical standards, unacceptable. The conclusion was therefore exactly the opposite of what had been the object of the original exercise, to support frequentist ideas, and had ended up, not by supporting, but by destroying them. The Savage school had produced a constructive approach that began to be explored and was found, within the limitations of the computing capacity of the day, to work, but which largely disagreed with Fisher.

One little curiosity remains; today we call these methods based on the postulate, not after any of the originators of them, but after the 18c. cleric who solved the earliest problem in inverse probability, Thomas Bayes, and we term them Bayesian despite the fact that in Savage's book there is only one reference to Bayes, and that in connection with his theorem. Why? I do not know the answer but here is a suggestion. Wald, in the development mentioned above, had proved that the only decisions worth considering, technically the admissible solutions, were obtained by placing a probability distribution on the parameters about which a decision was to be made, and then using Bayes's theorem. Moreover he called them Bayes solutions, using Bayes as an adjective, and although he did not use the term, it is but a short step to the proper adjectival form, Bayesian. This becomes more reasonable when it is recognised that Wald's work had much more influence on statisticians in the USA up to the 80's than did Savage's later work.

At any rate we are left with the term Bayesian as extreme example of Stigler's law of eponymy. Notice that, unlike the distinction between direct and inverse probability, the distinction between us and the frequentists is not over the type of problem (from p to r, rather than r to p) but over the methodology to be used for the same problem. Sometimes we agree, as when we pass from p to r, but we strongly disagree in inferring p from r, replacing their confidence limits by a posterior distribution.

So Bayesian it is, but that leaves the puzzle of defining the term. In direct descent from our true founders, Savage, Ramsey, though not Jeffreys and de Finetti, who were only concerned with inference, not decisions; it could be defined as using the principle of maximisation of expected utility, but I believe there is a better way of looking at the topic and that is to say that a Bayesian is one who holds that the only sensible measure of uncertainty is probability. Or to express the same idea differently and more operationally, statements of uncertainty should combine according to the rules of the probability calculus. There are two attractive features of this definition: first it applies to inference where no decision is involved, and second it gets to the heart of our position in concentrating on probability. Jeffreys and de Finetti were both wise in entitling their books, Theory of Probability, rather than making any reference to statistics, for statistics tackles the inverse problem whereas probability deals with both it and the direct problem. Decision analysts may worry that utility is omitted in this definition but, in response, MEU only makes sense if utility is defined in probability terms, as Savage did. (Ramsey reached probability through utility.) So probability is the fundamental idea. Recognition of the fundamental importance of probability has important consequences for Bayesians; for example, it commends a probabilistic model of your uncertain world. But that is a topic for the future; history has brought us, by a circuitous path to probability, let us use it.

BCal: an automated MCMC facility for Bayesian radiocarbon calibration

by Caitlin Buck and Andrés Christen
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Over the past decade researchers world-wide have contributed to the development of a group of techniques now known collectively as "Bayesian radiocarbon calibration". These techniques allow archaeologists and others to combine chronological information from radiocarbon dating, with that from other sources, to arrive at coherent interpretations of all the available temporal information. See, for example, Buck, Cavanagh and Litton (1996) chapter 9, and references therein. For a detailed description of the software outlined here see Buck, Christen and James (1999).

Radiocarbon dating can be used to date organic matter such as seeds, bones, charcoal and the like. The method relies on the fact that living organisms continually exchange carbon (including the radioactive isotope ^{14}C) with the biosphere. Upon death, this interchange ceases. Since ^{14}C is unstable and decays to ^{14}N with the emission of a beta particle, the proportion of ^{14}C to other, stable, isotopes of carbon such as ^{12}C slowly falls over time (at a rate determined by the ^{14}C half-life which is approximately 5730 years). Unfortunately for those who have to interpret radiocarbon ages, however, the proportion of ^{14}C in the biosphere has not remained constant over time. As a result, radiocarbon laboratories around the world have collaborated to produce internationally agreed calibration data which allow the conversion of radiocarbon ages to calendar dates.

Thus, for some time now the basic technology needed to obtain radiocarbon determinations has been available (costing more than 400 USD per sample dated) and to calibrate such assays for single organic samples. Commonly, however, a set of samples with some *a priori* chronological relationships are radiocarbon dated with the intention of dating not just the samples themselves, but also the contexts in which they were found. For example, pieces of bone taken from an ancient tomb in a pyramid are radiocarbon dated with the intention of dating the tomb and, perhaps, the construction of the pyramid itself. Or, slices of peat from a core are sampled and, using a model for peat deposition, the whole core is dated. It is in exactly such situations that the Bayesian radiocarbon calibration framework is most powerful. It allows us to include contextual information in the form of priors and to calibrate related radiocarbon determinations all at the same time. In this way, we are able to obtain not only posteriors for the age of sampled material but also for related contexts. However, only in the case of simple priors and data can analytic methods be used to compute such posteriors. In most published examples, Gibbs and/or Metropolis-Hastings kernels are used to obtain MCMC approximations of the posterior distributions. Interest then typically focuses on marginal plots of calendar dates and the length of time elapsed between key events in the chronology under investigation.

To applied Bayesian statisticians this sounds challenging, but achievable. However, the real users of this framework are archaeologists and other scientists with little or no background in Bayesian statistics or MCMC. BCal is on-line software that allows users to combine data, models of chronologies and prior information, via MCMC, to arrive at posterior information tailored to their needs. A big challenge here, is that potentially quite complex prior information may be included in the analysis. BCal handles this without difficulty, even when hierarchical priors are needed. After data entry and prior definition, BCal writes code for a tailor-made interpreter *mexcal* which performs the MCMC and outputs information for post-processing by BCal. BCal performs some convergence checking using the batch facilities of CODA and R (the Raftery and Lewis algorithm is applied) and gives the user recommendations about their MCMC output (poor convergence, high dependence, etc.). When the sample passes the predefined checks, BCal stops *mexcal* automatically. After that, posterior analyses are performed and plots for the posteriors of interest are generated. BCal has an on-line step-by-step tutorial and an integrated help system. These, along with other published material, allow even novice users to apply sophisticated Bayesian calibration techniques to their data, building up their understanding of MCMC techniques a little at a time as their interaction with the software progresses.

