PHILOSOPHY AND MY WORK LIFE

by Julian L. Simon

It is a privilege and a pleasure to write an essay in Michael Szenberg's fascinating series on philosophies of life of economists.

I

As a 17-year old starting college, I was anxious to study what the great philosophers taught about how to conduct our lives well. My second (and last) course was a selective gallop through Western thought. I enjoyed it and did much of the extensive reading. But though some of the philosophy of science made sense, much of the rest seemed meaningless to me. Most especially, the pompously unreadable school of Hegelian German philosophy seemed a fraud on the public; pure obscurantist nonsense, I concluded. When much later I read John Locke's and David Hume's similar judgments about the bulk of academic philosophers, they fortified my doubts.

The exam for the philosophy course was the last of five, so I let it slide. Then the night before the exam I could not find my class notes, which were unusually important in that course. Naturally, I panicked.

A philosophy major upstairs lent me his notebook for a few hours, but it didn't help much. I then had the inspiration to ask him to teach me some impressive-sounding German words that I could insert into my exam essays. He did so. And the next day, for the first and last time in my life, I faked the answers on an exam.

The result? During my first three semesters I had not received even an A-minus. This time I got a straight A, and my phil major friend upstairs—who had been doing very well in all his classes and understood the material thoroughly—got a C. Lest one put the incident down to the inadequacies of a third-rate institution, this happened at Harvard College. If I had drawn the obvious lesson from that experience, and applied it by fancying up my work instead of trying to keep it simple, perhaps my entire work life would have had a different course.

The Navy ROTC (which sent me to college) had a system whereby at the end of each semester we returned our used text books; they constituted a pool from which students would draw books the next semester. The book room was administered loosely, and we were not discouraged from taking books that might only be "relevant" to a course. Nor was there strict monitoring about book return. So it was not unusual for one of us to take a book on "extended loan".

Triggered by a stray remark in the philosophy course, the title *Positivism* by Richard von Mises piqued my interest, so I liberated the book and neglected to return it. When I got around to reading it after college, it greatly influenced my thinking, perhaps because von Mises was not doctrinaire to the point of considering poetry and religion simply metaphysical nonsense, as did some Positivists.

Questions about how a person may best live one's years—such as what to choose as goals in life for one's own sake as well as for others', and how much of one's efforts should be allocated between self and others—these questions are still as puzzling and almost as little-understood as when people first thought and wrote about them "philosophically". And the questions about how research work may best be done still belong mostly to the philosophy of science because they have yet been little studied by the sciences. Philosophy has entered my work life in both those ways: in deciding what I shall try to accomplish, and in the methods I employ to achieve those goals.

The best idea I got from the philosophy of scientific methods is the concept of the operational definition. It focuses on what can be observed and measured, and it battles against vague concepts that cannot be pinned down. I luckily learned about the operational definition in my undergraduate major of experimental

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psychology, a field in which the concept has greatly helped clarify such concepts as morale and intelligence. More about that later.

II

Underlying my choice of work goals is the belief—based on observation, data, and "philosophic" speculation—that there can be progress in human affairs; if I did not believe in progress, I would be condemned to think of my work and the rest of science as the game that many others consider it to be. That is, I believe that the states of material and non-material human well-being are not static in the long run. People nowadays have more of the most important material elements of welfare than in centuries past, notably life itself. That life is the highest value, and that all have an obligation to cherish life and good health in others and in oneself is an idea that I attribute to Judaism even if I did not derive it therefrom.

There also has been improvement in all the secondary material aspects of life: food; shelter and privacy; mobility; communication; and most important, education. I have recently spent much time pulling together the data that document this proposition, much of it published in an edited collection called *The State of Humanity*, and in the greatly-expanded 1996 edition of *The Ultimate Resource*. One of my dreams is to compile a much larger compendium of the most important time series on the human enterprise, going back as far into the centuries and millennia as possible. But this project requires institutional backing and the cooperation of many individuals; whether I will achieve this dream is an open question.

Most of the material progress has suddenly occurred in just the past few centuries, following tens of thousands of years of near-stasis in "consumer" welfare even though the base of knowledge that led to these momentous changes was building gradually for thousands of years. I conclude that the speed of this process up to the take-off point (with respect to consumer welfare, and not with respect to the capacity to produce) was the result of growths in population and in knowledge, and that population size was the ultimate determinant of knowledge growth; this work is embodied in a yet-unpublished book entitled, *What Governed The Speed of Human Progress?*

III

There is a great difference between me and most other economists that Professor Szenberg has invited to write essays here: They have been honored by their colleagues with Nobel Prizes, election to the presidency of the American Economic Association, and similar marks of distinction. In contrast, I have never had a single mark of professional respect (let alone honor) in my academic professions. I've never held an office (not even membership on the committee that nominates *other* people for offices and honors, or the committee that counts the ballots for the candidates), never been asked to give a to-be-published paper at an annual AEA meeting, and—any scholar must be amazed that any one could publish a huge pile of books and papers over many years and yet compile this particular record—never been asked to referee a paper for the major "official" journals in my three fields of economics, demography, or statistics, or by almost any other top journal. These implicit judgments by the professions accurately reflect my lack of success in achieving what I most sought to achieve—that my research would induce further work by my colleagues.

Why, then, did Professor Szenberg ask me to join this group? Most probably because I have had a peculiar kind of success outside the economic profession. This success is indicated most clearly by the invective directed at me by non-economists; curiously, they often denounce me as being quintessentially an economist. (Here it is interesting to note that I never studied economics, a matter we will come back to later.)

