

Quantitative Methods and Public Policy (A Professional Autobiographical Journey)

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Quantitative Methods and Public Policy¹

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I INTRODUCTION

This is about quantitative methods and public policy and how they shaped my professional career. Although it may look like a professional autobiography, it is much more. I am unfolding the process of doing the research I did; the research I did alone, as well as with teachers, colleagues and students. In describing that process I highlight the importance of working with teachers, colleagues and graduate students. I indicate the relevance of some of the old research in today's research environment. I also list a host of research problems which I did not explore that are still worth exploring. Jeffrey Archer, the famous British author of English fiction, said recently that when he wrote his novels the storyline would just evolve, and that when he was at any stage he would not know how the story would end. But after the whole story was written there seemed to emerge a continuous and logically connected story. The same thing seems to be the case with my professional biography. At any point in time during my professional career I would not have known where I was heading and what I would be doing. But on hind sight there seems to be a central theme to my professional career, quantitative methods and their application to public policy. In this article I not only trace the long professional journey I undertook but also contemplate occasionally on how the switching of trains and the tracks could have affected the nature and quality of my journey. It is a combination of these two that would suggest the lessons that my professional life has taught me that could be of some use to professionals who are at the beginning or middle of their journey.

II PREDOCTORAL PREPARATION (1959-1962)

I was a student of mathematical physics at Andhra University with a specialization in statistics. That specialization was supposed to prepare one for some of the theoretical work in statistical

¹ Prepared for the International Conference on Quantitative Approaches to Public Policy' in Honour of T. Krishna Kumar on his 70th Birthday, jointly organized by Centre for Public Policy, IIMB; Indira Gandhi Institute of Development Research (IGIDR), Mumbai, India; and Centre for Globalisation Research, School of Business and Management, Queen Mary, University of London, UK, August 10-12, 2009 at Indian Institute of Management, Bangalore.

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mechanics and quantum mechanics. It was then that I had an opportunity to listen to a convocation address by Prof P.C. Mahalanobis on “Scientific approach to national development planning”, emphasizing the importance of statistics for national development. Given my exposure to that talk I was indifferent between a career in theoretical physics such as statistical and quantum mechanics or statistics. Tata Institute of Fundamental Research (TIFR) was the best place for the former, and Indian Statistical Institute (ISI) was the best place for the latter. Unfortunately the people who interviewed me at TIFR were all number theorists, a subject not emphasized in my curriculum and which, in my opinion, is mostly irrelevant for research in theoretical physics. They examined me thoroughly and quite rigorously on that subject. It is needless to say that they made my choice by not selecting me. I joined two-year advanced training course in statistics at Indian Statistical Institute founded by Mahalanobis and took a specialization in econometrics and planning. During those days I had the privilege to meet Professor Mahalanobis on a couple of occasions.

Most of the students and research scholars at ISI in 1960 had the privilege to listen to Mahalanobis’ introductory lectures on statistics which he gave to the B.Stat students. Those few lectures were a sufficient statistic to infer that he was a great teacher. He could communicate with ease with the B.Stat students at their own level of understanding. Soon after finishing the training in statistics I joined the Planning Unit of ISI as a Research Scholar. After a year of study there I was offered a Research Assistantship by Karl A. Fox of Iowa State University to work on Quantitative Economic Policy, under a grant from the US National Science Foundation. Mahalanobis’ Draft Second Five Year Plan (Mahalanobis (1955)), Karl Fox’s Econometric Analysis for Public Policy (Fox (1958)), Tinbergen’s books on economic policy (Tinbergen (1952), (1956)), and Tinbergen’s and Chakravarty’s papers on optimal savings and resource allocation for planning with a multi-sectoral economic model, that used calculus of variations (Tinbergen (1960), Chakravarty (1962), (1965)), all dealing with quantitative approaches to public policy, had a tremendous influence on shaping my future professional career².

² The references are organized into two parts, my own and that of the others. References to my own works are cited chronologically and cited by the year and a letter, while references to others’ work are arranged in alphabetic order and cited by the author’s name and year.

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I did not concentrate on a single topic, nor did I stay at a single place to have continuity required to make what I consider landmark contributions. Such continuity at a single academic institution with a doctoral program can provide a research faculty the right academic environment for high productivity measured by academic publications. I can however say that I have had significant consumption benefits and thoroughly enjoyed reading, writing, teaching, and doing applied public policy work. I also hopefully motivated a few graduate students who are taking the relay race forward, and hopefully enjoying it the same way as I have been doing. My work has not been in depth in any single area but it has been quite wide with application of quantitative methods to a wide variety of subject areas such as applied econometric methods, public policies in education, health, transportation, urban development, poverty, development policy, irrigation and water management, and general management.

III DOCTORAL WORK (1962-1965)

In a graduate class on quantitative economic policy dealing with Sandee's linear programming approach to planning, J.K. Sengupta raised the point that the input-output matrix was known only with errors that are probabilistic. My first published paper with J.K. Sengupta ((1965a)) was on obtaining the probability distribution of the optimal solutions of a stochastic linear programming problem with possible applications to planning for national development. Estimation of probability density functions, which originated with this article, would engage me throughout my professional life. My graduate assistantship at Iowa State University not only provided me free time to chalk out an independent dissertation topic and work on it, but also provided me an opportunity to work with Karl Fox in giving shape to his idea of a Functional Economic area as a unit of economic analysis and policy. The statistical analysis of the commuting patterns of workers in 99 counties of the state of Iowa in USA in 1960, drawn from US Census, was my first exposure to dealing with real data. It also gave me an opportunity to learn data handling from Karl Fox, an expert who spent more than a decade in econometric work for public policy in US Government as the Head of the Division of Statistical and Historical Research of the Bureau of Agricultural Economics. While the job assigned to me was laborious, and it was to plot graphs of commuting patterns and to tabulate the economic statistics by sub regions, I discovered new ways of doing them. I used my prior background in physics to treat

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commuting as a force, and my prior knowledge in statistics for grouping sub regions into bigger clusters.