BCal is Web-based software accessible at `bcal.cf.ac.uk`. Users simply register, and use CPU time and storage space on the server which is physically located at Cardiff University, Wales, UK. This centralised provision was originally devised to allow users with limited access to CPU power (in particular archaeologists) to perform Bayesian calibrations on their own data. However, with increasingly cheap CPU power, a downloadable version is now being considered. In our own applied research we have, however, found the central server and the modular design of BCal to be most beneficial. It allows a group of researchers (perhaps, in different parts of the world) to share the same login and to work on the same project, each inputing their own data and expert knowledge and then bringing everything together when they perform their calibrations. It has also allowed expert users to help novices to get started on their own calibrations even when the expert and novice are on other sides of the world.

Expert users, who would like access to the basic code that is at the heart of BCal, may already obtain the text-based interpreter known as *mexcal* by requesting the C++ code from Andrés Christen (*jac@matmor.unam.mx*). The graphical interface with the data entry, prior definition, MCMC monitoring, and posterior analyses was developed in Java by Gary James and Caitlin Buck (*BuckCE@cf.ac.uk*) and the work was funded by a grant from Cardiff University Young Researchers' Fund. Since its launch in summer 1999, BCal has more that 150 registered users and to date (May 2000) there are a total of 434 projects stored on the Cardiff server.

The automation of Bayesian techniques for use by non-experts is an increasingly important problem. The BCal solution was to concentrate the number crunching and MCMC code in a stand alone program capable of running in batch mode (*mexcal*) and then to write a GUI, which utilises this and other software (such as CODA and R) to make the resource accessible to users around the world.

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PUTTING BAYESIAN DECISION ANALYSIS INTO PRACTICE

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Is Bayesian Decision Analysis easy to use in practice? No, it is not.

Bayesian Decision Analysis (DA) comprises various steps usually implemented in a decision support system (DSS): defining the alternatives and objectives, structuring the decision problem, and modelling beliefs and preferences of the decision-maker. The recommended alternative is the one with maximum expected utility, once all the assignments have been refined via sensitivity analyses. Although conceptually simple, there are a number of difficulties relative to all the steps in the DA cycle faced in practice when solving real large-scale problems.

My first *medical* real application on which I have worked aimed at finding optimal treatments in neonatal jaundice management. We employed an influence diagram that included as decision nodes the admission to hospital and various stages of consecutive treatments, together with a lot of uncertain factors to be revealed before or after making the decisions. At this modelling phase, we had to address an increasingly more complicated problem structure which was even more involved because of the existence of constraints on the sequence of treatment decisions. The solution consisted of defining their domains in a somewhat complex way, using combined treatments of different lengths associated with initial, main and final parts of the full treatment, always meeting the constraints.

As far as elicitation issues are concerned, the big size of the problem as regards the number of variables and their domains implied some huge conditional probability tables. The solutions were to define variables that represent progressive local accumulation and to use noisy OR-gates that only require a few intuitive probabilities of the table to derive the others, once their assumptions based on a model of causal nature have been checked. The saving was of 90% for our problem with 56 nodes. Most probabilities were elicited using standard encoding processes for subjective judgments.

The multi-attribute utility function included goals of minimizing costs of treatment, patient risks and injuries, and parents worries (related to the suffering and/or inconvenience of baby-mother separation). During the utility function elicitation, the assignments were allowed to be imprecise as a means of sensitivity analysis. Its functional form was derived as multiplicative with some additive decompositions.

At the evaluation phase we had to find solutions to cope with our large problem that was otherwise unsolvable since it reaches a size of 1.33×10^{13} memory positions (approx. 25.4×10^9 MB). The evaluation was possible by instantiation of evidence on some nodes which amounts to solve the diagram for each particular patient, instead of providing a general optimal policy for all the patients jointly. Decision tables become then smaller alleviating the computational burden although it is also costly.

We are still working hard on the problem and on a DSS that implements all the phases of the approach. The curious thing of this story is the different way the doctors look at the problem now and before starting our model. They consider jaundice problem as a very complex problem just now! Interpreting the optimal policy and understanding why and where it differs from doctor's common policy will be also complex unless we give them some help. This is our current task: to extract the relevant factors that guide each decision point and throw the irrelevant ones away.

Despite this costly work (at least 4 years long) which is perhaps one of the reasons why you hardly find too many DA applications in the daily practice, it has a lot of potential. First, we gain experience and a very general software kernel (grammar and compiler) to be used in other problems. Second, doctors gain a custom-made tool for understanding better the jaundice problem and judging their routine protocol and the implications due to possible changes in it. Moreover, a decrease of diagnostic and therapeutic costs and risks is attained fighting a defensive medicine.

Also, the tool may be used for diagnosing because pathology prior probabilities are updated via Bayes' rule as long as the diagram is solved. For further details, see Bielza, Gómez, Ríos-Insua, Fernández del Pozo (2000) Structural, Elicitation and Computational Issues Faced when Solving Complex Decision Making Problems with Influence Diagrams, *Computers and Operations Research* 27, 725-740.

