Observations on Graduate Programs in Statistics II

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1. Views Circa 1980-1988

"Prophecy is the most gratuitous form of error," the novelist George Eliot caustically once observed. One often escapes criticism because predictions made in the past are soon either paid no heed or just plain forgotten. Indeed, why would investors continue with the same economic advisors after being given flawed, but albeit honest, counsel? However, one is obliged to review one's own predictions, if nothing more than to ascertain where, why, and how one went astray.

In looking back on a talk I had given in 1980 at a conference at the Ohio State University on the teaching of statistics graduate students, Geisser (1982), I was interested in recalling what I thought then would be important for statistics programs to emphasize. Firstly I had hoped that more statistics would be taught by statisticians in Statistics departments as opposed to other university departments but had little confidence that it would happen. I also felt that there was not enough recruitment of American graduate students to American Statistics graduate departments – a phenomenon occurring in mathematics and most other hard science disciplines as well. I also proposed increasing the stress on the philosophical or logical foundations of Statistics. I don't think that happened. In some other areas I had some modest successes. Bayesian inference has made a substantial impact in the number of published journal articles and in the inclusion of graduate curricula as I had predicted, along with many others. The same is true of the area of model selection in partially replacing hypothesis testing and prediction being increasingly emphasized relative to parametric estimation, in which I was one of very few who foresaw that trend.

I also suggested that one should carefully steer a program towards a middle course in terms of the poles of data analysis and mathematics. I believe now that I was harder on the contributions that mathematics makes to statistics then I should have been as it now appears we have deviated too far from this valuable tool in the direction of raw computation. I had, as almost everybody else, thought that the computer would have an enormous impact on statistical practice particularly in graphical displays and number crunching. It would facilitate more complicated and realistic modeling that previously had not been possible and be especially helpful for the computation involved in bootstrapping, cross-validation, sample reuse, jackknifing and the combining of discrete and continuous data sets. But what particularly intrigued me would be the ease in which a host of alternative computations could be performed on a data set thus allowing for greater flexibility in the most appropriate modeling and analysis of the data. I thought that was particularly appealing because it would counter the views of a stringent Neyman-Pearson or even Bayesian approach where assumptions either regarding error rates, models, loss functions or prior probabilities preceded the disclosure of the experimental results. Now those inferential modes would serve primarily as guides rather than inflexible rules. Although in the past practicing statisticians might try out a very few modeling approaches now computing power would extend them to a host of potential models. I'm not sure as to the degree that this occurred – but it certainly was not as marked as I had thought it would be. The really large issue that I entirely missed was the pervasive use of simulation procedures that arose in the last 15 years such as Importance sampling, Gibbs sampling and Monte Carlo Markov Chain procedures. These methods have become so powerful, especially for calculating complicated posterior and predictive distributions and their use so addictive that the methodology is sometimes referred to as "Bayesian Cocaine." I also now suspect that computing will tend to diminish much of the need for the teaching of large sample theory and the use of the resulting approximations will become obsolete.

Another component of the statistical enterprise that I missed was the rising tide of Biostatistical endeavor fueled by the substantially increased funding of health research especially in clinical studies in the United States. This led to advances in the methodology of survival analyses among several other fields of particular interest in health research as well as the increase in size and proliferation of graduate programs in Departments of Biostatistics and statistical units in medical and public health schools.

2. Current and Future Directions

Predictions are generally based firstly on the extrapolation of current trends, i.e. some will become more important and others less so. Secondly there is the attempt to sort out which of the more recent innovations in current research publications have the capacity to permeate the statistical canon. The foundation of a statistics graduate program depends on a basic canon of theory and methodology.

In a paper, Geisser (1988) I discussed the various dimensions of the field of statistics, namely Motivation, Model, Mode and Method. The Motivation dimension ranges from interest in parameters to interest in observables, with the latter, I believe, continuing to gain ground and thus influencing the Methods dimension by focusing more on prediction and model selection over hypothesis testing and parameter estimation. In Biostatistics, I believe prediction will become primarily more important in deciding whether to continue or abort a clinical trial and which of competing therapies are most suited for an individual patient. For a view on other aspects of the potential impact of the predictivistic approach for growth areas in Biostatistics, see Gehan (2000). With regard to the model dimension, which ranged from statistical models to physical models for particular disciplines, we need to increase the diversity of standard off-the-shelf statistical models as well as developing physical and biological models for specific subject matter studies. Many new physical models in one area are often capable of being applied in others. With regard to the various modes of inference, Bayesianism and pure data analytic procedures will gain further ground over the classical inferential modes based on the repeated sampling principle exemplified by the Neyman and Pearson approach. In particular, enormous data sets are beginning to appear more and more frequently. Currently methods for their analysis are being considered much more in engineering and computer science departments Friedman (2001) under rubrics such as Data Mining, Dimension Reduction, Pattern Recognition and Data Visualization, than in statistics departments. But this is an area that statisticians are obliged to reclaim since many of the ad hoc methods that are being proposed in that growing industry really require skilled statisticians to put them on either a sound basis or to develop more appropriate methods. I would expect more progress in this area which will eventually be incorporated into an area which includes discrimination, classification, pattern recognition and dimension reduction.