I used the principles of resolution of forces and aggregation of vectors to present a consolidated picture of the commuting patterns through graphs of varying thickness and length, the thickness being dependent on the extent of commuting and the length being the weighted average of the distance commuted by the commuters. I also used Mahalanobis' generalized distance to group the contiguous counties into clusters, with the geographic distance and the number of persons commuting for work or shopping as various dimensions of that distance. This made it easy for Fox to quickly verify his theory that the state of Iowa, with its square grid of roads going North-South and East West, was made of diamond-shaped functional economic areas centered on major cities and extending to an hour's driving distance (60 miles). I could also apply the economic theory I was learning as a graduate student and demonstrate that such FEAs can result if the consumption baskets are labeled by distance one has to travel, and if the utility functions of consumers are separable in terms of goods bought from different hierarchical shopping centers. This work resulted in two research papers with Karl Fox (1965b, 1965c). With these two papers in hand Karl Fox's continued his emphasis on the need for such a unit of analysis for regional planning. With several other papers he wrote on the subject subsequently, he ultimately convinced the Bureau of Economic Analysis of Department of Commerce of USA to define (BEA) Functional Economic Areas and publish economic data for the US economy for each of several (BEA) Functional Economic Areas. This concept of Functional Economic Area, introduced and promoted by Fox, is now widely used by many countries. In the fields of geography and economic geography the Fox-Kumar paper (1965b) is widely cited.

When I started the graduate studies at Iowa State in 1962 a path-breaking study on mathematical theories of optimal control processes, a Lenin Prize study by L.S. Pontryagin and others, came out in print from the Soviet Union (USSR). Jati Sengupta motivated me to study that book thoroughly from beginning to the end and consider application of optimal control theory to quantitative economic policy. Both space research and economic policy dealt with taking a system from an initial low lying position to a final lofty desired position in an optimal way by applying some controls. This is the economic policy problem posed by Mahalanobis (1955),

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Tinbergen (1952, 1956 and 1960), and Chakravarty (1962 and 1965). I did extensive reading of several Rand Corporation research studies conducted for the US Department of Defense on mathematical modeling using differential games and optimal control theory along with several books and IEEE journal issues dealing with optimal control theory. I could immediately relate this mathematical modeling in space research and arms race to the economic policy problem. Pontryagin's Maximum Principle and Bellman's Optimality principle were being used in aerospace engineering and in defense research. That was also the beginning of the critical decade in the era of US space and arms race with the Soviet Union.

When the Americans put a man on the Moon I was not surprised as I could see that while the Soviet mathematicians were busy building nice mathematical models for the motion of a rocket the American engineers, R.E. Kalman (Kalman and Bucy (1961)) being the most prominent among them, were sending rockets, tracking their movements, and numerically calibrating the motion of a rocket without any refined mathematical model³. I could see a unified approach based on the above cited optimality principles. Incidentally those optimality principles of Pontryagin and Bellman seemed to me quite analogous to the century old Huygens's principle in optics, which stated that light travels in waves, and that one can treat each point on any wavelet (of optimally traveling light) as a new source of light. I could see the importance of dual prices in economic policy, and the need to get an optimal plan and also its dual that gives the prices to support the optimal plan. The relation between control theory, calculus of variations, and mathematical programming, so elegantly brought out by L.D. Berkowitz (1961) was a researcher's delight in the sixties, as it opened up gates for new research areas. A researcher's strategy should be to get hold of such research work to discover new problems to work on.

My thesis topic consisted of generalizing the concept of optimal economic policy with a dynamic economic model with inter-temporal dependence into that of an optimal control problem. Like a typical mathematician and a mathematical programmer I posed two questions in that order. The first question I needed to answer was whether the problem was well-posed or not, which is

³ This reminds me of a widely known joke I heard as an undergraduate student (It made rounds in different forms). There were two male candidates being interviewed for a job. Both were put a question—"You are facing a beautiful girl about eight feet away. You are given a rule to approach the girl by covering half the distance between you and the girl in the first step, half of the remaining distance in the next step, and so on. Can you reach the girl?" The mathematician replied that he cannot, while the engineer said that he could, for all practical purposes.

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reflected by the existence or otherwise of an optimal solution. The conditions for existence of a solution should guide one to formulate the policy problem correctly so that it would have a solution and it can be solved. The second question was, if the problem is posed correctly and an optimal solution exists, how to obtain it and characterize it. I answered the first question by deriving the conditions under which a nonlinear optimal control problem or a general economic policy problem with an integral objective function, a dynamical structural equations, initial and terminal conditions would admit an optimal solution. This was my original contribution. My result was an extension of the results given by Tinbergen (1960), and Chakravarty (1962). This work was reviewed and commented upon in 1965 by Karl Fox, Kenneth Arrow, Sukhamoy Chakravarty, E.B. Lee and L. Markus, and Hirofumi Uzawa before I submitted my thesis, and a portion of the thesis dealing with the existence theorem was subsequently published in *Econometrica* (1969a). The second question was answered routinely by applying Pontryagin's maximum principle.