My second real *medical* application faces the decision of when extracorporeal membrane oxygenation (ECMO) must be withdrawn to a neonate with cardio-respiratory insufficiency. This is a recent treatment first reported successfully in 1975, considered an alternative to conventional treatments when these are not effective or may negatively affect the baby prognosis. When the infant receives it, he is already at the risk of his life with a probability greater than 0.8. ECMO system is a complex circuit with many mechanical devices and requires up to 15 people being on the watch for its operation. We are developing a DSS for the same hospital *Gregorio Marañón* in Madrid and, now, the time modelling is one of the most important issues. See Mateos, Bielza, Ríos-Insua (2000) Applications of Decision Analysis to Extracorporeal Membrane Oxygenation, Work. Pap., Univ. Politécnica de Madrid. We hope we can implement these programs in other hospitals.

Reservoir operation under uncertainty is another application in which I have been involved. We solved the Zambezi river case within the Bayesian framework (see Ríos Insua, Salewicz, Müller, Bielza (1997) Bayesian Methods in Reservoir Operations: the Zambezi River Case, *The Practice of Bayesian Analysis*, French and Smith (eds.), Arnold, 107-130). Key issues were the use of Bayesian dynamic models to forecast inflows to the reservoirs and a careful modelling of preferences which included a term reflecting deviation from a reference trajectory. The lack of an explicit expression of the expected utility led us to introduce a heuristic providing policies of approximate maximum expected utility subject to constraints on the control variables (water released for energy production and for storage provision) and reservoir storages, and taking into account the dynamics of the reservoir system. Basically, a Nelder Mead algorithm with a multistart strategy was applied to its Monte Carlo approximation. A thorough checking of our policies through sensitivity analyses was carried out to provide additional modelling insights. As a consequence, the operation of the reservoirs was fairly well balanced, both in economic and safety terms. Since the application of this approach is far from simple again, we developed a DSS for reservoir operations.

Finally, a case study about *road project* selection in central Spain tried to avoid practical difficulties in specifying reliable utility functions, see Ballestero, Antón, Bielza (2000) Bayesian Approach to Road Selection with Compromise Utility Functions, Work. Pap., Univ. Politécnica de Madrid, for details. Many decision-makers facing the choice of investment projects in the transport sector often discard the use of Bayesian approaches as they question the way of determining utility functions in practice. We proposed to employ compromise utility forms, which allow the user to become familiar as painlessly as possible with the specification techniques. Although assuming additive independence, the proposed utility forms were general enough with the advantage of being defined by meaningful few parameters. The following criteria were used: expected traffic flow in the road, investment costs, "right of way costs, discounted value of saving in travel cost, noise pollution and landscape intrusion. Even though the Spanish authority made its decision by using other methodology, it chose the same road project as us.

My last words are to advise the reader to be aware of the difficulties found so often in real problems, but without despairing because Bayesian techniques offer us plenty of possibilities.

ARTIFICIAL INTELLIGENCE

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Application of Bayesian analysis in Artificial Intelligence, largely by researchers in Computer Science, is extensive, and is rapidly growing.

Here, we only give a list of expository articles and books that may be useful for learning about Bayesian approaches and applications in this area. We also give a short list of articles with applications - a list, which by no means represents the breath or the depth of applications on this topic. We hope to focus on a specific application area and give a more thorough account of applications in a future issue.

• D. J. SPIEGELHALTER, A.P. DAWID, S.L. LAURITZEN, AND R.G. COWELL (1993). **Bayesian analysis in expert systems. (with discussion)**. *Statistical Science*, (8) 219–283.

This article reviews Bayesian ideas to expert systems using a real, moderately complex, medical example. It illustrates how qualitative and quantitative knowledge can be represented within a directed graphical model, generally known as a belief network in this context. Exact probabilistic inference on individual cases is obtained using a general propagation procedure. When data on a series of cases are available, Bayesian statistical techniques are to be used for updating the original subjective quantitative inputs, and a set of diagnostics are given for identifying conflicts between the data and the prior specification. Details are given on the use of Dirichlet prior distributions for learning about parameters and the process of transforming the original graphical model to a junction tree as the basis for efficient computation.

• E. HORVITZ, J. BREESE, M. HENRION(1988). **Decision Theory in Expert Systems and Artificial Intelligence.** *Journal of Approximate Reasoning*, Special Issue on Uncertainty in Artificial Intelligence, 2:247-302.

This paper surveys the potential for addressing problems in representation, inference, knowledge engineering, and explanation within the decision-theoretic and Bayesian framework. It describes early experience with simple probabilistic schemes for automated reasoning, reviews the dominant expert-system paradigm, and survey some recent research at the crossroads of AI and decision science. In particular, the belief network and influence diagram representations are presented.

• R. G. COWELL, A. P. DAWID, S. L. LAURITZEN AND D. J. SPIEGELHALTER (1999). **Probabilistic Networks and Expert Systems.** Springer-Verlag New York.

Probabilistic expert systems are graphical networks which support the modeling of uncertainty and decisions in large complex domains, while retaining ease of calculation. This book gives a thorough and rigorous mathematical treatment of the underlying ideas, structures, and algorithms, emphasizing those cases in which exact answers are obtainable. It covers both the updating of probabilistic uncertainty in the light of new evidence and statistical inference, about unknown probabilities or unknown model structure, in the light of new data. The book provides an understanding of the mathematical and statistical basis of probabilistic expert systems, and an introduction to this fascinating and rapidly developing field expert systems. The careful attention to detail will also make this work an important reference source for all those involved in the theory and applications of probabilistic expert systems.

• The following book is a good source of articles dealing with (philosophically) different approaches to uncertainty, and includes a chapter with many articles on the Bayesian approach.

• G. SHAFER AND J. PEARL (1990). **Readings in Uncertain Reasoning.** Mogan Kauffmann Publishers Inc., San Matco, CA.

Three more books on the subject with emphasis on Bayesian approach are listed below.