IV

Roy Pascal wrote that the key question for the autobiographer is how s/he became what s/he became. The answer inevitably needs much more than an article, of course. But I can at least try to answer a more limited question here: How did I become the kind of *economist* I am?

My father and mother were both born in Newark, New Jersey, in 1895 and 1900 respectively. Their parents were Jewish immigrants who arrived impoverished in the previous decade, but who had by then saved money and opened small stores. In both cases, the women were the driving forces. My mother's mother had in 1900 opened a hardware store measuring about 100 square feet in an Italian neighborhood in Newark, New Jersey. From that store eventually sprang a hardware "empire" in Newark, in the 1920s totaling 21 stores and a wholesale operation, and in the 1950s including a chain of stores. Each new operation was started by a new-immigrant relative who came to live with my grandparents for a while, learned the business, and then opened up on his or her own in an area of the city that as yet had no Goodstein-family store.

My father's mother opened a "dairy store" that sold no meat and therefore avoided the many problems of food being certified kosher.

My parents grew up working in their parents' stores. Dad quit high school around the end of his junior year, ostensibly because he was needed in Grandma Simon's store. My mother was valedictorian of her class in high school, her greatest pride in life. Her father having died, she then went to work to help support the family, and she took some night courses in accounting at NYU.

I was born on February 12, 1932—Abraham Lincoln's birthday, which accounts for my middle name being "Lincoln". My first clear memory is of being downstairs in Grandma Goodie's old-fashioned hardware store and being given a small box of nails of assorted sizes to sort—probably nails that had spilled in the process of weighing them by the pound for customers. That was a wonderful task to give to a child to occupy him and make him feel that he was "working" and doing something useful.

I remember with pleasant warmth the first nine years of my life. Our block in the gritty, working-class Weequahic neighborhood of Newark was composed of two-story two-family houses; we lived downstairs at 124 Grumman Avenue. All the people on the street were Jews except for the Greek family that had a nice one-family home at the corner with garden; they owned a restaurant. Son Jimmy-the-Greek was my age. It was a neighborhood of nicknames. The kid across the street was Bupkiss (Yiddish for "beans") Barnhart.

I swam like a fish in water in Newark. I felt a part of the group, never like an outsider. I may even have been pampered by the older kids and grown-ups; this certainly is the impression given by a photograph of my gang, taken when I was about 6. I remember a tribal-like arrangement in which the gang made allowances for the kids' various ages and physical capacities (I was the youngest), but I may be romanticizing. The 17-year old in the picture was like an older brother to me; it was the last time in my life I had the experience of a protector.

There was always something to do on the street, ranging from A (apple-crate scooters mounted on worn-out steel skate-wheels) to Z ("zip-guns" made from orange-crate corners and rubber bands that shot tiny squares cut from shirt cardboards). When I was 6, 7, 8, and maybe 9 years old, we scaled the baseball and "Horrors of War" series of picture cards that came with bubble-gum against the house wall in the alley (that is, the driveway between two houses), with the closest toss being the winner of all the cards played on each round. This game illuminates a central point in my nature.

There were two styles of throwing: a) holding the corner of the card between the second and third fingers, or between thumb and forefinger, and flipping the card from the back of the hand; and b) propelling it by bending it between the thumb and the index finger and using the spring of the bend, plus the thumb, to snap it forward. The latter was the "big kids' " way—sophisticated and macho. Little kids did it either way. I well remember experimenting to see which worked best for me, and reflecting that the backhand method was more efficient, in considerable part because it was simpler—no moving parts and no need to have a properly springy new card to flap. So I opted for the backhand method because of its efficiency, despite its lower "little-kids" status.

That decision foreshadowed many decisions in my life—simplicity over sophistication, efficiency over style. I have always wanted to do well what I was doing, even when the choice is between doing the task skillfully and impressing other people. The interesting part about the picture-cards story is how clearly I remember making the decision on those grounds even at that young age.

I like to win, but winning is usually less important than doing it well. To win a squash game when the opponent errs on the last point, rather than my scoring a nice winning shot, leaves me unsatisfied. And playing in tournaments has never mattered a lot to me. I don't get a special rush out of competing. And I almost never had a sense of competition against other students in my school work.

My introduction to duopolistic price competition came when, about age 10, I overheard my father on the phone fixing prices with his competitor. Dad owned a tiny business selling sal soda—crystallized sodium carbonate, also known as "washing soda"—mainly for use as an industrial water softener.

National Crystal Company—it sounds rather grand—was located next to a railroad siding in Irvington. Dad's "factory" was four sideways concrete steps up from the railroad track, through a heavy tarpaper door that slid sideways, then onto rough wooden tracks on which barrels were rolled up and down. The main equipment was a crude mixing vat that combined water and the central input, potash; the technology was vintage 1830s (sic) except that it was driven by an electric motor rather than an overhead belt powered by steam or water power. There was also an "office"—a walled-off desk and telephone, my father's desk chair with a ripped horsehair seat, and a pile of burlap bags for another seat in case anyone else came in. The establishment had no heat in the winter other than a kerosene stove in the office.

Besides potash the main input was my father's labor—shoveling the potash from the railroad cars (with which half-time laborer Willie helped), mixing the potash with water (and perhaps other materials), packing the crystals into wooden barrels, and finally loading the barrels onto a customer's truck. The business employed only my father and Willie, but it provided the family perhaps \$50–\$100 a week (say, \$500–\$1,000 in 1996 prices) during the Depression—very respectable.