IV POSTDOCTORAL WORK (1965-1968)⁴

I spent a year as a Postdoctoral Fellow at the Center for Research in Management Science, University of California to work further on application of optimal control theory to economic policy. I also used that opportunity to augment my training by auditing a few courses there. While reviewing the draft of my thesis work Kenneth Arrow remarked that I should see the work of Leonid Hurwicz on programming in linear spaces (Hurwicz (1960)). Although the mathematics of Hurwicz's paper was much above my head, I could get the gist of it: that a closed ball in a Banach space is weakly compact. What I was doing in my thesis was establishing closed ball condition through the introduction of functions of bounded variation that satisfy Lipschitz condition, and proving the compactness of the function space through calculus of limits with differentiation and integration under an integral sign. That was a profound observation on my work that Kenneth Arrow made. It opened up a host of interesting economic policy problems for which the existence theorems could have been proved. I took a special case of it, a linear optimal policy problem or programming with linear functionals. I described it as a time dependent linear programming problem with continuous time and showed the existence of the optimal solution

⁴ This period includes one year at University of California, Berkeley, and one and half years at Indian Institute of Technology, Kanpur.

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through a duality theorem (1966a).⁵ Thus, for the linear case, I could relate optimal control model to a programming model, and optimal solution of a primal to the optimal solution of the dual problem. Leonid Hurwicz's paper, like Berkowitz's paper cited earlier, has a profound insight into a variety of problems in mathematical programming, control theory and differential games in function spaces dealing with functionals. Even today people can go back to that paper and extract new researchable problems. I will comment later how I returned to this topic thirty two years later in 1998.

It was my intention to extend the results to the nonlinear case, but frequent dislocations caused by changing of jobs could not see such continuity in thought and work. In fact such discontinuities and my preoccupations with practical applied problems took me far away from mathematical explorations that characterized my doctoral work. A few years later I would have as my colleague at Florida State University's statistics department Morgan Hanson who established the duality theorem for a time dependent and continuous time nonlinear programming problem (Hanson (1968)).

When I showed my dissertation to G.S. Maddala in Berkeley he said that it was incomplete without an econometric application⁶. I told him that there were no readily available computer programs to compute optimal controls. To meet his objection I wanted to get into the problem of writing a computer program in Fortran IV for solving an optimal control problem. Before returning to India in 1966 I was working on a flow chart to write a Fortran Program employing discrete approximations and Using Marquardt's nonlinear least squares program. My move to India made me abandon that project as I lost the momentum due to lack of continuity. Also people like David Kendrick and Gregory Chow had in the mean time prepared such computer programs. Being in India I did not have access to such latest programs. A decade later I was brought back to applied optimal control theory by Manohar Rao of Bombay University, who wrote books on the topic (Rao (1984), Rao (1987)), by asking me to examine the doctoral theses

⁵ Around the same time William F. Tyndall also proved the duality theorem for continuous time linear programming problem. As the conditions I had were different from his, he had a thorough scrutiny of my paper at the suggestion of David Gale who saw both the papers. Tyndall discovered that the conditions I mentioned were wrong by giving a counterexample. This was shown to me by David Gale in 1966 when I was at Berkeley.

⁶ Maddala and I were close friends during the year 1965-66 at Berkeley. While I was a Post Doctoral Fellow with the University of California, Berkeley he was on the faculty at Stanford University, but lived in Berkeley.

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of his students Abhay Pethe and Balwant Singh⁷. While I was in India (1966-68) I was teaching mathematical programming and control theory to undergraduate and graduate engineering students, at the Indian Institute of Technology, Kanpur. One of my students with a rural farm background there wrote a brilliant term paper on application of discrete inter-temporal linear programming to the question of the choice of an optimal crop rotation (1972a).

V PROFESSIONAL WORK IN USA (1968-1978)

After spending one and half years in India I returned to US, to work with Karl Fox on a book dealing with resource allocation problems in universities (1972b). One of the problems I encountered then, and not attempted, was specification and estimation of an objective function that needs to be maximized to arrive at the optimal resource allocation. One of my Ph.D. students at Florida State University, Jean David, and I worked on that (1974d) using the economic theory of preference ordering, Debreu's theorem on representability of a preference ordering by a continuous utility function⁸, and the representability of any continuous function by a suitable polynomial approximation. As far as I know, this was the first attempt to do so in the programming literature.

I then moved to Florida State University (FSU) with a joint appointment in the economics and statistics departments. Teaching of econometrics courses that were jointly listed in both departments was my main task. While teaching a graduate course on growth theory, my attention was drawn to the mathematical elegance of the neoclassical growth theory and the neoclassical production function as presented by Uzawa. Frits De Jong was visiting from University of Groningen, Netherlands and was associated with me in offering the growth course. I had discussions with him on the interfaces between physics and economics and about his book on Dimensional Analysis for Economists, which is one such interface. This resulted in one paper

⁷ I am glad to note that Balwant Singh, a senior officer at RBI today, is presenting a paper at this conference.

⁸ After I completed my Ph.D. degree Karl Fox, who was also the Head of the economics department at Iowa State University, offered me a faculty appointment. I told him that I did not qualify to be a faculty as I did not learn enough of economic theory, and that I want to learn more before I returned to India. He appreciated my frankness as I said that to the Head of the department. He helped me to explore postdoctoral opportunities at University of Chicago and University of California, Berkeley. I went to Berkeley and had the privilege to audit the graduate seminar course on mathematical economics offered by Gerard Debreu, and Linear Programming course of George Dantzig. Our Indian faculty may be interested to know that David Gale and Robert Dorfman, who were quite established economists, were also attending Debreu's lectures. Another person who attended those lectures was Daniel McFadden who just joined the economics department as an Assistant Professor.

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with Frits De Jong on the usefulness of dimensional analysis in economics with application to a neoclassical production function (1972c).