• E. CASTILLO, J.M. GUTIÉRREZ AND A. S. HADI(1997)., **Expert Systems and Probabilistic Network Models**, Springer Verlag, New York.

This above book provides a clear account of progress in the use of probabilistic networks and how they are used to construct expert systems which incorporate uncertainty into their rules. The authors begin with a survey of rule-based expert systems and the basics of probabilistic expert systems and they then build on this foundation by showing how a probabilistic model is built. Subsequent chapters discuss how knowledge is updated using both exact and approximate methods and how Bayesian network models are built. A final chapter provides some case studies of the applications of these methods.

• F. V. JENSEN(1996). **An Introduction to Bayesian Networks.** Springer Verlag, New York.

The above book covers Bayesian network approach for the construction of decision support systems or expert systems. The theoretical exposition of the book is self-contained and does not require any special mathematical prerequisites.

• **R.M. Neal(1996). Bayesian Learning for Neural Networks.** *Lecture Notes in Statistics.* Vol. 118, Springer Verlag, New York.

This book demonstrates how Bayesian methods allow complex neural network models to be used without fear of the "overfitting" that can occur with traditional training methods. Insight into the nature of these complex Bayesian models is provided by a theoretical investigation of the priors over functions that underlie them. A practical implementation of Bayesian neural network learning using Markov chain Monte Carlo methods is also described, and software for it is freely available over the Internet.

• Numerous articles on development and applications of Bayesian methods in artificial intelligence can be seen in the proceedings of the annual conferences of the Association for Uncertainty in Artificial Intelligence at the website: `www2.sis.pitt.edu /%7Edsl/UAI/uai.html`

The following articles deal with some specific applications, such as "key-words-search" and speech recognition. More articles with applications can also be found at the above website.

• D. HECKERMAN AND E. HORVITZ(1998). **Inferring Informational Goals from Free-Text Queries: A Bayesian Approach.** *Proceedings of the Fourteenth Conference on Uncertainty in Artificial Intelligence*, G. F. Cooper and S. Moral (eds.), Morgan Kaufmann: San Francisco, pp. 230-237.

This paper describes a Bayesian approach to modeling the relationship between words in a user's query for assistance and the informational goals of the user. People using consumer software applications typically do not use technical jargon when querying an online database of help topics. Rather, they attempt to communicate their goals with common words and phrases that describe software functionality in terms of structure and objects they understand. After reviewing the general method, several extensions are described that center on integrating additional distinctions and structure about language usage and user goals into the Bayesian models.

• L M. DE CAMPOS, J. M. FERNANDEZ AND J. F. HUETE (1998). **Query Expansion in Information Retrieval Systems using a Bayesian Network-Based Thesaurus.** In *Proceedings of the Fourteenth Conference on Uncertainty in Artificial Intelligence*, G. F. Cooper and S. Moral (eds.), Morgan Kaufmann Publishers, San Francisco, 1998. pp. 53-60.

The above paper deals with Information Retrieval (IR), which is concerned with the identification of documents in a collection that are relevant to a given information need represented by keywords. IR systems may improve their effectiveness (i.e., increasing the number of relevant documents retrieved) by using a process of query expansion, which automatically adds new terms to the original query posed by an user. In this paper the authors develop a method of query expansion based on Bayesian networks, and report the results obtained by their method on three standard test collections.

• G. ZWEIG AND S. RUSSELL (1999). **Probabilistic modeling with Bayesian networks for automatic speech recognition.** *Australian Journal of Intelligent Information Processing Systems*, 5(4), 253-60.

This paper describes the use of a Bayesian network system in large vocabulary isolated word recognition.The authors review the algorithm and network structure, and present results showing significant improvement in word error rate from modeling acoustic and articulatory context with a multivalued context variable.

A COMMENT

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In this issue we present two perspectives on the current job market and how to negotiate your way through the job search process. Dr. Lee and Dr. Laading offer some thoughts and advices based on their recent searches/experiences. We conclude the Students' Corner with the the abstract of the thesis of Dr. Lopes.

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Reflection on Last Year's Job Market

In the current job market for fresh Ph.D. statisticians, career choices can clearly be divided into two categories, namely, academia and industry. A job as a statistician in the corporate world is certainly more rewarding financially with no pressure of tenure-type promotions, but it tends to limit your freedom as to what kind of work you would like to do. I have just stated some pros and cons when working for an industry but I am sure that there are pros and cons for working in academia, too. Whether academia or industry, this major decision has to be made on an individual basis, with personal likes and dislikes heavily taken into account. One simple advice that comes to mind is that before making a final decision, it is worthwhile imagining yourself in each kind of job environment and to seriously think about what aspect of your choice career would be most satisfying to you.

A recent development in the job market for Ph.D. statisticians is the availability of more postdoctoral positions in research institutes. If you are still undecided about your career of choice at the time of graduation, a postdoctoral position might be something to consider. Although a postdoctoral position is not exactly a faculty position in a university, it is nevertheless attractive in two ways. First, of course, you can postpone your decision of whether to join academia or industry for at least two years. But more importantly, a postdoctoral position enables you to sharpen your research skills and gives you a feeling of the research environment in general. It also gives you the opportunity to meet and work with experts in a certain field. These contacts would certainly benefit you in later years. However, there are a few important points to keep in mind. A one-year temporary position is never a good choice, simply because there is not enough time to hone your research skills. In fact, you will be on the job market again essentially after a few months and not after the whole year.

I must mention here that since my inclination has always been towards academics, I do not know much about the job search process for the corporate world. However, I would like to mention a few guidelines, based on experiences of my friends and colleagues. The experience of an internship with a company seems to be invaluable if you know that you will pursue a career in industry. You would definitely have an edge in the job market with an internship experience. If the company that you worked for decides that you are exactly the type of person that they are seeking, you will certainly be looking at an offer. Otherwise, you will definitely be well poised in the job market for companies that need a person with similar kinds of experience that you acquired during your internship period.

