My Intellectual Trajectory

The time allotted for these talks is pretty short, so I won't talk about my father Levi, who lost his small business in 1929, a few months after he married my mother, Lena Elkman. My twin brother Frederick and I were born in July, 1930. My father never recovered from the Great Depression. We were helped out by my mother's siblings and lived in the house given to my mother's father by one of my uncles.

So let us fast forward to puberty, a time when one fine morning, I woke with the realization that I was a mathematician. I don't mean this in a formal sense; I simply grasped what mathematics was all about. I knew that a mathematical result might have several quite distinct arguments, which could be combined in a variety of ways. I knew that a Theorem was different from a Lemma. I read the biographies of great mathematicians, and I still have my annotated copy of "Men of Mathematics" by E.T. Bell. I taught myself the calculus of several variables and the Theory of Complex Functions. I memorized the first 35 digits of Pi.

My instructors at the South Philadelphia High School for Boys - a pretty roughhouse school - knew nothing about this passion of mine. In my eleventh grade I learned about a Mathematics Tournament offered by Temple University for all high school students in Pennsylvania. To the shocked surprise of my teachers, and my relatives, I placed first in the tournament.

Temple University offered me a scholarship, which I accepted. As a student, I had unusual habits. I started to take graduate courses immediately. I rarely attended class.; I would learn the material by myself and drop in to take the exams. The professors must have been somewhat taken aback by this behavior. But I had a wonderful piece of good luck. The single female mathematics professor at Temple, Dr. Marie Wurster, then perhaps 30, became my friend. I was invited to knock on her door at any time that was convenient for her. We talked about mathematical topics that I was studying or planning to study; she talked about her fields of expertise. And she told me about the William Putnam Mathematics Tournament, open to every undergraduate student in North America. I took the tournament in my junior year at Temple and placed in the first 10.

I graduated from Temple in 1951, a year containing two extremely significant events: I met my wife and good friend, Maggie Scarf, to whom I have been married for 61 years. The second event was being admitted to the Graduate Department of Mathematics at Princeton in 1951.

At that time, Princeton had the very best Department of Mathematics in the entire world. My fellow - students included Ralph Gomory, Lloyd Shapley, Martin Shubik and John Nash. Nash, a recent graduate, would frequently return from M.I.T.to Princeton. My advisor (and friend) was Salomon Bochner. My first scientific paper resulted from a remark that Bochner made in a class that I attended, and which he submitted to the Proceedings of the National Academy of Sciences.

The Graduate College, where I lived, was physically close to the Institute for Advanced Studies and I would frequently walk on the Institute grounds. It was not unusual to see Einstein strolling with Kurt Gödel, the great logician. Einstein would smile benignly but Gödel never did.

During my time at Princeton, I was disappointed by the ultra-pure mathematics that was the bread and butter of the department at that time. My hope was that the mathematical problems I worked on would have an ultimate practical application: that life would be better for someone, or some group of people, because of the intellectual issues I was struggling with.

And so it was that I left academic life and went to the RAND Corporation in Santa Monica, California, with Maggie and our newly born daughter Martha. I was in the Mathematics Department, along with George Dantzig, the inventor of the simplex method for solving linear programming problems, and Lloyd Shapley, who made the same transition as I had.

At some point, the organization suffered a budgetary crisis and I was transferred to a unit of the Economics Department involved in Operations Research and Management Sciences. I started working on Inventory problems: the purchase and storage of commodities whose future demands were not known with perfect certainty. I met Kenneth Arrow, who was himself working on the management of inventories. My life was changed: He invited me to spend a year with him at Stanford jointly working on Inventory Theory. It was a perfect time for me. The major themes of economic theory were being formulated in mathematical terms, and I fortunately had precisely the right set of skills to make serious contributions. A lovely set of apples was hanging from the tree and I plucked them and ate them one after another with great pleasure.

I continued to work on Inventory Theory, and wrote a paper with Andrew Clark, then at RAND, which started the entire field of Supply Chain management. My most elegant result was a demonstration that the optimal policies associated with the management of inventories involved a sequence of very simple ordering policies. The simplicity of these policies in a general setting was totally unexpected. The proof did not involve very deep mathematics, but it was very odd: strange and extremely unusual. Some of my friends have said that this was the best work I had ever done.