I believe we shall also see the recurrence of emphasis on the Design of Experiments and Model Robust Inference (usually called non-parametric inference). The reason for both of these, oddly enough, is Bayesian inference. In the first case experimental design is increasingly being investigated in the light of the Bayesian approach while non-parametrics in the classical sense is making a comeback because the Bayesian approach has as yet not completely been able to come to terms with robustness, although one can also expect more progress in the Bayesian direction.

3. Breakthroughs and Prophecy

Breakthroughs in statistical science are nowhere near as exciting and potentially earthshaking as in the physical and biological sciences. Even breakthroughs in mathematics can be given wide publicity e.g. a proof of Fermat's last theorem, and have virtually no particular human benefits. However, so-called breakthroughs from a prospective or retrospective frame of reference may have important ramifications on statistics curricula and deserve some scrutiny.

About 40 years ago Neyman (1962) published a paper with reference to a certain historical perspective. He lauded various "breakthroughs" within the confines of a "behavioristic" theory (initiated by Neyman and Pearson). He then turned to what he considered to be two new developmental breakthroughs on the Bayesian front, "Empirical Bayes" and the "Compound

Decision" approach – both innovations due to H. Robbins (1951, 1956). Other approaches, a Bayesian approach by J. Cornfield and an elucidation of the relationships among principles of statistical evidence by A. Birnbaum were deemed to be in a phase that was vague and unsatisfactory. A further breakthrough by Robbins, namely "Tests of Power One", was also hailed by Neyman (1971).

Some 23 years later, the beneficiary of Neyman's glowing remarks, H. Robbins (1984), reminiscent of Norman Mailer's "Advertisements for Myself," lauded three "breakthroughs" he had made. Here he listed "Stochastic Approximation" in addition to "Tests of Power One" and "Empirical Bayes". Perhaps he did not believe his "Compound Decision Approach" was as great a breakthrough as Neyman had thought. Interestingly enough the idea of 'Stochastic Approximation" was first formulated by S. Monroe and published by Robbins and Monroe (1951). With regard to tests of power one, Barnard (1964, 1969) first thought up the idea while Robbins independently but several years later was involved in a more rigorous and precise rendering of this concept, Darling and Robbins (1967).

Some seven years later perhaps a more authoritative "Breakthroughs" volume by Kotz and Johnson (1991) listed only one of the four Robbins "Breakthroughs" among 40 considered and that was "Empirical Bayes." However, it remains somewhat perverse that many of the subsequent papers bearing this appelation did not deal with Empirical Bayes as defined by Robbins, but just borrowed the term, Good (1991). Interestingly enough one of the vague and unsatisfactory approaches that Neyman discussed in the 1962 paper was listed here as one of the 40 "breakthroughs" namely Birnbaum (1962).

The reader may well ask why all this emphasis on "Breakthroughs." Clearly the retrospective view of Katz and Johnson (1991), is much easier to defend while the prospectives of Neyman (1962, 1971) and Robbins (1984) are to this date, for the most part, unfulfilled prophecies. This of course brings us full circle to George Eliot's remark in this regard and my own obviously fragile predictions.

Of course one should remember that with regard to inferential theories and methods, there is often a cyclical nature to their interest and activity. Bayesianism, which lay dormant as an inferential mode from early in the last century, reemerged in the 1960's and now enjoys a prominent place in every statistics graduate program of any quality. There are many other lesser examples so one should not rule out the Robbins "breakthroughs" as being forever forgotten.

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RESUME
Je discute les prèdictions, tant correctes qu'incorrectes, que j'ai faites dans le passè sur ce qui allait avoir de l'influence sur les programmes universitaires de statistique. Je passe en revue l'avenir des recherches actuelles et passès, et leur influence sur le canon en domaine statistique.