Now, let me talk about the job search in academics. If you are planning to graduate, say, in May or August of 2001, then you should certainly start your job search a year earlier, preferably the previous summer or fall. Deadlines for tenure track applications vary greatly for the universities. Some of them set it as early as November, 2000 and some, as late as March of 2001. However, in general, deadlines for most universities fall within the period from December, 2000 to February, 2001. You need to start preparing your job application, keeping this in mind.

Finding the tenure track job advertisements is not very hard. You can find them in statistics news magazines such as the Amstat News and IMS bulletin. Many statistics departments also advertise job openings on the web, while many run their own websites of job opportunities for their students. Among these websites, I find The University of Florida's Statistics Job Announcements site (`www.stat.ufl.edu/vlib/jobs.html`) very helpful and current. By compiling advertisements from these sources, you will almost always get a complete list of job openings for the year of interest.

Job advertisement are specific on what is required from the applicant. Usually, several or all of the following are requested, namely, a cover letter, a curriculum vitae (CV), three to four recommendation letters, academic transcripts, technical reports (if you have written up some of your research results), the abstract of your thesis, teaching evaluations, and an AMS cover letter. Almost all Departments of Mathematics ask for a standard AMS (American Mathematical Society) cover sheet which can be downloaded from `www.ams.org/employment/coversheet-info.html`). Among these, let me briefly discuss the curriculum vitae (CV) and the recommendation letters. The first thing you need to do is to write your CV. You may have no idea of how to do this but don't worry, because you will find many examples of fine CVs from personal homepages. Having written your CV, the next thing you need to do is to ask some faculty members to write recommendation letters for you. Usually, the people who you ask for a recommendation letter are the people you have worked with or someone who is familiar with your work. You need to include your thesis advisor for sure. Also, as a general rule, include someone who can commend you on your teaching abilities. In fact, some universities specifically ask for at least one recommendation letter describing your teaching abilities. It may not be wise to notify your recommenders just days before an application deadline, after all, they are also busy people. Give them about a month's notice at least. Usually, your recommenders will request a copy of your CV and abstract of your thesis, so it is a good idea to have these ready before you go asking for recommendation letters. Even if you think they know you quite well, they might want to know more about you. They may know you like wine and tango, but they may not know what your future research interests are. Wine and tango are important but they should not make it into your recommendation letters without some mention of your research interests and abilities. Lastly, you collect all relevant materials together in large envelopes and send them all. The postal service is usually delighted to see some one spending so much money on stamps.

After the seemingly endless waiting period, you get telephone calls and emails from members of interested hiring committees. The waiting spell usually ends late January with a plethora of phone conversations and lasts till about March. The phone conversations are very similar, starting with " Are you still interested in the position ?" If you say, "Yes", an interview date will be scheduled. If you have a tight schedule, this may not be an easy task. The department needs to consider its interview schedules for other candidates, schedules of faculty members in the department, and sometimes that of the Dean. For this reason, you should try to keep a flexible schedule during the job search period.

Now, let me talk about the interviews. Interviews are usually scheduled from January to March, with most of them falling in February. In an interview, you will typically stay 2-3 days in town. The center piece of an interview is your presentation. If you have not given a talk before, practice enough. A talk is a form of communication. You like to convey to the audience certain message, which may be a way to implement the perfect sampling or your experience in analyzing an important data set. If the audience fail to understand your message after the talk, the talk plainly did not achieve its mission. In preparing a talk, it is worthwhile to imagine that your audience are graduate students who do not work in your field. You will have the chance to meet most of the people in the department. You will be asked many questions. The questions I had to answer over and over again were "Could you tell me something about your research?" and "Could you tell me something about your teaching experience?" Be prepared for at least these two questions. An interview is a two way process. This is, of course, your chance to show them what you are capable of. From the departments' point of view, they would like to know what you can contribute to the department. Contributions can be measured in many different ways, for example, your success as a scholar in your field of research, your competence as a teacher, your ability to conduct collaboration and joint work, your ability to get grants, and so forth. They would also like to know whether you are a person who they can easily get along with. Once a person is hired, they may spend the rest of their lives in the next door office. The other way of the interview process is for you to get to know the department. You need to know the people in the department, the research environment, the area, the housing price, the school system (if you have children), etc...

Finally, offers are made from mid February to April. You will get a telephone call from a person in the hiring committee. You will typically be given two weeks to accept an offer.

Before I finish, let me mention a few aspects about being a Bayesian on the job market. Because I was not on the hiring committee which reviewed my applications, I don't know for sure whether being a Bayesian helped me or not. But, because of the revolution in the field of Bayesian computations and the explosion of applications which can easily be done in the Bayesian framework, I guess that being a Bayesian was favorable to me in the job market. If you are a Bayesian, it would be nice to think of a reason as to why you are a Bayesian or, more generally, what Bayesian statistics means. "I became a Bayesian because my advisor is" is certainly not a convincing reason, even though it has a truth to certain degree. On one occasion, I had to explain the basic concepts of Bayesian statistics to a Dean with geology background. Bayesian statistics was recently introduced to his field and he asked me about Bayesian statistics. Even though I had been asked this question many times, I don't think I did a good job in answering the question. Even worse was that a prominent statistician who introduced me to the Dean was silently listening to my poorly prepared lecture. I did not get an offer from there.

I remember the time I first started preparing for job applications. I had no idea how a job search process proceeds. I hope this little article will help prospective job finders with their hunt. Good luck!!

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Choosing the Job

As a relatively new member of the work force, I must admit that I'm still not sure exactly how I decided to take the job that I did. It was a case of decision making under enormous uncertainty, and I had to contend with the prospects of both two different continents and (at least) two different career paths. After education in the United States, it came time to move back to my country of origin, Norway, but first I went through a long process of considering options in both places. This is a brief summary of a few things I found to be important.

