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My Chancy life as a statistician

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I start with an informal discussion that took place several years ago in the coffee lounge of a leading western university. The topic of discussion was "Education System". I casually remarked to my colleagues that their education system was certainly efficient in imparting knowledge and competitive students could benefit a lot from it. Yet, because of its very structured or formal nature it may not leave much scope to meditative type of students to pursue their own way of thinking.

Yes, broadly students could be classified as "competitive" or "meditative". By no means these classes are not overlapping. A competitive student unlike a meditative student is expected to be quick and sharp while a meditative student though slow may be expected to be "deep". Of course, all qualifications, quick, sharp, deep are degrees and students come in all types.

Anyway, I classified myself as a meditative (and not so much a competitive) student. The education in India, when I was a student, did not exist as a system. This was quite conductive to my temperament. I did not like any of my schools and was happy to stay at home and think of things of my own interest. By the time I got my first degree, statistics was not yet introduced to me. However, I was well read on my own in philosophy, theoretical physics, cosmology, sociology and mathematics. It was clear to me then that if I went for scientific research it would be mostly conceptual, theoretical research clarifying and extending basic ideas of science. But, such a kind of research in physics and mathematics did not have much of a job opportunities. On the othen hand, Mahalanobis' National Sample Surveys had created lot a job opportunities in statistics. Hence, I thought if I could do the kind of research I wanted (that is basic, conceptual, theoretical) in statistics, I should probably not have much of problem of getting a good job.

I took a chance. This came true, soon after my master's degree in statistics, I landed into a lucrative job in the then Bureau of Economics and Statistics, Government of Bombay. I was given double the salary than the one I applied for ! The Director of the Bureau, Mr. Sankpal, encouraged me to pursue my own research interests. My researches soon were directed to survey sampling. In a year (1951), I published a paper in JRSS-B: "On two stage sampling". The paper clearly demonstrated how much confused thinking existed even at a very elementary level of survey sampling practice.

In the year 1934, Neyman read before the Royal Statistical Society his influential paper on survey sampling. The main thrust of the paper was to establish superiority of random sampling over purposive selection. I am not discussing the point here. But, in support of his argument, Neyman claimed that for stratified sampling,

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the conventionally used estimates are unbiased minimum variance in the sense of Gauss-Markoff theorem. Fisher, in discussion of the paper concurred with Neyman only suggesting that Markoff's name be deleted from that theorem.

This is one of the few points on which Fisher and Neyman ever agreed and subsequently on this point both were proved to be incorrect. Both, Neyman and Fisher, overlooked the fact that a survey population, unlike the hypothetical populations for instance in biological sciences (the underlying model here being that of coin tossing), consists of labelled individuals. The existence of these labels renders Gauss-Markoff theorem virtually irrelevant for the survey sampling. Actually, barring trivial exceptions, for no sampling design, unbiased, minimum variance estimate at all exists (Godambe, 1955 JRSS-B). The confusion caused by the search for non-existent unbiased minimum variance estimation is abundantly illustrated by the survey sampling literature published during the period of twenty years or so after Neyman's 1934 paper. I am glad that my 1955 paper helped bring about conceptual clarification.

Earlier I have mentioned that my going to study statistics was a chanchy event that gave a new turn to my life. In 1956 I left the Bureau of Economics & Statistics of Bombay and went to the Imperial College to study Statistical Inference. I had the advantage of discussions with George Barnard and Alan Birnbaum, who was then visiting London. Both of them proposed the Likelihood Principle for inference. They, not being familiar with survey sampling set-up, used the usual hypothetical population model, mentioned earlier. Later, when I investigated the implication of the Likelihood Principle and the related Conditionality Principle for the survey sampling population (with labelled individuals). The conclusion I arrived at puzzled many and shocked some others: -The inference or estimation should be independent of the sampling design used to draw the sample-, (Godambe, 1966, **JRSS-B**). A traditional survey practitioner would never accept this conclusion.

The two results of mine in relation to survey sampling stated above, namely, (i) nonexistence of an unbiased minimum variance estimate, (ii) irrelevance of the sampling design for inference, gave rise to energetic discussions in the statistical world. Ultimately, they culminated into an international conference entitled "New Developments in Survey Sampling" (1983, Chapel Hill). This was organised by the University of North Caroline, Chapel; Hill.

The Chapel Hill conference just mentioned gave rise to a controversy which though sometimes was heated, it also brought about enlightenment. (For names of the participants and other details, see the proceedings of the conference: (**New Developments in Survey Sampling**, (1969), De Johnson and Smith, Wiley Interscience, New York). Many statisticians, specialist of survey sampling and others, started having a fresh look at the subject. In fact, there was a movement to try to reconcile the various approaches to sampling inference.

In 1971, Godambe and Thompson read a paper before the Royal Statistical Society entitled -Bayes, fiducial and frequency aspects of inference in survey sampling-. The book **Foundation of Statistical Inference in Survey Sampling** was published by Cassel, Sarndal and Wretman (1977) in the wake of the controversy. Again Godambe and Thompson presented a paper -"Robust near optimal estimation in survey sampling practice" at Delhi I.S.I. meetings in 1977, showing how much of the survey practice could be explained and justified within the unified theory that I was proposing.

It is gratifying to note my 1955 paper, mentioned above, was applauded several times in the past for its influence in the subject. Again, just recently the paper has been classified among about half a dozen most influential theoretical papers in survey sampling of last two centuries by T. Wright ("Selected moments in the development of probability sampling: theory and practice", Newsletter, US Census Bureau and ASA, July 2001).

Along with survey sampling since (1960) I also have been working with the theory of estimating functions. Its initial result (Godambe, 1960, **Annals**) showing finite sample optimality of the score function evoked only a mild interest in the statistical community. However, general acceptance of the theory of estimating equations came about with the demonstration (Godambe, 1976, **Biometrika**) that the theory can correct the inconsistencies of the maximum likelihood estimation, arising when the dimension of the nuisance parameter increases with the sample size (cf. Neyman and Scott, 1948, **Econometrica**). By now estimating function theory in routinely employed in Actuarial Sciences, Biostatistics, Econometrics, Generalised Linear Models, Sample surveys Stochastic Processes, etc. What the theory actually does is to provide in an unified manner a set of handy tools for the construction of estimates, confidence intervals, tests, for parametric as weel as semiparametric models. The theory can also incorporate in its formalism Bayes prioir, fully or partially specified, when available! all this was amply demonstrated by the papers presented at a recent (1996) conference on Estimating Functions organised at Athens (Georgia) by the University of Georgia

(see, "Selected Proceedings of the Sumposium on Estimating Functions", Eds. Basawa, Godambe and Taylor, IMS Monograph Ser., Vol. 32, 1997). With this I believe estimating function theory yet has many other things to contribute to statistics. Just recently I came across some papers suggesting that bootstrapping of estimating functions could be more beneficial than bootstrapping of the estimates as is conventionally done (Lelel, Kalbfleisch and others).

It is worthwhile to note that in the recent centenary issue of **Biometrika** (March, 2001), an author while summarising **Biometrica's** 100 years of contribution to Theory and Methodology generously devotes a full section to this relatively new topic, Estimating Functions!

The statistical methodology in some areas looks conceptually more unified today, than before. Possibly, I had some nontrivial contribution to this unification. My chancy life as a statistician so far was interesting and productive. Whether it would have been any better had I gone for physics or mathematics the alternatives I once considered seriously, I cannot say.