

REFLECTIONS

Esfandiar (Essie) Maasoumi

Department of Economics, Southern Methodist University, Dallas, Texas¹

maasoumi@mail.smu.edu

“Reflections”, such as the one recently written by Peter Phillips for this journal are helpful to other researchers, particularly the early career researchers, who often must invent their way through this “business”. Peter wrote on his “typical day”. I was encouraged to write on my professional experience as well. Surprisingly, I discovered myself too modest to put pen to paper for a long time. But perhaps this type of writing is instructive precisely because it is so intensely personal.

In my case “reflections” come naturally and may also contain a clue or two for a large number of “insider-outsiders” of our profession, including the army of economics researchers who have made the Anglo-Saxon/European academic world their home. I have lived, studied, worked and taught on four continents. All of my university education and degrees are from Britain, the London School of Economics, in the late sixties to 1977. Other than two years of teaching in England, at the LSE and Birmingham, my regular jobs have been in the USA, in practically all of its culturally distinct regions! The latter two are the best known systems to me, but I have been privileged to have spent long visits to Australia and other parts of Europe. And, I am exceedingly grateful for, and proud of my early education and old cultural values from Iran, the land of Avesina (Abu-Ali-Sina), Omar Khayyam and, at any moment in history, dominions of hard to suppress spirits. To put it politely, we are genetically disinclined to “follow”! Which goes a long way to explain my choice of an academic career, and especial love of freedom of thought and choice, especially in research topics.

History is almost everything! The background just summarized is remarkably predictive of both my daily and long term activities and choices. My days and weeks are extremely chaotic, pulling me from one deadline to another, from one necessary read to the next, from one composition in economics to another in econometrics, from one lecture to another. My choice of social welfare function being sufficiently altruistic, I am happy that, for a long time, there had been no wife or children to suffer long-term consequences of this life style. I am newly wed, and my wife Lynette joins more than a few friends and family members who have tolerated this academic lifestyle with uncommon grace.

"Normal" Days, and Teaching.

Since nothing had until recently been able to compete with work, even a simple appointment or committee meeting will give “work” an unfettered priority. My days are now unequally divided between the university office and my home office. I am fully prepared to work at home and, even in terms of interruptions, it is still a little less disruptive to accommodate attempts by my dog (Spaetzle) to grab my attention at home compared to the university office. My wife is at work while I work at home.

Quite a lot of time and effort goes into editorial work and evaluations of writings, grants, promotions, exams, etc. *Econometric Reviews* (ER) took a good deal of my time and attention during its formative years. It is enormously challenging and rewarding. Modern facilities and methods have eased the clerical component of this job, but the

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substance remains as demanding and deserving as ever. It is especially demanding of reflection, time, and judgement to maintain ER on top and yet more open to as large a community of scholars as possible.

When teaching time comes along, quickly and miraculously I am transported into a different zone. Not much continues to exist a little before or during teaching, or indeed most other presentations. Except at the LSE and Birmingham, I have almost always taught students who were, on average, less well prepared than their peers at the top schools. For reasons that are not so well hidden in my background (!), I take special pleasure in having the challenge of trying to reduce the differences in the final product. After every final exam is graded with childlike anticipation, the heavy emotional toll sends me away, often far away, since the occasional success, while gratifying in the longer run, is just not enough for the moment.

In recent times I have not had the luxury of spending a lot of time with students who are writing Ph.D. level theses. We have a very small program that has allowed no student to specialize in econometrics for about 6 years. I cannot emphasize the enormously positive impact graduate students can have on both the volume and depth of research produced within a community. They make it possible to get deeper into issues, explore important related questions, and can force you to reflect on why topics are important or useful in practice. In his ET interview, the late GS Maddala claimed that he got into most areas because of his students' interests (see the ET Interview by Kajal Lahiri in *Econometric Theory*, 1999). My late friend GS was well known for his modesty, but we have all experienced the phenomenon to which he so charmingly alludes. With such small numbers of graduate students at SMU, I am baffled by our extraordinarily high rankings in econometrics; see Baltagi (1997, 1999).

Teaching a classroom remains, however, the final refuge. The one place for communion, building, changing, enabling and receiving that is the least changed. A constant place of challenge to give "them" what they need without forgetting what they can take, or make of it.