Regardless of where you look or what you are looking for, the most important part of the job hunting process always seemed to be to network. Sure, it was important to keep the eyes open for listed openings, be it on the internet or in the good old paper form, but the really desirable opportunities invariably came up either at meetings or in conversations with people who knew of or worked in the area of interest. Also, if a posting was really interesting, it would invariably be mentioned by a member of the faculty or someone would drop me an email about it. So for me, keeping the eyes and ears open - and letting as many people as possible know that it was time for me to find a job, so keeping the mouth open, too - was definitely the most important part of the process.

Another factor along the same lines would be to find out which conferences are good for networking. My experience was that the really big ones, like the Joint Meetings, are simply too big to really use effectively unless you already know exactly who you want to contact. Smaller meetings, however, like ENAR, seemed perfectly suited for someone with a specific interest, e.g. in the commercial biostatistics area, but who doesn't know exactly with which company or in which specialty.

Another key decision seems to be to decide between industry and academics. Job hunting and interviews are quite different in the two areas, so it really does make a difference in workload for you as an applicant as far as preparation goes. Of course, it's possible to straddle the fence a little longer, too, and interview in both areas, but be prepared that that lengthens the process by a lot and makes the decision not that much easier in the end. Myself, I ended up choosing a job which enables me to straddle the fence a little longer, at a research institute with strong academic ties but mostly commercial funding.

Other than that, my experience is that there are as many paths to a job as there are outlooks on a professional career in statistics, and that enthusiasm and a good presentation gets you an awful long way in terms of generating interest in a prospective employer. AND - there are lots of interesting and good jobs out there for us statisticians, so go for it!!

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Bayesian Analysis in Latent Factor and Longitudinal Models
Advisors: Dr. Peter Müller and Dr. Mike West

This thesis is a collection of studies in the field of multivariate Bayesian statistics and is basically divided into three parts that will be briefly explained.

In the first part we concentrate on model uncertainty in factor models by proposing a novel reversible jump MCMC algorithm that accounts for model uncertainty directly in the model setting. For comparison we apply to factor models a variety of strategies to compute normalizing constants. We study briefly cases where little prior information is available and default analysis must take place. We end with some simulated examples and a real application.

In the second part we use factor models to describe the covariance structure of time series, with special attention to financial time series where the factor variances have a multivariate stochastic volatility structure. We extend previous work by allowing the factor loadings, in the factor model structure, to have a time-varying structure. Simulation-based sequential analysis techniques are used in a couple of real data application, where predictive and financial performance are the main interest.

In the third and final part of the thesis we propose a new way of combining information from related studies. We extend traditional random effects models to random *measure* models by allowing parameters in the model to be partially described by a probability measure common to all studies, and partially by a probability measure that is specific to each study. Both measures, common and specific, are represented by mixtures of normals, where the numbers of components in the mixtures are fixed in a first stage and treated as random in a second stage, in which case a reversible jump MCMC algorithm is need to assess the posterior probability for the competing models. The motivation comes from related cancer studies.

BAYESIANS IN POLAND

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A great statistician of Polish origin (the greatest Polish statistician), Jerzy Neyman, is well-known to be one of founders of the contemporary frequentist approach to statistical inference. Is Polish statistics inherently frequentist?

In 1945 great Hugo Steinhaus, one of founders of functional analysis, after some years of hiding himself to avoid German persecutions, came to Wrocław, because his Lvov University was no longer a Polish university (Lvov had been captured by Soviet Union in 1939–41 and after 1944; now it is a major city in independent Ukraine). The professors moved to Wrocław and started to organize a Polish university. Steinhaus was interested in probability before 1939, but in Wrocław he started to develop his interests in stochastic applications, including statistics. They were strongly influenced by statistical quality control. For Steinhaus the Bayesian approach turned to be the most natural approach to statistical inference. He started to publish papers and to inspire others, mostly in new Polish journals, *Colloquium Mathematicum* and *Zastosowania Matematyki*, the latter one in Polish with English and Russian summaries (the title means „Applications of Mathematics", now entitled *Applicationes Mathematicae*). The first papers were Steinhaus (1948; 1951; 1953; 1954). In Steinhaus (1954 and 1963 for the English version) a discussion with opponents of Bayesian inference can be found. Steinhaus argued among others that even Bayes' uniformity assumption is not irreasonable in comparison with assumptions and patterns of likelihood theory. He opposed to e. g. Feller's attitude resulting with pushing Bayes' Theorem down to a footnote in the famous Feller's book.

The team of Steinhaus consisted of mathematicians from the University of Wrocław and from the Wrocław branch of Institute of Mathematics of Polish Academy of Sciences, with Stefan Zubrzycki and Stanisław Trybuła among others. His circle included also Marek Fisz from Warsaw (University of Warsaw and Polish Academy of Sciences). Fisz (in 1954) and Zubrzycki (1966) were authors of two Polish important academic textbooks on probability theory and mathematical statistics. Both contained chapters devoted to Bayesian statistical methods, in particular in the third edition of Fisz's book (Fisz, 1967); in Zubrzycki's text it is more concise.

S. Trybuła influenced in Wrocław a group of statisticians who studied estimation theory including sequential methods and stochastic processes, with much interest to minimax and Bayes solutions. M. Fisz collaborated with Warsaw statistician Wiesław Sadowski, later well known as Professor of Statistics at Central School of Planning and Statistics (now Warsaw School of Economics).