There was one other sal soda firm in the New Jersey area, at the other end of Newark. From my parents' comments I'd guess it was about the same size as my father's business. And I remember my father's side of the worried phone conversation—we may have already moved to Millburn, in which case it would have been after June, 1941—in which he and the competitor agreed to hold prices at something above a cut-throat level. Afterwards there were discussions between my father and my mother about whether or not the competitor would keep the agreement. (This is the stuff or industrial economics that gets lost in the equations we write.) Here George Stigler's theory of polypoly, with its emphasis on firms making price-fixing agreements and then cheating on them, resonated with me. (None of this is inconsistent with my belief that, taken as a whole, the world of commerce induces people to act more honestly and cooperatively than does any other system of economics.)

We moved to Millburn, a New Jersey suburb a few miles north of Newark, in the summer of 1941. The move produced a crack in my life, though not an earthquake. I never again felt like an insider. The picture of the gang in Newark with me as the littlest kid in the center was my last such experience.

Though I had no brothers or sisters, I do not remember wishing for siblings until we lived in Millburn. But then the phrases "my sister," and especially "my brother," became for me the sweetest words in the English language.

About 1942 or 1943 National Crystal Company shut down for war-induced lack of raw material. For the rest of his life my father worked only for two periods of about 18 months each, until I could hire him part-time in 1961 when he was 66. My mother would not allow him to take the jobs he could easily get during the war—such as driving a bus, and working as semi-skilled laborer in war industries—because of status considerations; his not working on the sabbath was mostly an excuse because during the war, there were jobs galore. My mother then went to work as an office manager, and the wolf lurked behind the door thereafter. Ensuring the future economic well-being of my parents became a constant element in my thinking for the next 25 years.

I was a pretty good student up through high school, and a fairly good athlete. I tried not to stick out in any way, and succeeded in doing so. I did not think of myself as different or special in any way. Socially, I was always a bit marginal, perhaps in part because I was Jewish in a mostly-non-Jewish town, and perhaps because of some aspects of personality such as my feeling that there was nothing especially desirable about me; after all, I was just another guy trying to find a place in the world. Also, there always lurked inside me some irreverence for authority and for orthodoxy, and that probably contributed to my being at the edges. I never had any interest in socialism, and I remember joking that I was against communism because I would be one of the first people they put up against the wall and shot—because I would not be properly reverent.

I remember nothing of what happened in classes except a few bizarre events such as the students—including me—cruelly torturing teachers. I remember lots of sports, hanging around, and telling jokes outside the classrooms. I also have no memories of classes in elementary school and very few in college. And I had no personal relationships with teachers.

I took a test for the Navy ROTC scholarship program, and wound up at Harvard; the scholarship, plus jobs after I got back from cruises in the summer, enabled me to get along without any money from my parents. College years were fruitful and enjoyable. Majoring in experimental psychology was a good choice for me, and my senior honors thesis on concept formation was the high point of the four years; I felt that I was obtaining new knowledge unknown to anyone in the world, an exhilarating feeling. It turns out that I really was making a considerable discovery in cognitive psychology, but my advisor had left Harvard for a sabbatical and no one in my behaviorist department was interested. Later when I was aboard ship I sent an article on the subject to a psychology journal, but my lack of proper formalism—references, etc.,—was abysmal, and that is all the referee saw; as I re-read that referee's report now, I see the herald of much of the rest of my life.

For almost two years I served aboard a destroyer as a deck officer. I loved my job as "First Lieutenant"—the title for the officer in charge of the decks and sides and all seamanship. But I got fired because we failed an inspection. (We were short of personnel, and I did not know how to "fight" for more.)

Serving as the ship's defense counsel at captain's mast and lesser courts-martial for sailors accused of minor offenses perhaps inevitably fell to my lot. I did not realize that I was supposed to act my part in a court-martial drama whose script was informally written in the wardroom over coffee. Giving one man a spirited defense that got him off on appeal to higher authority got me in trouble because it was a black mark for the ship. My stupidity about organizational realities like this has persisted throughout my life.

After the Navy I came within two weeks of going to medical school. But I had a vision of my brain being fossilized as I passed through the gate of the school. I also worried about the give-'em-a-pill mentality of the medical profession at that time, as if the body could safely be dealt with a part at a time. So I took a leave of absence, and never looked back. (Tufts Medical School will be surprised if I write them that I plan to matriculate next fall.)

So I went into business: Cost accountant at Prudential Insurance Company for four days, until the extraordinary regimentation did me in. Technical writer of instruction manuals for a week, until I was terminally bored. Encyclopedia Britannica salesman for a month, until the lies became impossible and I found another job. Then advertising copywriter in a pharmaceutical drug agency for six months, and advertising promotion writer for a complex of magazines for six months. Halfway through that year I decided to learn the theory of business, so I went to the University of Chicago for an MBA. That took a year, and—hungry for bodies at the time—associate dean Jim Lorie recruited me for the PhD program, telling me that it would only take another 3 months of class work, plus a thesis I could do in 6 months. I stayed because I had a girlfriend, and I was enjoying the university and university life, though I had no intention of becoming a professor; I thought I had no special talent for it.

My career at the School of Business was distinguished by having been the only PhD candidate in living memory, before or after, to have failed his/her PhD orals. I had studied so little economics that when asked a question about consumer surplus I put price on the horizontal axis and quantity on the vertical axis, and never could get untracked after that; I had a fugue, the only such happening in my life; it was as if I was gliding through the air in a Chagall painting. The committee mercifully reexamined me a couple of months later, and I passed.

After a total of three years and a thesis on the storage operations of a university library (I took a job directing the project to make money, with no thought to the academic importance of the work), my new wife (sociologist) Rita James Simon and I moved east to start a mail-order business in Hoboken, New Jersey, where I could employ my parents and provide them a source of income.