I almost never turn down a student or colleague's request to meet and discuss work. I also never turn down a request for evaluations of current or former students and associates. When refereeing requests from other journals seem to go overboard relative to the current load, I inform the requester promptly. It is extremely damaging and selfish not to do so. It is myopic and dysfunctional to neglect correspondence of this kind. We do get paid for "professional activity" and "service", both monetarily through our salaries, and in kind when our own work is evaluated. A quick read will plant a submitted work in one's mind. The review of it begins to develop in the background almost effortlessly. Related work gets noted, enriching the review, as well as one's command of the subject matter. There comes a time, hopefully before a reminder from the journal office, when you prefer to write down the review rather than carry it in your head. The latter aspect is a "cost", and one should not accept to review when such cost is too high or untimely. Because of my modest work outside of "econometrics", I do get papers to referee, or books to review that are over my head. Even though it is not uncommon for editors to receive casual "opinions" from the older professionals in lieu of careful referee reports, I try to avoid the practice. There is both an obligation and a desire to referee work that is closely related to one's own. It is important to deal squarely and openly with the "moral hazard" issues involved in these instances. I am glad to say that I don't face this issue as often as some of our stars must, certainly not as much in econometrics as in "economics".

I keep hours as needed, which means quite irregularly and, sometimes until very late. The clerical component of our work can be back breaking, especially now that we have all technologically arrived. I do almost all of my own word processing and data-basing, scheduling and receptionist work is done by office assistants, when I can get one who is both good enough and generous enough to stick around for the type of pay at most "big city" universities in the USA!

My hobbies include exercise, reading, music and movies/plays, entertainment, securities markets, and travelling. Time left for these hobbies has been insufficient. My wife is helping me recover the glorious balance I had in my student days in London.

On the Road Again, and Again.

The only way I break this schedule is by travel, of which I do a great deal, mostly to be closer to my many co-authors and professional friends and associates. Keeping in touch with other people and developments takes a bit more effort when, unlike a few years ago for me, one is not sitting at a gateway with a strong oral tradition and intellectual buzz. I go to a lot of meetings and visit a lot of universities. I find that if you give some lectures on your visits you get more deeply involved with others and the life of the place.

Travelling is one of the great fringe benefits in our profession. I have been the recipient of great generosity and hospitality by numerous members of this profession. Large meetings are less enjoyable for me than smaller, thematic, ones, or individual visits. I find the ones I enjoy more are also the ones that teach me more, both about my own work and others'.

There used to be an "oral tradition" in our profession and within our departments. That is almost non-existent now, certainly within departments. Some are fortunate to be co-workers in the same department. But that is not quite what I am nostalgic about. LSE had it in the late sixties and the seventies. The whole community had it when it was small enough, and when you were "productive" if you had a notable paper (published or not) once every few years. Nowadays, most people receive their first reactions to their work through the journal review process and after publication. This is where the best schools are somewhat different and so advantageous to serve at. It isn't that they have such lively interchanges, but you have an opportunity to have your work examined and noted early by good readers. This makes a first job at one of the top institutions a very good investment. There is no loss really in such a career path, except if you fall in love with the ivy leagues and forget the mission.

It is very good to spend some time at the better institutions when you are on the road, but don't hold your breath. You never know where the best feedback will come from next.

Research

I find there is much more reluctance to "debate" among people about their work, assumptions and findings. As was noted earlier, this is partly because the oral tradition is considerably weaker than when I was a student. But I do not find that opinions are either lacking or more moderate! They are just not expressed as often or as forcefully in open interchanges. For most people, papers do not profitably circulate for comment before they are sent to journals. This is quite a negative development for the orderly progress of knowledge. It is also quite perilous for the younger scholars to use the journal "review process" to fish out comments on their work. They can develop undeservedly bad reputations which may be very hard to repair. People are too busy to reclassify a "bad author" in their "book". Part of this is due to the much larger size of this profession; it is just so much more anonymous. Part of it is due to balkanization following unhealthy levels of specialization. People are quite unembarrassed to say they do not read certain journals! It is apparently "OK" to say, "sorry, I don't do time series 'stuff'", or, "I don't do micro stuff"! The problem is that our subject is much more matured in its development and applications, making for an amazing volume of printed and typed matter. Also, "economics" has become one of the most demanding fields which requires deep information from mathematics, statistics/econometrics, economics, finance, politics, and other social sciences/domains. Long gone are the days of being ill prepared to entertain

a famous colleague in the "senior common room", or the Shaw library of the LSE, who might initiate a conversation about an idea in the latest issue of any journal.

"I do econometrics", and "I do economics". I pay all the enormous costs of not specializing on a topic. But there is no real "personal" cost, not in development, and not in overall utility. I am reasonably certain that there is also no net social cost. But you have to be strong to live with even close associates in any specialization who may not know what you are doing at any point in time, or even forget about you.

From the very beginning, my own choices have been profoundly influenced by a constant awareness of additional opportunity costs faced, or perceived, by a scientist from a "developing country". Assuming one is wise and knowledgeable, my lot burns with the questions like this: How does a successful career in the research institutions of the "west" compare with a little act here, a wise word there, a constructive role in some committee, and a little teaching that may one day add up to a little positive change in the life of generations of less economically blessed? There are surely no definitive answers to such questions. Everyone must struggle with his or her own personal circumstances.