Sadowski was engaged in decision theory and operations research and in his book (Sadowski, 1960) Bayesian decision making was mentioned. And his colleague Jerzy Greń published the first Polish Bayesian monograph (Greń, 1972) being an interesting introduction to the game-theoretical approach to statistics with unavoidable element of Bayesian paradigm. He considered a number of economic decision problems, starting from statistical quality control. However, the influence of such research on the centralised Soviet style economy was hardly essential, in any scale. Greń was known as an econometrician and „economic" statistician and his collaboration with mathematical statisticians was weak.

In the seventies Bayesian approach was usual in textbooks and lectures as a part (a mode) of statistical inference. In Poland the duality „Bayesian–frequentist" was not a point of discussion (from time of Steinhaus' argument mentioned above). Both Bayesian and frequentist methods were accepted as well founded approaches. Or rather say, Polish statisticians were mostly frequentists who accepted Bayesian approach as a useful method with a good theoretical base in statistical decision theory. It is reflexed by contents of contemporary Polish textbooks on mathematical statistics.

What are we doing now? In Wrocław pupils of S. Trybuła, i. e. Ryszard Magiera, Maciej Wilczyński and their colleagues at Wrocław University of Technology work in Bayesian and minimax estimators and stopping rules. In Warsaw an outstanding Polish statistician Ryszard Zieliński (Institute of Mathematics, Polish Academy of Sciences) who works also in Bayesian methods and his pupils, Agata Boratyńska (University of Warsaw) and Marek Męczarski (Warsaw School of Economics), gave a number of solutions in Bayesian robustness, mostly for optimality criteria derived from robustness and also for some sensitivity and prior classes studies. Jan Kordos (Central Statistical Office), an expert on survey sampling, studied Bayesian methods of estimation for small areas. Cracow is the town of Jacek Osiewalski (and his students) from Academy of Economics, an outstanding expert who has a number of results in Bayesian multivariate models and Bayesian econometrics. In Gdańsk we have to mention Franciszek Grabski (University of Gdańsk and Naval Academy) whose research concerns Bayesian reliability analysis and also Mirosław Szreder (University), a Bayesian econometrician. In Lublin at Agricultural University Teresa Jelenkowska works in Bayesian linear models. A number of other statisticians present Bayesian solutions for various problems. Some recent monographs are: Osiewalski (1991), Szreder (1994) and Męczarski (1998).

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NEWS FROM THE WORLD

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✻ *denotes an ISBA activity*

➤ Events

Foundations of Statistical Inference: Applications in Medicine, Social Sciences, and Industry, and the Interface with Computer Science. *December 17-21, 2000, Kibbutz Kiryat Anavim near Jerusalem*.
The conference will be devoted to the examination of the interrelationships between the foundations of statistical inference and the practice of statistics. It will include formal talks and informal discussions that will focus on philosophical and scientific justifications of the methods of statistical inference, and on analysis and statistical modeling as they relate to real-life applications in science and technology. Web page: www.stat-infer.huji.ac.il.

7th Islamic Countries Conference on Statistical Sciences. *January 2-5, 2001, Lahore, Pakistan*.
Contributed papers related to any theory and applied work in the areas of Statistical Sciences, Management Sciences, Information Technology and Software Engineering, Public Health and Health Sciences, Pharmacy, Crimnometrics and Population Studies are welcome. Submit your contribution by July 20. The conference has no web site; more information is available at the Allstat archives (www.mailbase.ac.uk/lists/allstat/2000-05/0101.html).

Eighth International Workshop on Artificial Intelligence and Statistics. *January 3-6, 2001, Key West, Florida*.
To encourage interaction and a broad exchange of ideas, the presentations will be limited to about 20 discussion papers in single session meetings over three days (Jan. 4-6). Focused poster sessions will provide the means for presenting and discussing the remaining research papers (deadline July 10). Papers for poster sessions will be treated equally with papers for presentation in publications. Web page: www.ai.mit.edu/conferences/aistats2001/.

✻ **ISBA Regional Meeting on Bayesian Applications in the Behavioral Sciences**. *April 5-8, 2001, Laguna Beach, California*.
The topics covered will be Bayesian applications in cognitive science and psychology, economics, philosophy of science, political science, sociology, statistical methodology. There will be eight two-paper sessions (with discussion) and poster session for contributed papers. The timing of the conference is intended to fall between the World Meeting of ISBA of May 28 in Crete, and the Valencia 7 Conference expected in June, 2002. Locating the conference in Laguna Beach is in the spirit of the Valencia Meetings which have all been held on the shores of the Mediterranean. One of the aims of the meeting is to attract researchers from the social sciences which have been under-represented in ISBA. (Local organizer: D. J. Poirier, dpoirier@uci.edu)

✻ **Second Workshop on Bayesian Inference in Stochastic Processes**. *May 31-June 2, 2001, Varenna, Italy*.
People interested in presenting a paper at the workshop (following the one in Madrid, 1998) are invited to contact Fabrizio Ruggeri (fabrizio@iami.mi.cnr.it). Web page: www.iami.mi.cnr.it/conferences/varenna.html

Mixtures 2001: Recent Developments in Mixture Modelling. *July 23-28, 2001, Hamburg, Germany*.
The conference is sponsored by the German Research Foundation, and its topics belong to many of those fields where mixture modelling can be applied.
Authors are invited to submit a paper (by December 31) in one of the areas listed above. Selected papers will be published in a special volume of Computational Statistics and Data Analysis. These will undergo the normal review process of CSDA. Web page: bruce.unibw-hamburg.de/mix01.

➤ Internet Resources

TSAOS, a Web site entitled Time Series Analysis for Official Statisticians, is now available at time-series.jrc.cec.eu.int. The site's main objective is to fill the gap between practitioners and methodologists in the field of time series analysis:

optimally, practitioners should receive there support from methodologists and academics could gather there applied problems.
Since a special attention is devoted to official statisticians and to programs in use in statistical institutes, a large space given to developers and users of dedicated programs.