After a year and a half in business, I realized that the writing of a correspondence course I then sold—naturally entitled *How to Start and Operate a Mail-Order Business*—was what I most enjoyed doing. And because of something wrong I did, I fell into a deep depression that lasted for years until I found a new theory of depression that cured me in a week (see my *Good Mood* book). On a fluke I also gave a lecture at Columbia that I enjoyed. Hence I decided to become a professor.

The better of the two opportunities in a thin market for my services was teaching advertising in the College of Journalism at the University of Illinois. There I started doing the economics of advertising; I think I was born an economist. After a year and a half I was offered the opportunity to be put up for tenure, but readily acceded to the department head's suggestion that I wait a year. A year later he wrote me a letter saying that I could expect never to receive tenure in that department, despite two books in publication, a flock of articles, etc.; I did not fit into the head's vision of what the department should be. This freed me to go across the street and solicit a job in the Department of Marketing in the College of Commerce. Three years later, in 1969, I was also given a joint appointment in economics. But I always wanted to remain half-time teaching business because I liked teaching practical subjects even though they had almost nothing to do with my research.

V

This essay is addressed to economists and therefore focuses on my life as an economist rather than my private life. But my family life has more influence on me emotionally. I am enormously lucky to have a wonderful wife who has had an extraordinarily productive and valuable professional life as a sociologist, plus three grown children who are healthy and who have their feet firmly planted on roads that lead to useful, productive lives; indeed, they already are doing so. I can shake off a letter or a newspaper article that calls me "the Devil Incarnate", or even the rejection of a technical article, in a relatively short time. But a family unpleasantness can throw me into the pit of despair for days, and the feeling that something good has happened to one of my children can buoy me up for days or weeks; this is a measure of the relative importance of family life for me.

VI

Though I only had three MBA courses in economics, years ago I stopped feeling like a pretender and a fraud when I referred to myself as an economist. And I have thought from time to time that my lack of socialization in the field may have helped me be open to some ideas that graduate study might have closed off, or to use tools that we did not easily revert to because we had never learned them.

VII

The subject here is my philosophy and not my work. Yet my life philosophy is not understandable without a bit of information about the professional work I have done. I'll mention the main topics briefly here, and many of the topics will re-appear later as I discuss some of their aspects in particular contexts.

After three years of work on the economic and managerial aspects of advertising, I ended that study because, as I wrote at the end of a book of my technical articles and essays on the subject (badly titled as *Issues in the Economics of Advertising*; it sounds like an edited collection): "[T]he economic study of advertising is not deserving of great attention except for special problems . . . (As the reader may realize, this is not a congenial point at which to arrive after spending several years working on the subject.)" (1970, p. 285)

The main subject of my professional life has been the economics of population, and the effects of size, density and growth on the standard of living, availability of resources (including such human resources as the amount of education), cleanliness of the environment, and other related phenomena that may be related to the demographic variables. Both my empirical and theoretical studies have mostly confirmed the prior discoveries by Colin Clark, Simon Kuznets, Harold Barnett, and Ester Boserup (and the speculations by Alexander Everett, Friedrich Engels, and Henry George) showing that first-edition Malthusianism does not fit various sets of relevant long-run data (as Malthus himself published in his second and subsequent editions); models that allow for human adaptation to physical conditions through increases in knowledge fit the facts much better in the long run.

The economics of immigration is a sub-topic that has some special twists and professional surprises. For example, immigration is not at all analogous to trade because the "pure" Ricardian gains to trade go to consumers whereas the "pure" gains to immigration (in equilibrium) go to producers (the immigrants themselves, if one abstracts from capital effects). On the other hand, the larger is the government sector in a country, the larger the one-time windfall contribution made by immigrants through their excess of taxes over services used (because they typically arrive when just starting their labor-force years).

Population economics also led to large-scale gathering of data on the important material aspects of human life over as many decades, centuries, and even millennia as possible.

Another main topic has been the development of ways to do statistical inference by simulation rather than Normal-based and non-parametric formulas. This has come to be known as resampling. Among other devices I introduced the bootstrap method in 1969, and developed a computer language and program in 1974 to perform the Monte Carlo simulations efficiently but still with such simplicity that the user understands every logical step. This line of work—which I went back to with renewed vigor in 1989—threatens much of the intellectual capital of mathematical statisticians. My main interest, however, is in presenting workaday tools to statistics users rather than investigating the properties of complex variations of the method.

Simulation of duopoly and triopoly is another topic that I worked on around 1970 and then again in the 1990s, with the same colleague: Carlos Puig, who was an undergraduate when we began collaborating. And I have also allowed myself to get interested in a wide variety of empirical questions ranging from the effect of smoking on life expectancy to the relationship of media coverage to public opinion.

VIII

More and more as I have gotten older, and consistent with my belief that progress is not only possible but inevitable, and probably irreversible by now, the crucial philosophical element in my work life has been the desire to be useful. The sage Rabbi Tarphon enjoined, "One may not neglect the work." I am comforted, however, that he added, "but one is not required to finish it, either." Here I consider "work" to be that which aims to produce something useful to others or to oneself.

The value of doing something useful led me to stop working on the economics of advertising, my first major topic.

I may have succeeded in being of some use to the larger society. My reverse-auction "volunteer" scheme to resolve the problem of involuntary bumping of passengers from oversold airline flights, and the lengthy and strenuous promotion of the scheme, has been an complete and unmitigated success starting the very first day it was inaugurated—after 12 years during which everyone (except economists, I'm proud to say) asserted that it was ridiculous in principle and could not possibly work in practice. My studies of immigration may have had some effect on federal legislation regulating the volume of immigration into the United States—at least Senator Rudy Boschwitz (the only immigrant then in the Senate) told me so, and the anti-immigration organizations have paid me the tribute of calling me a liar and a profiteer. And maybe my work on population and resources has had some salutary effects. But it is very easy to fool yourself that you have been more important than you really have been in such matters where causal influence is so indirect.