For me, the choice of topics has always had to deal with this additional complication. I thought better planning and policy analysis may be possible by better forecasting methods in systems of macroeconomic relations. So I have worked in this area since 1973. I believed rigorous analysis of welfare, what it is, how it may be measured and quantified are fundamental issues in economics. An econometrician can play a desired and much needed role in developing implementable concepts of aggregation, multivariable indices, mobility and inequality, which can be applied with modern statistical standards. The last time I checked economics still promised to say something about the creation and *allocation* of wealth and income. And history, a subject that one day may be restored as "required" in economics, makes clear that negative perceptions or reality of "who gets what" have always toppled the best of economic/political regimes. Education is about informing perceptions with facts. I believe that all "models" are abstractions, and hence are "mis-specified" to various degrees. I have therefore worked on understanding the consequences of mis-specification and ways to analyze models that admit this truism.

These days I spend most of my energies working on statistical testing for dominated welfare states/policy programs, and discovery and measurement of nonlinear relations. The first topic involves developing and applying techniques for tests of "stochastic dominance" of various orders. There are quite challenging technical and empirical questions in this topic to make it professionally useful and rewarding. But the prospect it offers for far deeper and decisive policy evaluation exercises is the real promise.

Regarding the second topic of nonlinear relations, I am one of those who consider linear modeling and linear relations, with their correlation-based measures, rather embarrassingly juvenile. It is difficult to ignore the strong evidence of nonlinear relations, and the inability of the correlation measures and tests to detect patently strong nonlinear relations. Approximation is another name for science, but it has to be investigated and justified. Linear approximation is no exception. Professional inertia is one of those forces that are not constructive. We rather get very inventive with linearizing transformations, even if that forces us to focus on quantities and concepts that lack in common sense and perceptual relevance. Why?

It is difficult to predict the specific directions in which our field will move. Alfred Cowles would not likely be pleased with our achievements in seven decades. He abandoned what he regarded as embarrassing and baseless "analysis" in the securities markets. He turned to economists whom he heard had discovered "correlation analysis" which would be capable of uncovering fundamental laws in economics/finance as has come to dominate in the physical sciences. Today, once stripped of their respective jargons,

economists and "technical analysts" in the finance houses are uncomfortably similar in their unreasonable and unsuccessful assumptions and methods. They both assume what has been will be, failing to account for the adaptive and the interactive/reactive qualities of the agents and the phenomena with which we deal in the social sciences.

But I am very excited by the new vistas that are opened up by computational methods. More than any other time, we have a chance to see a lesser addiction to "closed form" formulae as part of our solutions, or as an absolute necessity of successful work. Another area is nonlinear relations. We will surely discover whole new layers to such concepts as dependence, exogeneity, causality and others. We will continue to be humbled by an inability to forecast and to discover "laws" as has come to be expected in physical sciences. But we will have a better understanding of why that is so.

My somewhat radical prediction is that we will witness a backlash against "time series" econometrics/finance. Pure time series techniques will come to be seen as "curve fitting" exercises which cannot satisfy our need for multivariable and "causal" relations that accommodate behavioral, institutional, and market elements. More adaptive and nonlinear distributed lag type models will be rehabilitated and developed! Pure time series models will be seen as technical, black hole, and rather barren reduced form translations of more informative relations of economic interest.

I also see a more sophisticated approach to program evaluation that avoids single indices, such as average scores, inequality indices, and other summary measures. Whole distributions will be evaluated and compared, as in stochastic dominance relations, and mobility over extended periods of time will be even more emphasized in place of "snap shot" comparisons of welfare states and policies. I also foresee us stepping out to multi-factor, multi-score, evaluations of economic states. For example, income alone is inadequate and misleading in inter-regional/inter-cultural comparisons. This realization should help develop more sophisticated concepts of market performance and economic outcomes.

I find reading in areas other than economics quite enjoyable and necessary. Reading in information theory, no longer strange to econometricians, has helped me on many occasions, including in dealing with both of the above topics. I owe this to my friendship in the late 70s with Hans Theil. I suffer the phenomenon of "publication explosion" as do all of us. Being reflective and, trying to see the "forest for the trees", is helpful in becoming a "shrewd reader". I don't have to follow every proof line in a mathematical text to see what is done and what is not done. Almost all contributions are minutely incremental, and it is helpful to read with that expectation when you are in command of your field. I read a great many "working papers". While it would be helpful to know that a paper is filtered by an editorial process, it is costly to wait for the publication delays, or to exaggerate what that "filter" can do.