My assigned duties in the statistics department were, in addition to teaching econometrics courses, to teach one introductory statistics course for social science students and to serve as a faculty member of a four member Statistical Consulting Center. At that time I had discussions with Richard Savage, brother of James Savage of the Bayesian statistics fame. He introduced me to Bayesian statistics and asked me to introduce that in my course. But I was not convinced of its usefulness then as it required the prior to be either a uniform distribution or a conjugate distribution. Many years later when that limitation did not exist anymore with the emergence of MCMC algorithms and WINBUGS computer software I did introduce Bayesian inference in my teaching of introductory econometrics at Indian Institute of Management, Bangalore.

The Consulting Center activity made me realize that I was drifting a bit too much into theories and I must get back to applied econometric problems. The first statistical problem brought to my attention by my colleague James Gapinski was the fact that different estimates for elasticity of factor substitution were given by different authors using more or less the same data for the same country. This I thought was due to lack of identification of the elasticity of factor substitution in a constant elasticity of substitution (CES) production function. At that time there were only two publications (Fisher (1966) and Rothenberg (1971)), which dealt with identification of parameters in nonlinear models, and neither of whom gave any practical tools, such as diagnostics or rank and order type criteria to test for identification in such models. In fact I felt then that the text book treatment of the identification problem was focused only on inclusion and exclusion restrictions, and emphasized that identification is primarily a problem in specification prior to estimation of a system of simultaneous equations. This, in my opinion, has done some damage to econometric practice. A return to the basic Cowles Commission Monographs that first dealt with identification would reveal that identification is the limiting case of estimability when the sample size tends to infinity, when the sample becomes the population itself. The problem could be present even in a single equation case, particularly so if that equation is nonlinear in a parameter. So whether a parameter within a system of equations is identified or not can also be inferred by taking a specified model and performing a simulation experiment with increasing sample size.

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This is what Gapinski and I did in early seventies. This approach to identification is a live wire for researchers in econometrics. Identification of an econometric model is a very fundamental issue in econometrics, and this experimental approach has an enormous potential for future research.

I worked on a series of applied econometric papers on estimation of CES production function using nonlinear regression models, small sample properties of nonlinear least squares estimators, and estimation of small sample distributions through parametric and nonparametric methods. In working on these I was very fortunate to have as my colleague James Gapinski. In the first paper with Gapinski I developed a computer program using Fortran IV compiler for estimating the Pearsonian system of probability density functions (1972d)⁹. This was needed for further work we both did on examining the small sample distributions of nonlinear least squares estimators. We demonstrated that elasticity of factor substitution cannot be estimated precisely as the likelihood surface is flat with respect to that parameter (1974a), suggesting that even in large samples the same situation could prevail, suggesting a lack of identification. The small sample density function estimation was applied by us (Gapinski and I) to the nonlinear regression estimators of the CES production function to draw inferences on applicability of the large sample or asymptotic tests in small sample situation, and on applying the linear model tests in nonlinear case with a tongue-in-cheek approach. This was reported in (1974c, 1975b, and 1976a).

The paper with Ephraim Asher showed how an imprecisely estimated elasticity of factor substitution with a CES production function could generate misleading conclusions on factor productivities and total factor productivity (1973a). Asher and I also applied nonlinear CES function for estimating the total factor productivities of market and non-market economies (1974b).

Another outcome of the statistical consultancy activity was the realization that the situation of multicollinearity was all pervasive. The most important question was how best to deal with it.

⁹ Gapinski and I were grateful to the editor of *Econometrica* as he wanted us to explain to him more convincingly why the paper should be published by citing possible applications of such a program, as he could not see any. While answering his question I mentioned that we could understand why he was not convinced of its usefulness as such uses were very rare and hence seemed non-existent. I identified a few applications that Gapinski and I were engaged in for a few years.

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This resulted in my showing that the then existing statistical tests for multicollinearity were meaningless as the theoretical basis for them was on shaky foundation. This was in my paper (1975a). While Gapinski and I did extensive work on estimating the small sample distributions of nonlinear least squares estimator two questions arose in my mind. First, was that method good in all cases? Second, can we use that method to perform a computer intensive research on cataloguing the small sample properties of econometric estimators such as 2SLS, 3SLS, LIML, FIML, etc? I suggested this problem as a thesis topic for my doctoral student Joseph M. Markmann. Markmann and I extended the density function estimation by adding a few nonparametric density estimators. The comparison of the old method of parametric estimators and the new method of nonparametric estimators was done by us and reported at the 3rd World Congress of Econometric Society in 1975 at Toronto. At the Annual meetings of the Econometric Society the same year Markmann and I presented another paper in which we demonstrated that the nonparametric estimation of sampling distributions of econometric estimators using computer simulations could be a good alternative to theoretical approximations. This we did by applying the method to the theoretical approximations Anderson and Sawa used for a simple two endogenous equations case, for which the exact distribution is also known. My move to Abt Associates from Florida State University and Markmann's personal loss and family obligations made it impossible for Markmann to complete his dissertation which was almost 90 percent complete.

In spite of doing applied econometric work and doing empirical work I was not happy with the type of academic work that goes on in academic departments. This is because such research has very little economic policy focus. Hence, when Bernard Sliger, my colleague in the economics department and the Vice President of the Florida State University brought to my attention a letter from a social science consulting company, Abt Associates, located in Cambridge, Massachusetts, inviting a university economist to spend a year with them as a Visiting Social Scientist I readily agreed to try it.¹⁰

¹⁰ There was another major reason for Sliger bringing that to my attention and my accepting it. My family needed some medical service, the best of such service being available at Massachusetts General Hospital in Cambridge-Boston area. Thus the loss of continuity caused by my choice of moving to Abt Associates was due to Hobsons's choice.