I judge, however, that I have failed abysmally in what I have spent 90 percent of my work effort trying to do—being useful to my fellow economists and statisticians and social scientists by providing to them theoretical ideas, frameworks, data, and methods that would help them in doing more advanced work.

When I say I have aimed to do work that is "useful" to *economists*, I do not mean work that other economists would simply *consider* valuable, but rather work that would advance other economic work that would be useful to *society*. In a memorable interchange in their 1960s presidential addresses to the American Economic Association, Paul Samuelson and Kenneth Boulding proposed very different tests of the value of economic work. Samuelson stated his criterion to be the approbation of one's peers: "In the long run the economic scholar works for the only coin worth having—our own applause" (1962, p. 18). Boulding then replied that "I did not become an economist for anybody's applause; I became an economist because I thought there was an intellectual task ahead, of desperate importance for the welfare and even the survival of mankind" (1966, p. 13).

Boulding's criterion appeals to me, and I suspect that it appeals to Samuelson, too. But Samuelson's criterion is much more easily measurable—with citations, for example, or speaking invitations or salary offers—than is Boulding's criterion, which helps Samuelson's criterion to dominate the profession. And many of those who espouse Samuelson's criterion say that the acceptance of the profession is in fact the best available proxy for Boulding's criterion of usefulness.

IX

Turning to the role of philosophy in actually doing my work: One does not need the philosophy of science for run-of-the-mine research circumstances. But one does need it for tough questions that arise in unusual circumstances.

Even before I intended to become a professor, I realized that I was interested in research methods. And when I first began to teach and was scheduled to teach a course in research methods, I was delighted. Because I thought I had a new way of looking at the subject—viewing research methods as devices to overcome obstacles to knowledge—I wrote a textbook, *Basic Research Methods in Social Science*. I read a lot of philosophy of science while doing so. I realized that any text on research methods—especially mine—bootlegs a working philosophy of science. That is, a text is almost an operational definition of the subject.

I have sought to do work in a manner that the methods would be effective in producing useful information rather than "finding" an abstract "truth". And along the way I have concluded that general principles of research laid down by philosophers, most of whom have never done any empirical research, are worth little. Consider, for example, Popper's assertion that a proposition is never confirmed but can only be disconfirmed. The plain fact is that the weight of evidence matters; if there is a lot of evidence for, and little evidence against, a proposition, people will *act as if* it has been confirmed, and the acting as-if is what matters. Another example is the hypothetico-deductive "method" that for quite a while ruled the roost in the philosophy of science, and even within economics. This principle condemns all rooting around in the data for plausible hypotheses, and insists that hypotheses be deduced from theory. That was a helpful corrective against the danger that if you conduct many tests of statistical significance, you will surely arrive at some apparently-significant findings as artifacts. And it was a handy device to help referees condemn papers they do not like. But the hypothetico-deductive "method" is an intellectual straitjacket, and no sensible researcher pays much attention to it. If philosophy runs too much against standard practice of the most competent workers in a science, one should consider that the philosophy must be wrong, rather than the practice.

An early foray into philosophy was into the question of whether one should refer to smoking as a *cause* of lung cancer. That problem roiled the medical and statistical communities after the *Surgeon General's Report* in 1964. And R. A. Fisher came down on the side of "unproven". Having read that *Report*, I thought that the aggregate of the evidence—the proper way to consider the matter, in my view—constituted an open-and-shut case, so I wrote a letter to that effect to *American Statistician*.

Then chance intervened in a convoluted fashion, as it so often does, leading further into the concept of causality. In New York I had done some consulting work for a liquor trade association. A key issue in liquor-tax hearings is the extent to which liquor sales are affected by a change in price. Looking over the available studies, I was struck by their lack of solidity; none of the many methods tried, many of them extremely complex, seemed persuasive. It occurred to me that with a simple set of adjustments, the sales volumes observed before and after state changes in liquor tax laws should yield a reliable answer. Later in Urbana I did the calculations for the liquor study, and after writing them up I sent the study to *Econometrica*. To my surprise and delight, the article was accepted for publication. This was amazing because the article contained not a single equation in algebra (though it did contain an equation in plain English), probably the first time in the history of *Econometrica* that such a "mathematically unsophisticated" article had been published.

The explanation for the happy accident of the article being accepted is that the referee was Herman Wold, far too capable a man to be put off by lack of mathematical dazzle. And he requested the editor to put me in touch with him because he wanted to tell me about a related piece of work he had done in Sweden with postal rates.

Now finally causality: In the course of our correspondence in 1964, Wold enclosed a draft of a paper about causality in statistical investigations. Flattered by the thought that he sought my comments, I read the paper carefully. Try as I would, however, I could not figure out just what the term "causality" meant in the context of that paper.

After I grappled for some time with the concept of causality, I realized that the concept of the operational definition provides a key to the matter. What is needed is not a definition of causality with respect to the *properties* of the concept, but rather an operational definition which in practice is a set of criteria. And that in fact cracks open the conundrum.

This is the criteria set I proposed: A statement shall be called "causal" if (a) the relationship correlates highly enough to be useful and/or interesting; (b) it does not require so many side-condition statements as to gut its generality and importance; (c) enough possible "third factor" variables must have been tried to give some assurance that the relationship is not spurious; and (d) the relationship is deductively connected into a larger body of theory, or (less satisfactorily) is supported by a set of auxiliary propositions that "explain" the "mechanism" by which the relationship works. This checklist constitutes the definition of criteria. Whether a given relationship meets the criteria sufficiently to be called "causal" is not automatic or perfectly objective, but rather requires judgment and substantive knowledge of the entire context.