The entire series of Statistics Notes published in the British Medical Journal is now online at the BMJ Website. A list with links is at `www.sghms.ac.uk/depts/phs/staff/jmb/pbstnote.htm`. This might be useful to those with medical colleagues. As more are published, this list will be updated.

All about Fortran. `www.fortran2000.com` is a new Web directory about Fortran. It contains links to Fortran compilers, libraries, and other products and services of interest to Fortran programmers, developers, and users. Visitors can also rate and review a resource (after submitting a query), if they have the patience to wait for the loading of the numerous banners that appear on all pages.

➤ Awards and Prizes

✻ Mitchell Prize
The Mitchell Prize is awarded annually in recognition of an outstanding paper that describes how a Bayesian analysis has solved an important applied problem. Named for Toby J. Mitchell, the Mitchell Prize was established by his friends and colleagues following his death in 1993.
Toby spent much of his career at Oak Ridge National Laboratory, and made incisive contributions to statistics, especially in biometry and engineering applications. He was a marvelous collaborator, an especially thoughtful scientist, and a dedicated Bayesian; hence the focus of the prize.
The award is made annually under the cosponsorship of ISBA, SBSS and the Mitchell Prize Founders' Committee.
The Prize includes an award of $1000 and a commemorative plaque.
Announcement of Award of the 2000 Mitchell Prize.
The 2000 award is the fourth Mitchell Prize. The Selection Committee this year has members Gary Koop, Henry Wynn and Mike West (chair).
The committee deliberated for a long time over what they took to be lots of high quality submissions in this year's competition. As a result of these deliberations, the committee is very pleased to announce that the winner of the 2000 Mitchell Prize is the paper "Markovian Structures in Biological Sequence Alignments" by Jun Liu, Andrew Neuwald and Chip Lawrence.
The winner was announced at the ISBA 2000 conference in Crete in late May 2000. Jun Liu was present to accept the award on behalf of the authors.
The paper, which appeared in JASA in 1999, represents the authors' broad contributions to, and innovations in, Bayesian modelling and computation for central problems in computational biology. This paper synthesises their work from the mid-1990s, providing a complete framework for automatic model identification and estimation in sequencing problems, and a framework that is well suited to the large scale data base searches that characterise the field. The work involves statistical methodology that represents the frontiers of Bayesian modelling and advanced computation, and provides path-breaking solutions to important and challenging biological problems. The recent upsurge of interest in Bayesian methods for bioinformatics and computational biology is in no small part due to the work of these authors and the accomplishments partly represented in this paper.
The 2001 Mitchell Prize.
The 2001 Selection Committee will be chaired by Gary Koop and constituted later this year.
A call for nominations for the 2001 prize will be forthcoming shortly thereafter.
Information on the Mitchell Prize.
Further information, including details of the charter defining the Mitchell prize competition procedures, is available at `www.stat.duke.edu/sites/mitchell.html`

➤ Miscellanea

Report on "Kitcken workshop" on Model Criticism, *by Tony O'Hagan*.
A workshop on Model Criticism sponsored by the Highly Structureed Stochastic Systems program of the European Union

was held at the Norwegian Computing Centre, Oslo from March 15 to 18, 2000. The subjects and conclusions are as follows.

THE MODELLING PROCESS. The model criticism process is a cycle of 3 stages - model proposal/criticism, model extension, model comparison/choice.

The proposal of the initial model should incorporate all the features thought to be relevant, but otherwise should attempt to be parsimonious. The current model is taken as a "null" proposal, to be criticised without explicit proposal of alternatives. Alternatives are at least roughly implicit in any choice of criticism techniques, but the role of model criticism and the checking strategy is basically exploratory.

Any model shortcomings exposed in the model criticism stage are now considered and appropriate extensions of the "null" model are formulated. The result is a proposal of a range of models, containing the "null".

The range of models proposed in the model extension stage are formally compared. The result may be to choose one model as the new current model. It may also be to propose a mixture of some or all of the models as the new current model (in a model averaging sense).

MODEL CRITICISM TOOLS. The following are the main (groups of) tools.

T1. Comparing data with model predictions. This includes prediction of external, reserved data not used for model fitting. It also includes internal prediction by "leave one out" or cross- validation methods.

T2. Checking functions. This is a similar technique, but the idea is to predict functions of data and/or parameters rather than individual data points. Checking functions look for specific model deficiencies, and should ideally be "orthogonal" to features actually fitted by the model.

T3. Residuals. Both traditional "data residuals" and higher order "parameter residuals" might be thought of as checking functions. However, it was felt that residuals are a particular class for which the relevant distribution theory should be developed.

T4. Conflict measures. Data or parameter outliers might be identified by residuals, but represent part of a wider phenomenon of conflict between information sources. In HSSS models particularly, it was felt that conflict might be directly recognised between the various information sources bearing on each node of the model.

T5. Identifying sensitivity and its consequences. Sensitivity to prior distributions is a perennial concern, and a proposal was made to diagnose such sensitivity relatively simply. The idea involves comparing a parameter's posterior and prior distributions, and examining its posterior relationship with other parameters of interest.

T6. Local sensitivity. Sensitivity to the various model components can be measured in a local sense through derivatives of posterior summaries with respect to variations in those components. Such derivatives might be computed via derivatives of MCMC weights.

T7. Balance of power. The relative strength of prior information and data may be gauged by looking at the extent to which posterior distributions of quantities of interest differ from their prior distributions. This may be compared to our intuitive ideas of where the balance of power between prior and data ought to lie in the model application.

Anyone wishing to learn more about the model criticism kitchen is invited to contact either of the organisers Arnoldo Frigessi (frigessi@nr.no) or Tony O'Hagan (a.ohagan@sheffield.ac.uk).

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