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At Abt Associates I had an opportunity to exploit my consultancy experience at FSU and function as an in-house statistical consultant. Soon I was asked to work on two of its major consultancy projects, and both were to do with econometric analysis of data from social experiments. One was the housing demand experiment for the US Department of Housing and Urban Development, and the other was a social experiment on prospective re-imbusement scheme to control hospital costs. In the first problem I was dealing with determinants of participation in the housing demand experiment, with a dichotomous or dummy dependent variable. While Probit, Logit, and linear discriminant function approaches were quite well-known in applied statistics the real issues were (i) to figure out which procedure works best for that problem and why, and (ii) to find a suitable computer program that can handle a very large data base. I had outlined an approach under which a potential participant examines the costs and benefits that depend on various factors and then decides to participate if the benefits exceed cost. I assumed that the benefits and costs could follow a Normal distribution and hence the difference would also follow a Normal distribution justifying both a principal component or a linear probability function approach and the probit approach. This was in 1974-75 before Daniel McFadden's discrete choice models became popular among economists. In an unpublished study I used the demand experiment data and applied the three different methods and demonstrated that all of them yield similar results in terms of the goodness of fit and raised question about the proper measure of goodness of fit to properly discriminate between alternate models.

Another interesting public policy study I undertook at Abt Associates was on measuring the productivity of budgeting function and of that the office of budget director in state governments in USA (1977a). This was part of a larger study on measuring the productivity in state governments, universities, and hospitals, all rendering public services. I used the concept of productive efficiency and frontier production function and their dual, efficient cost or expenditure functions. This was at a time those concepts were at their infancy. I postulated a state expenditure function as a function of different determinants, demand and supply factors, with errors that are distributed in the nonnegative range. I assumed an exponential distribution for the errors. As computer programs were not available for frontier regressions then and as we had large sample of panel data we estimated the frontier regressions iteratively by deleting the sample observations with positive values for the errors iteratively and stopped the procedure

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arbitrarily after three or four iterations. Looking back at that problem now I would like to pose an interesting econometric question. Does that iterative procedure give results similar to the maximum likelihood procedure? Only last year I found a use of that approach in trying to understand what are the minimum resources required by a court to clear the backlog in courts. This was a consultancy project that I did with my colleagues at Samkhya Analytica India Pvt Ltd in 2008 for the Union Ministry of Law and Justice (2008a)

Working on econometric models with real data, and on policy relevant issues, was very challenging and more satisfying than the academic work. Hence, when Clark Abt, the President of Abt Associates, asked me to stay back I resigned from Florida State University's tenured faculty position and remained at Abt Associates. But as I told him that I wish to teach and be in touch with academic world he arranged for me a part-time Visiting Professorship at Northeastern University, Boston. I taught there macroeconomics and econometrics. One of my students in the econometrics course was a student in the sociology department and he introduced me to his Professor William P. Bowers, Director of the criminology program at Northeastern University. He was doing excellent empirical work on the deterrence effect of capital punishment and was quite critical of the work by economist Isaac Ehrlich (1975). Our discussions lead to a joint research project funded by the National Science Foundation. The work I did on the topic was reflected in a paper I wrote with Wen Fu. P. Shih (1978a). I said that the number of murders committed during a period, even if it were a year cannot be assumed to have a Normal distribution, an assumption implicit in Ehrlich's research. I proposed that the dependent variable be regarded as a Poisson random variable with its conditional mean and variance being the same and a linear function of the explanatory variables. I showed that our results differed from those of Ehrlich significantly altering the conclusions. Years later this paper opened up doors for me to teach quantitative methods to law students at the National Law School of India University, Bangalore and to expose myself to the legal theories of evidence, and to the similarities between statistical and legal theories of evidence. There was another student in my econometrics course at the Northeastern University who showed keen interest in my course and wrote a good term paper on estimation of a liquidity trap, the concept that refers to an interest floor to demand for money. Almost twenty five years later Sushanta Mallick revived my interest in that topic and we published a paper on that topic (2001a).

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My exposure to policy relevant consultancy projects exposed me to the need to extend the multiple regression model to situations where the dependent variable is binomial (participation in the demand experiment), Poisson as in the case of number of murders, and exponential as in the case of frontier cost or expenditure functions. All these are special cases of a multiple regression model in which the error term belonged to an exponential family of distributions. This was in an era when the mainstream econometric models were either standard linear regression models or a system of standard linear regressions. At that time (1974) the concept of generalized linear regression models in which the error term of a regression belonged to an exponential family was just emerging, but I was not aware of it then (see Nelder and Wedderburn (1972). Noting that McFadden was motivated to develop the discrete choice model from the choice of mode of transport for a transportation study for the Bay Area Metro in San Francisco region it is clear that consultancy experience can extend the horizons of research.

But soon I realized that a private company environment did not provide enough time to complete a task to my professional satisfaction, and to write journal articles. I therefore preferred to have the best of both worlds- consulting opportunities and academic environment. So, I went to Florida Atlantic University as the Director of Institute for Behavioral Research. Unfortunately there was a dislocation again sooner than expected.

VI PROFESSIONAL WORK IN INDIA-PRE RETIREMENT (1978-1999)

The Director of Indian Institute of Management, Bangalore (IIMB) who was visiting me at Abt Associates a year earlier offered me a Professorship at IIMB. I felt that was the right time for me to decide where I would like to be in the long run, in India or USA. As it is rare that a premier higher educational institution set up by the Government of India would offer directly a senior level appointment I decided to return to India. As it was extremely difficult to adjust to professional environment in India after spending nearly 15 years in USA there was considerable friction to the research momentum that I gathered in USA. Every time there was a dislocation caused by my changing a job or moving between India and USA there was a significant shock to my research activity. A new working professional environment dislocates the family, requires a new orientation and hence a shift in types of problems I would work on. It invariably meant that

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the continuity and momentum in research was sacrificed for other personal advantages associated with the move.