Another application of the operational definition: Because I had learned as an undergraduate that psychology dealt with such difficult concepts as morale and intelligence by recourse to operational definitions, it occurred to me that the concept of utility and interpersonal comparisons should be treated in similar fashion. It is necessary simply to find one or more measures that are relevant to a particular context of discussion; one can then abandon the debilitating idea of the "properties" of utility. Thought of this way, the "problem" of interpersonal comparisons simply disappears. Questionnaire measurements of happiness, and rates of suicide and mental illness, are obvious candidates as measures. Utility is then simply defined as, say, a score on an questionnaire, just as Einstein defined time as what is read on a clock. So I organized and presented data on these and other matters and showed how they could help make sense of problems in taxes and transfers.

Hand-in-hand with the operational definition goes the analysis of language the Vienna Circle taught. For example, when I examined closely the usages of Chamberlin's term "product differentiation" I found that they usually are tautological, and it is impossible to give consistent meaning to the term and the concept. This—together with George Stigler's demolition of Chamberlin's concept of "the group"—shows that the concept of monopolistic competition is without value as an analytic tool.

Another example: What is the "true" effect of income on fertility? In a short book I wrote in the 1970s showing different relationships between income and fertility in different times and places, I concluded that the "true" relationship even at a single moment in a single country is impossible to state meaningfully. Rather, the relevant statement about the relationship must depend upon one's larger purposes—a theme concerning the need for judgment that runs through all my methodological work.

It is quite amazing how many concepts that seem to be pillars of economic theory crumble like sand upon close examination. For example, the capital-output ratio turns out to be meaningless for most or all long-run and inter-country comparisons because the value of capital is a function of the price of the output it is used to produce, rather than having any meaningful independent valuation. (Original cost is not meaningful in such contexts.) This assertion is perfectly analogous to the value of farmland, which Colin Clark showed is about 3.5 times the value of the average year's gross output all throughout history and in all countries, apart from temporary bubbles or depressions. Deviations from average capital-output ratios therefore tell nothing fundamental about the state of an economy. Naturally, it is not easy to convince capital "theorists" of this point of view. I published this idea in an appendix to a book, but after 20 years I am still looking for a journal to publish it.

Off and on for about 20 years I have spent many hours on sabbaths trying to understand special relativity intuitively. I devoted several happy years to the early chapters of Einstein's own little book intended for pre-university students(!). I understood Einstein's critique of the concept of simultaneity because of my prior understanding of the operational definition, and in turn that critique sharpened my understanding of operational definitions and helped me apply it to such problems as the theory of natural resource supply; with respect to the latter, the operational definition helped me formulate the view—mind-boggling to many—that the supplies of natural resources are not finite because the definition of "finite" implies measurability, and there is no way—even in principle—to measure those future supplies because of the very real possibility of new substitutes being invented and new sources being discovered in the cosmos.

X

Fall or Spring, 1938 or 1939 or 1940

The dispute was about payment for the rent of some beach umbrellas. I was perhaps seven years old, and it was early in the day in Bradley Beach, New Jersey. One well-muscled, suntanned lifeguard had the beer-bellied truck driver's arms pinned behind him and another muscular lifeguard was smacking him with his fists in the midsection. It didn't go on long, I suppose; I don't have an end to the recollection. But the memory has remained vivid.

All my life I have identified with individuals and groups in the truck driver's situation. For myself, too; having someone attack me in print when I can't obtain the opportunity to reply is a bit like the situation of the umbrella man. Somebody is getting hit and is denied the opportunity to fight back.

XI

I noted earlier that my professional history is different from the other persons whom Michael Szenberg has invited to write essays in this series: They have been successful in the eyes of their colleagues, and their work has been fruitful in inducing their colleagues to build their work. And they have mostly taught in university departments of high prestige, and have been honored with offices in their professional associations. None of this is true for me. This is not a statement of modesty about my work, which I believe to be powerful, sound, and important. It is rather a statement about how my work has been received (or to be more accurate, not received). Nor is this a lament, because this result was almost inevitable, given the nature of the work and of my own professional habits. It is just a fact that the reader must recognize in order that the rest of what I write here will make any sense to you.

Naturally, I've speculated a lot over the years about why the corpus (corpse?) of the work I consider my best has not been attended to and built upon by other economists. The simplest and most obvious hypothesis is that the work is no good. But by now there has been enough confirmation of the results of my work, both in practice (the airline oversales auction system), and in the non-appearance of work showing me to be in error (as well as the same work being done later by others and received well), that that hypothesis does not explain the whole. Therefore I have cast around for other explanations.

For a while I thought that it was my mode of presentation that was at fault—too staid, or not well-enough organized, or too lengthy. Another early hypothesis was that I have worked in too many areas, spreading myself too thin and weakening my credibility. But now I can't believe either of these is the main explanation.

Still another hypothesis is that I have not been a member of a group of people working in a shared tradition on a related set of problems in which the profession at large is interested. Maybe. Yet the successful innovators do not confine themselves in such a fashion, and they manage to get their ideas across anyway. Perhaps such successful innovators have greater force of personality than I do, and are supported by more confidence in themselves and their ideas, which shows up as infectious enthusiasm. But this can't be the whole answer, either.