Reading widely also makes it clear that, in economics and econometrics (perhaps all of social sciences), there is a reluctance to come forth with acknowledgement of original sources from statistics and mathematics. The citation practice is quite inadequate. But this must not be exaggerated since most good ideas naturally arise in many domains at about the same timeframe. But there is no question that we do love our names as well as naming techniques and procedures after our contemporaries. This is often a means of establishing credibility and precedence. Referees are quite reluctant to acknowledge a very different idea/method coming from just "you"! For instance, the idea of analyzing posterior bounds by varying prior variances, used in my 1974-5 papers from my Ph.D. thesis, could not be "sold" until, in the middle 80s, I began to credential them as "a la Leamer". In seminars after 1978, I found it a lot smoother to refer to my specification test of a system, based on system reduced form estimators and first analyzed by Malinvaud (see Maasoumi (1973,1978)), as Hausman's test; see Hausman (1978).

Why did I get into economics? It is a little easier to choose economics nowadays since we have learned to monetize everything, including some serious doctrinal divides! Some say economics is everything because so many things can have a potential economic valuation and motivation, or a demand and supply translation. But when I chose economics over physics and chemistry (at the tender age of 18), I was merely going for a scholarship to study in Britain. I had been so determined to be a petroleum engineer and had first rate opportunities to become one. A close decision at best! But every year since I count my blessings for doing economics. I would have been lost and seriously misplaced in any other subject. Every subject of interest to me, history, sociology, politics, mathematics/statistics, finds a very satisfyingly disciplined way of being pondered with a training in economics.

As for being more of an "econometrician" than an "economist", if that were true, it owes a little bit to my great professor at the LSE, Michio Morishima. He wanted to interview me in depth for a possible admission to economics. I thought I was God's gift already: was it not true that I was already a very young "lecturer", a faculty member at the LSE, for goodness sake? I was a reluctant interviewee, so Michio suggested I consider doing econometrics, maybe with a fellow called Denis Sargan! I was already one foot across the Atlantic when in the late summer of 1973 I heard Denis Sargan had agreed to supervise me. I don't recall applying, but he had read my Master's thesis paper on Nagar expansions and assumed receiving it was my way of persuading him to take me on board! Better lucky than good. More seriously though, how could I help but be in awe of teachers like Denis Sargan, James Durbin, Alan Stuart, R.G.D. Allen, David Hendry, and Ken Wallis, among others. Visiting faculty included people with names like Ted Anderson, Takeshi Amemiya, and Rob Engle. Peter Phillips was writing his thesis on continuous time models and David Hendry and Grayham Mizon had just finished theirs with Denis. I thought I could always go back and do something with what was being taught by Amartya Sen, Michio Morishima, Ken Binmore, Frank Hahn, Terrence Gorman, Harry Johnson, and others who were all at the LSE in those years (Partha Dasgupta and Steve Nickel were TAs then). I have been fortunate to do so.

A young researcher must think very hard and long about the subset of topics she/he wishes to specialize on. It is important to have Ph.D. theses that are not dead ends. That should sustain one for a few years (a very large number of Nobel prizewinners began their seminal work in their theses). Often you may not last long enough to do other things, if you don't get recognized/associated with some topic or area of interest. Equally tricky is the tension between the fashionable areas and those that move you to deep thought and personal satisfaction. Swimming against the tide, or pursuing your own unusual topic/agenda is not for the faint of heart. You must be "good", be seen to be good, and be very good at what others do! There is no doubt that many find it "prudent" to stay within the herd. Some hope to venture out of the mainstream later. Evidently, that dream remains unfulfilled for almost all. When was the last time you witnessed anything more than spirited catfights among theoretical "doctrines" that differed on the basic premises and assumptions? In economics, belief in "trickle down" is a matter of degree. In econometrics, the belief in the ability of "asymptotic theory" to handle any process or model is always one modified CLT away.

The challenge is always the same: To know the difference between being brave and suicidal? My reflections on this have always led me back to what John Denis Sargan said on the occasion of his retirement. Do that which you like and enjoy doing. A modest addendum to this is, do not underestimate fate and the chance encounters that shape the careers of many scientists. Starting with the question of who might supervise your research, what aspects of your own interests may be better received, or developed by demand, what other colleagues or students may engage you in, what different jobs may demand, all the way to what others do, play a role in the choice of research.

Seminal works

It is very difficult to single out great and influential papers. It is inevitable to omit. We are very lucky with a small percentage of great scholars that is still quite large in number. If you were to insist that I name one contribution in economics that I find the most profound, durable, and consequential, I would say that the "impossibility theorems" of welfare economics, as in Arrow and Sen's work, stand alone.

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