When I was at IIMB I had used my earlier US consulting experience and tried to develop public policy orientation to management. This was mostly in terms of designing graduate curriculum, teaching and consultancy assignments. I worked in the areas of irrigation projects (1979a), housing (1982a, 1987a, and 1987b), health (1982b), urban studies (1988a, 1989-90a), environmental studies, transportation studies (1986a), etc. There were two major studies that I conducted at IIM which resulted in unpublished Final Reports and not in journal articles. These were (i) A Socioeconomic Feasibility Study for the East-West Railway Corridor from Kurla to Panvel over the Thane Creek, For Bombay Metropolitan Region development Authority, (ii) Socioeconomic Feasibility Study of Ganga-Brahmaputra Link Canal for the Central Water Commission. At IIM-B my work was being interrupted by disturbances caused by disputes between the management and the workers union. So, I examined in general and in some detail what factors could cause such disturbances, how to anticipate them and take policy measures to prevent organizational crises. This I did employing organizational theories and mathematical modeling (1984a). This work was a return to control theoretic models for an altogether different type of problems. It was presented at an International conference on Systems, Man, and Cybernetics held at New Delhi. As my professional research was being adversely affected, and as I had little opportunity to teach, my most favored activity, I left IIMB and went to University of Hyderabad.

The move to University of Hyderabad was based on a series of irrational expectations on my part. My expectations were not realized and I ended up wasting time, money, and effort in commuting between Hyderabad and Bangalore, where my family had to remain. I eventually decided to return to Bangalore when the Institute for Social and Economic Change (ISEC) offered me a Professorship and asked me to set up a Quantitative Analysis Unit. One good thing about my four years of stay in Hyderabad was I had a good set of students at MA and M. Phil level. Some of those students came to work with me for their Ph.D. in Bangalore at ISEC¹¹.

¹¹ The two main organizers of this conference, Srijit Mishra and Sushanta Mallick were first my students at University of Hyderabad and later at ISEC.

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Being back in the econometrics mainstream at ISEC I contributed articles to the Journal of Quantitative Economics (JQE) published by the Indian Econometric Society of which I am a Life Member (1992a, 1993a, 1994b, 1995a, 1995c, and 1995d). The first paper (1992a) arose as a comment of a paper I reviewed for JQE, and it highlights not only the importance of our having to use proxy variables due to paucity of data, but also what characteristics such proxy variables should have. This concept of a proxy variable is quite similar to the concept of an instrumental variable in econometrics. Hence what I said in that paper applies equally well to the method of instrumental variables. An instrumental variable estimator obtained by using bad instruments must be suspect, even if it is supposed to give us a consistent estimator. The paper on Probability Approaches in Economics was a tribute to Haavelmo for his Nobel economic science prize (1993a). In particular it traced the origin of econometrics, stressed the importance of modeling the entire probability distribution than simply its first and second moments as is done in most econometric models. The paper (1995a) dealt with the latest econometric concept of cointegration and error correction and related it to Frisch's concept of multicollinearity and Tintner's Variate Difference Method. This is the advantage of a third generation econometrician trying to bridge the work of the first generation with that of the fourth generation of econometricians. I suggested comparing the two methods empirically through an example. This is as yet an unexplored topic for research. This was a topic that was engaging my attention at the end 1995, but I abandoned it as I moved to ISI from ISEC and did not have the assistance of a graduate student to do applied time series analysis.

On July 4, 1991 when the high dose of devaluation of Indian rupee was announced as a cure for the balance of payment crisis the immediate question I asked was "From where such a number 19 % of devaluation comes from?" Given my sustained interest in quantitative methods for public policy that was a very natural question. The answer to that question led me to the belief that econometric models for public policy cannot be built ignoring the geopolitical environment. I wrote a series of articles in Economic and Political Weekly on the need to re-orient our economic policies in the Mahalanobis' tradition but under a newly emerging global economic scenario making open economy a necessity. As 1993 was the year of PC Mahalanobis' birth centenary I had written an article in Economic and Political Weekly (EPW) saying that instead of

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forgetting him one must remember him (1992b). While in India we forgot Mahalanobis, the French remembered him and devoted an entire issue of their economics journal, *Economie Applique*, for articles on Mahalanobis. I had written an article there, at the invitation of its editor, on Mahalanobis' contributions to economic planning and the relevance of that work today (1994a).

I studied carefully the history of Brettonwoods and creation of IMF and the World Bank. From that reading I could distill a clear message that the so-called reforms through structural adjustments cannot be seen from the looking glasses given to us by IMF and the World Bank, and every country must examine them through their own looking glasses. This message was communicated through my article on Fund-Bank policies in EPW (1993c). This was also the time I had high hopes of building a team of dedicated researchers to carry out my dream project of econometrics for public policy with an Indian perspective. I had two Ph.D. students (Nagaraju and Mallick) to help me with the project. Both were to discuss with me regularly the literature on Indian economy within a reform environment and econometric methodology of building macro models using the new time series methodology. Nagaraju was to work on evaluation of alternate econometric models for his thesis and Mallick was to develop for his thesis an econometric model for an open Indian economy to address policy issues related to structural adjustment. This project could go on just for a few months. After that I was to have another student to join this group, Srijit Mishra, but it so happened that he wished to work on an entirely different topic and wished to continue the program with me only if he could register for Ph.D. with I.S.I just the same way that Mallick did. In order to extend such facility ISI wanted me to associate myself with them as an Honorary Visiting Professor by interacting with their Economic Analysis Unit for about half day every week. ISEC did not permit me to associate with ISI on a part-time visiting basis. This was in spite of my suggestion that whatever honorarium I receive from ISI would be deposited with ISEC, and that I was willing to do so mainly for the sake of my Ph.D. students. In anticipation that it would be approved by ISEC, ISI had earlier approved me as Guide for its doctoral programs in economics and registered Mallick into their program. For the sake of continuity of doctoral work of Mallick with ISI, and for my own sake to have a better academic environment for quantitative economic research, I left ISEC and went to ISI on a full-time basis. This created a professional dislocation again for me. After seeing all this Mishra did

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not want to take chances and went back to CDS, Trivandrum where he came from after doing an M.Phil there. Fortunately for Mallick, he could continue to work on the same topic and complete his doctoral work from Warwick University under the guidance of Kenneth Wallis with a commonwealth scholarship. Nagaraju had immense degree of patience and completed his Ph.D. degree staying at ISEC and seeking my inputs even after I left ISEC over a period of six years. He completed his thesis a year after my retirement!