My social relationship to my professions certainly has been dysfunctional. Michael Szenberg wrote that "There is increasing recognition of the significance of face-to-face interaction, interpersonal communication, and cooperation among scientists" (1992, p. 2). But because of a combination of the circumstances that have constituted my opportunity set, my shifting of interests from subject to subject, and my own bashfulness at seeking instrumental relationships, I have almost no professional friends among economists, demographers, and statisticians. How important this element has been I cannot estimate.

Friends have speculated that I may have evoked damaging resentment by having participated in public debate on various issues in newspapers and television. Scientists are not "supposed to" speak in blunt terms and be seen in the media. But this hypothesis is too self-flattering; resentment could hardly explain the fates of my writings in the years before 1980 when I stuck completely to my academic grindstone.

And ideology is not the explanation. Joan Robinson may or may not be correct when she wrote that "Economics has mixed its ideology into the subject so well that the ideologically unconventional usually appear . . . to be scientifically incompetent" (1977, p. 1319). But my economic and social ideology are quite in the mainstream of Anglo-Saxon economics, and hence cannot explain the fate of my work.

My approach to the craft of science, and my philosophical approach to scientific method, however, are out of the mainstream. It is true that, on grounds that *seem* ideological, some economists and almost all demographers disagree vigorously with the conclusions about population growth that I (though not I alone) arrive at. But my work on subjects other than population—including on advertising, long before I was at all notorious—also evoked chilly reactions, with no ideological explanation.

Certainly my reluctance to employ the framework of the optimizing allocation decision—with full mathematical regalia—in those situations where I consider that framework unnecessary or inappropriate, has not helped. And much of my work deals with very long run economic development, a context in which the creation of new technology is the most important element. That process often is best not treated with the logic of profit maximization.

The pro-technique tendency of the profession is a disaster for me professionally because I have a distaste (irrational because counterproductive) for unnecessary flourish. As an outgrowth of how my mother's need for status kept my father from taking available jobs, as I mentioned earlier, I grew up seeing how the desire for proper front can destroy a person's self-respect, and keep a family from getting a living; therefore I hated it. Nowadays I no longer actively hate front and flourishes, but only despise them that attitude shows through.

My methodological philosophy of simplicity certainly is in some part responsible for my failure to win the attention of my economist colleagues, though I still believe in its essential soundness. For a long time I was taken (or better, taken in) by the notion that simplicity is an ideal in science. (I even titled two of my early articles "A Simple Method" . . . .) It took me years to appreciate that T.S. Eliot's remark about poetry applies also to scientific writing, to wit: It must be easy enough to understand, but hard enough so that it cannot be understood immediately. Simplicity fails because, as Stigler puts it, "The form of work takes on a value independent of its content: a scholar should be literate, and his work should be pursued with non-vulgar instruments" (quoted by Fisher, 1986, p. 78). And as Goodwin (quoted by Fisher, 1986, p. 80) put it, "It is the essence of a profession that the skills required therein are not possessed by those without." This has led my work to take some lumps. For example, I wished to compare the areas between two empirical curves showing sales responses to advertising over time, and I simply counted the squares under the curves on graph paper. A referee was horrified at the "primitiveness" of this mode of integration, and at the ignorance of the author.

To a considerable extent, however, the content of my best ideas is inseparable from a mode of presentation that damns them; hence using other than my unacceptable style of work was not within my power, and negative effects were inevitable. Here are some examples:

1. My article on the price elasticity of the demand for liquor that appeared in *Econometrica*, mentioned earlier. The very essence of the work was showing that a very simple method—the "quasi-experimental" study of a large set of controlled before-and-after comparisons—can be more powerful than standard or even "elegant" non-standard methods of the sort that had been tried before, and this method is more flexible and easier to interpret.
2. Applying the Monte Carlo method to the entire range of problems in statistics—including the bootstrap technique—and also to simple and complex problems in probability, may have enormous practical advantages. But it is the very antithesis of the highest value of the mathematically-minded persons who are the gatekeepers: It foregoes all of the esthetic quality of the deductive formulaic proof-based methods of mathematics. Hence it encounters resistance from those who prefer formulae. Even as a method of teaching and doing probability and statistics, the Monte Carlo simulation arouses hostility despite the experimentally-demonstrated success in the classroom.
3. Simulations of duopoly and triopoly competition produce novel propositions about the effects on the results of various changes in conditions, such as the number of competitors and the cost of capital. But this is from a July, 1995, referee's report to the *American Economic Review*: "The bottom line after all of this is that I am of the strong opinion that this paper should not be published—anywhere, anytime". The referee's main criticism is that the model is "of the sort that no one has been interested in for at least 15 years".
4. The book *Applied Managerial Economics* shows that with only a simple spreadsheet—either on paper, as I first showed it, or wonderfully better with a computer spreadsheet—one can get perfectly sound answers to all of a firm's decision problems concerning control variables. The spreadsheet is incomparably more flexible than the standard marginal analysis conducted with the calculus, because the spreadsheet handles multiperiod analyses—which are at the core of all difficult firm decisions—with perfect ease; it does not require simple equational forms, as does the calculus. But to people raised on the calculus who have invested much professional capital in learning how to surmount and enjoy its intellectual difficulties in conquering these problems, the simple tabular system has all the intellectual charm of hammer, handsaw, and manual screwdriver.

A close friend who is a very imaginative and productive student of finance, and an astute observer of the academic world, chides me with "Why don't you do things the way other people do them?" That simply isn't possible in the sorts of cases mentioned just above.

Also: Like engineers and business-persons, I intend my studies to fill needs and do jobs, rather than to be works of art. This inevitably makes it more difficult to attract the interest and attention of colleagues in the knowledge "game".