I started to move into economic theory. Economics has a notion of a competitive equilibrium, a set of prices for ALL of the goods and services in the economy such that DEMAND = SUPPLY for all goods simultaneously. How do you find these prices if the economy is not currently at equilibrium? One idea is that prices adjust in a simple way: if the demand for a good is larger than its supply then the price increases. If the demand is less than the supply, then the price decreases. In class, I would always refer to this as the NY Times adjustment mechanism. This adjustment process can be formulated mathematically as a series of differential equations.

My first paper in economics was to provide a very simple example in which this process did NOT converge. The prices oscillated forever in a closed loop without approaching the equilibrium. BRING DRAWING?

And so, I turned my attention to the purely mathematical question of constructing an algorithm that would always find an equilibrium. The proofs of existence of a competitive equilibrium typically used Brouwer's Fixed Point Theorem, which can be paraphrased by the statement "A nice transformation of the unit circle, and its insides, into itself, will always leave at least one point fixed". AGAIN A DRAWING

And so, I decided to find an effective algorithm to calculate that point. And I did.

This result opened an entirely new field of economics: APPLIED GENERAL EQUILIBRIUM ANALYSIS, with the result that large GE models could be solved on the computer, more or less unrestricted by size.

I, myself, was not knowledgeable about the details of the American economy, and my original examples were small, elementary models. But I was very fortunate to have some marvelous graduate students: Timothy Kehoe, Jaime Serra, John Shoven, John Whalley and others, who actually knew about these details. They took over from me and published a number of papers containing realistic models. I quote Shoven:

"Scarf's algorithm permitted the general equilibrium model to enter the mainstream toolkit for applied economists. It removed the restrictions of being analytically tractable. Before Scarf's breakthrough, the only general equilibrium analysis of tax and trade policy was a two-sector model that could be solved analytically. Today, the models are much more disaggregated, much more sophisticated and capable of providing real guidance to economic policy makers."

Next I started to look at the general equilibrium model from a game theoretic point. Game Theory comes in two colors: cooperative game theory and non-cooperative game theory. In cooperative game theory groups of people (players) are allowed to engage in mutually beneficial activities. Alice can say to Bob, "We each own a house. I like your house more than I like mine. How do you feel?" Bob responds, "Well, I actually like yours better than mine. Let's exchange houses." And they do.

Non-cooperative game theory works with strategies. A selection of strategies, one for each player, is a Nash equilibrium if no player has an incentive to change his or her strategy, assuming that the remaining players do not change theirs. This idea has captured all of Micro Economic theory. I am not delighted by it, personally.

Teaching has, of course been an important part of my life. I prepare talks with great anxiety, even if I know their contents perfectly. I make elaborate notes are instantly discarded when I enter the class, and I begin singing from the musical score of an Opera that I have in my head. I smile at all of the students and ask them rhetorical questions, which I frequently answer myself.

I really like my colleagues, though I sometimes have no idea at all about the nature of their research. In my heyday, I was on lots of university committees. I was Director of the Division of Social Sciences. Charles Taylor, the provost of those years, once asked me to join the Provost's office. He not yet realized an important aspect of my personality.

I CANNOT MAKE THINGS ACTUALLY HAPPEN IN THE REAL WORLD!

I adore my family: I have three daughters, with three respective spouses, seven grandchildren, and one great grandchild. I am told by many, many people that my lovely wife is a wonderful writer. I believe them completely, and I have always enjoyed being her first editor.

Maggie and I have seen a great many operas and we love ballet. We have, for many years, had the same wonderful seats at the Met for the American Ballet. We also go to the movies in Manhattan and we have some favorite greasy spoons near our apartment on West 66th Street near the Met.

Back to Economics. It may not be clear to the general public that economic theory has no way of dealing with economies of scale in production – I mean NO WAY AT ALL. Economic theory makes the assumption of constant returns to scale. Taken literally this means that if I want an automobile, I would purchase steel, glass, rubber, electrical wiring, tools and hire skilled labor and produce the car in my own back yard. The assembly line is irrelevant to classical economic theory. I think about this issue a lot. It deals with INDIVISABILITIES IN PRODUCTION: large items like an assembly line, a bridge, the railroad track from New York to Boston, the underwater channel from Britain to France.

And so I busied myself as I was meant to do. I wrote complex and elegant papers, some of which started new fields. One of these fields led to the beautiful figure being passed around the room. The figure was made by an eminent sculptress, Ann Lehman, who is in the audience. Ann tells me that we will be able to construct the figure and many, many of its variations using a 3 Dimensional Printer. How wonderful!