While Nagaraju wrote an excellent thesis on econometric model comparisons, which was reviewed and appreciated by Ray Fair of Yale University, he has not published any articles from the thesis. His thesis was judged as the best doctoral dissertation during that year from Osmania University. Mishra showed excellent potential for independent research by proving a conjecture I made on the probability of obtaining pure strategy equilibrium in a matrix game (1997a, 1997d). Mallick demonstrated his high professional competence and drive for rapid professional growth by publishing papers while still working on his dissertation (1995c and 1995d). How great it would have been for all four of us, and for ISEC, if we all were to continue to stay at ISEC and carry out my dream program of quantitative economic research of high quality in association with ISI¹²! The academic institutions must note that this is where lies the importance of providing enabling academic environment to get the best out of the synergic forces between bright students and experienced researchers.

In the absence of such a team work on my dream project I settled for a lonely armchair research. I wrote a series of articles, some professional and some semi-professional in Economic and Political Weekly and some elsewhere, all of them dealing with the structural adjustment and thereafter. In the academic year 1994-1995 Ashok Mittal took sabbatical leave from Aligarh Muslim University and spent that time at ISEC. He had an enviable degree of determination and was determined to catch up with the latest time series econometrics working with me and Sushanta Mallick. He and I wrote an article applying the concept of cointegration and market integration to the world tea market (1995e). As tea was an agricultural commodity, trade in which was permitted by the Indian Government for many years before economic reforms, we

¹² In one sense the organization of this International Conference by Sushanta Mallick and Srijit Mishra is to cherish that excellent learning environment we all created for ourselves and make amends for the failure of our dream project.

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examined the implication of market integration of Indian agricultural commodity markets with the world market through liberalization of trade in agriculture. Based on our econometric investigation we suggested that one should exercise caution in following the advice of C.H. Hanumantha Rao on liberalizing trade in agriculture, as it could raise prices paid for essential food items by a common man.

In a paper presented at a seminar on Development Planning: Its Relevance and Approaches, organized by Erasmus University, Netherlands, Planning Commission and Institute of Economic Growth during March 1996 I had argued that the economic reforms fell short of our expectations mainly because we accepted the reforms suggested by IMF without a serious examination of how such reforms work in our country and by not making the reforms suitable to our country's specific needs (1996b). As a typical example it was mentioned that the reform concepts of exit policy and safety nets are not good for us. Instead we should adopt the scheme of privatizing all loss making public sector units and provide alternate job opportunities through incentives to the displaced people. Once privatization of public sector units takes place in a high growth Indian economy the private employment so created would make irrelevant the concept of a safety net.

In another article I examined the economic reforms in the guise of "Washington Consensus" and argued that such a notion of a consensus goes against the basic principle of economic policy making. The basic economic policy principles call for country's own objective function with its own econometric model. It is extremely unlikely that one can come up with the same package of reforms for a variety of countries with their own objective functions and economic structures (1997b). I took up the threads from this paper and asked the question how a set of different countries should plan for optimal strategies in a competitive global economy. While for a closed economy a policy model with a dynamic economic structure turns out to be an optimal control problem such as what I did for my doctoral dissertation, in an open economy setting under globalization the optimal economic policy making calls for a differential game approach. Working on this topic has some nostalgic aspects. My extensive reading of Rand Corporation reports on mathematical modeling for space and arms race in the sixties made me aware of the importance of game theory when different players choose their strategies based on their own interests. Taking a special case of either a two country example, of a small developing country

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dealing with a large developed country or South countries as a group dealing with the North countries, I explained the nature of the problem and its solution in a technical paper. I presented it at the Fourth International Conference on Optimization Techniques and Applications at Perth Australia (1998a). There is an extensive scope to do applied econometric work using methods for solving differential games through the solution of associated Riccati equation (Basar and Olsder (1982))

I had three small notes published in JQE after my retirement. One of them was on how to specify a VAR model (2004a). This arose as an academic disagreement between me and an officer of RBI, with whom I was associated, to do a Development Research Group study on building an econometric model for the financial sector. As he used some sections of a text book in econometrics to justify his point I chose to publish my point of view as an article in JQE. It is because of that disagreement, and my moving from ISEC to ISI, and a back pain I was having that I had to abandon that project before it was completed. Two other notes were based on a paper written by Marwa and Klein on the impact of economic reforms on the total factor productivity (2003a, and 2005a). I disputed the policy conclusions they drew on the basis of their econometric investigation. By referring to Haavelmo-Lucas critique I said we cannot make a policy recommendation for post reform period with a different economic structure based on a sample of observations prior to reforms, and secondly that one cannot use the experience of other countries in South Asian region with different economic structures to draw policy conclusions for India.

I had a series of papers dealing with measurement of poverty with V. Sitaramam (a bio-chemist) and others. It is interesting how the problem arose and how the collaboration took place. I was a member of a Committee set up by the Indian Council of Medical Research (ICMR) around 1980 to advise ICMR on the needs of its computer centre at the National Institute of Nutrition. V. Sitaramam was a senior scientist there working on poverty measurement with NSSO data and National Nutrition Monitoring Bureau data. I had known him ever since but lost touch with him until 1991. When I visited Gokhale Institute in Pune to give some guest lectures, I came to know he was at University of Pune as Professor of Biotechnology. I went to his laboratory. He explained to me the work his team was doing on catalysis and how the model used by them in

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catalysis can be used to measure poverty. He showed me a draft of a paper on poverty written by him and Anil Gore. I could immediately see that the saturation function that the biochemists use in catalysis is very much like an Engel curve that economists use for essential commodities. As poverty is referred to as consumption deprivation of an essential commodity his idea made an excellent economic sense. In fact it made better sense to me than using an arbitrarily defined poverty line. I joined them and three of us wrote our first paper on poverty by suggesting that we can define poverty using all but the focus axiom of poverty (1996a). Simultaneously we estimated Engel curves for cereal and all food using NSSO data and found that the specification used by biochemists for the saturation function fits much better than the specifications normally used by economists. We applied this method and derived a natural hierarchy of needs and showed that cereal consumption saturates at levels of income, total expenditure being a proxy for income, much less than other commodities. We then used cereal consumption deprivation as a poverty measure (1996c).

VII POST RETIREMENT WORK (1999-2009)

Almost a decade later when I was going to USA I stopped at London and Mallick and I discussed the need to revive that work on poverty. Mallick took the initiative and obtained unit level (household level) data on consumer expenditure from NSSO for the three quinquennial rounds (1987-88, 1993-94, and 1999-2000). We estimated poverty using consumption deprivation methodology developed in 1996. In that paper we used cereal deprivation measure. The empirical Engel curve did not saturate as expected and stated earlier. We argued that at higher levels of income consumption of food is a luxury. So, we determined empirically, through an iterative algorithm, the point where the Engel curve turns from concave shape to a convex shape and used that as the cut-off point to estimate the consumption deprivation (2009b). As consumption basket is different in different parts of India, and as cereals are not consumed in isolation and need some food complements we extended that analysis by replacing cereal consumption by food consumption. This analysis was presented as PV Sukhatme Memorial Lecture at the Annual Meetings of the Indian Society of Probability and Statistics in November 2007 (2008b)

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After my retirement from ISI in 1999 I spent a year at Arizona State University-West's School of Management as a Visiting Professor. I was assigned an office within the Accounting Department and I came in touch with the accounting discipline through my conversations with colleagues there. The question of valuation of intangibles attracted my attention. In a term paper prepared by one of the executives of a mining company in Phoenix, dealing with the merger decision of his company with another, the merger decision depended crucially on valuation of intangibles such as the goodwill, brand name, nature and quality of workforce and work environment, etc. In particular the work by Baruch Lev of New York University attracted my attention.

After I returned to Bangalore in 2000 I started teaching quantitative methods and managerial economics to management students again, after a gap of fifteen years, at Indian Institute of Science (IISc) and IIMB, as a guest faculty. The office allotted to me at IIMB was next to the editor of IIMB Management Review. I endeared to him and his staff through my nostalgic remarks that I started the journal more than twenty five years ago. I have been a contributor and a referee to IIMB Management Review (2005 b, and 2005c). The first article was based on my reading of Baruch Lev's work on valuation of intangibles. In that article I made a conjecture that stock market bubbles could partly be due to our ignorance on valuation of intangible assets. I had several suggestions in that paper which can serve as the grist for a researcher's mill. In particular I said one has to use the risk metrics to value risky assets, and combine more judiciously than the industry practice of combining the valuation of assets of different risks. I also suggested that as people trade tangible assets and intangible assets, or people often take allocation decisions between tangible assets and intangible assets, one can use that trade-off as an implicit price for valuation of intangibles¹³.

After retirement I was visiting USA frequently to visit my sons. I used to observe that different parents used to visit their children for different lengths of time. I noted that those who have grandchildren stay there longer, and those who still continue to do some work in India return early and so on. That was the time Ashutosh Kulshrestha and I applied the benefit cost calculus to the decision on retention and attrition of employees in a software company (2005c). I had an interesting correspondence with a close relative of mine on how long he can stay away from

¹³ I owe this point to my Professor Karl Fox who wrote a book and several articles on social accounting. Fox and I have been in constant touch both professionally and personally, and we used to exchange our work.

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home and with his son and family abroad. In a lighter vein I applied the same economic benefit cost analysis and valuation of intangible assets and said those considerations applied in his own case should guide him in his decision.

In 2007 there has been considerable controversy, discussion, and debate on the reservation or mandatory affirmative action in admissions for academic institutions at all levels. The issue was so confusing that I seemed to have taken opposite positions on the issue within two weeks! It then dawned on me that not only I, but the lawyers, judges, policy makers, educationists etc may all be acting with inadequate understanding of the education system and its gradual progression from elementary education to higher education. I also felt that there was not sufficient empirical evidence to base once opinion on such an important public policy issue. This resulted in my deliberating on this issue and doing some extensive reading on the topic. This resulted in my article in EPW (2009c)

IIMB Management Review started a new feature called Educator's Forum. I contributed the first article under that Forum on the need to use cutting edge statistical modeling with a competitive spirit of extracting the maximum information from prior and sample information to arrive at the best pattern and least ignorance (2009a). This paper is based on my efforts to make econometrics course salable to aspiring business graduates and to market statistics through a business analytics company that I started with a few friends.

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(1999 May resigned/retired from ISI, Bangalore. As the extension of retirement age was being dilly-dallied I resigned and went to USA for a Visiting Professorship at Arizona State University-West, Phoenix)

(2001 started working as Guest faculty at IISc first and then at IIM-Bangalore)

2001

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