Along with engineers and business-people, I focus on the independent variables rather than on the dependent variable. That is, economists (and scientists generally) usually want to know the causes of the variation in the dependent variable—for example, the birthrate, and the movements of the stars. In contrast, business-people and engineers want to know how a particular independent variable—say, income or education—changes a dependent variable such as consumption of alcohol or the birthrate.

The subjects I have worked on were not well-chosen for success. I did not choose topics that were "hot"; I had little sense of what hot topics were, and I would not have liked the sense of competition to publish first; also, I have believed that if lots of people are already working in a subject area, I am not needed (unless I think that the field is pointed in the wrong direction). Instead, I chose topics where I saw big lacunae of knowledge that I often came upon because I went looking for published results and did not find them; this was the case in 1974 with estimating the effect of amounts of smoking on life expectancy, and it was often true in population and immigration economics. I also chose theoretical and empirical topics that seemed important because of public policy discussions—again, especially population and immigration economics. I did not have the knack of choosing those topics that other researchers would like to build on because they are a natural forward extension of the main stream; by contrast, George Stigler was a master of doing so. My work in endogenous growth theory with respect to population was an exception in this respect, but I did not impress it upon the consciousness of the profession, and my work therefore was bypassed.

The most self-flattering speculation is that I fail because my ideas run against common beliefs. I believe that, as information theory teaches, information contributes most to knowledge if it is dissimilar to other information presently in the system (and, of course, if the information is correct). As it is said, "If the two of us agree, one of us is unnecessary." Indeed, much of what I write does not simply bill itself as new, but begins by saying that the conventional view is wrong. Still, I can't convince myself that this is a major explanation of my outcomes, much as I would like to so believe. Others also offer unpopular or unconventional ideas, and yet they manage to get people to pay attention to those ideas.

XII

Why did this person Julian Simon arrive wherever I got to, rather than lots of other young kids who were indistinguishable from me in high school or in college or in the Navy?

I started out in life quite close to the main stem of the tree from which I grew, closer than many other young persons. But unlike many others, I kept growing away from the main stem, rather than closer to it. One reason may have been that I was not afraid to get further away from the main stem—farther and farther out into the uncharted forest, a longer and longer distance from the shelter of the community. In adulthood I grew more and more comfortable with being away from the main stem.

Later on I knew I was a bit eccentric, particularly in my irreverence for authority. But I always thought that there were plenty of others a lot more eccentric than I—in fact, downright weird, which I did not think myself to be. When I was young I did my best to hide whatever eccentricities I had.

My working life has been a curious blend of discipline and in-discipline. I have been very disciplined in the regularity of my work habits and the length of my work day; even at the pit of my prolonged depression I managed to work from morning to night, in part because I could partly escape the depression by working. And my research seems to me to always have been careful and comprehensive. But in the larger picture of what I chose to work on, and whether I continued to work on topics even though every sign pointed to the outcome being fruitless for one reason or another—there was no disciplined thought at all; I simply followed my nose into imprudent and sometimes haphazard pursuits.

I have simply followed my nose in two senses (sic). First, I have gone where I have sniffed possibly interesting new discoveries, without worrying too much about the dangers of pursuing those scents to their sources. Second and more important, there has been a very random quality to the path I have followed wherein my brain has followed behind my nose and my feet, rather than my planning faculties directing my nose.

Indeed, even on those occasions when I did analyze and plan my future—as when I decided to work on population economics in order to help lower the world's birth rate (that's how I began)—my plans often were negated and my direction eventually became the opposite of what I had planned it to be. That is, I wound up working in a field that was very good for me only because I chose it for entirely the wrong reasons, based on a completely backwards analysis of what I would find myself thinking and doing.

Another element in my development is my curiosity about almost every human enterprise. Perhaps I started out with just an ordinary kid's curiosity, but it never got stamped out of me the way that institutions (including schools) and other individuals seem to squeeze the curiosity out of most people.

It would be easy to conclude that my sense of being an outsider after I left Newark had great influence on my intellectual development and led to my ideas that have been outside the mainstream. But I think that if I had decided when at college or in the Navy to be an academic, and perhaps a psychologist rather than an economist, and if I had gone to a mainstream graduate school after getting out of the Navy, I think that I might have gone on to a rather conventional academic career. Or at least it would have been quite a few years before my thinking began to deviate from the main stream. Given the peculiar route I followed in becoming an economist, it is not surprising—if not inevitable—that I would find myself doing studies of far-out topics using unconventional methods.

Sooner or later I probably would have begun to take a radical approach to various issues because of what I now consider the key element in my thinking: skepticism. This element would have been present whether we remained in Newark or moved to Millburn. Whether or not I had begun in a more conventional educational path than I did, eventually I might even have addressed some of the same issues, such as the influence and role of IQ, which were not at all logically connected to other topics I have worked on.

But what was the cause of my skepticism? It would be easy to attribute it to my encounters with my father, who was a very unreliable authority; one was wise to be skeptical about every statement he made. But I am inclined to think that there were other important roots as well, some of them perhaps congenital. My inclination to first think concretely rather than abstractly about problems is relevant here. I remember a conversation with a cosmologist who had concocted models of the future extending far beyond the extinction of our sun 7 billion years from now. I said to him that I compared some of his conclusions about "human" life at that time with my observations about human nature now, as I always begin with the most concrete elements of information and build upwards and more abstractly from there. He responded that his thinking mode is exactly the opposite: Start at the abstract top and work downwards.

This inclination for the concrete impels me to want to see data on a phenomenon before I discuss or theorize about it, lest I find myself discussing something that does not exist. And I always ask myself what I have seen with my own eyes or heard with my own ears that does or does not fit with the generalization or theory under discussion. When reading a technical article I look first for the empirical data (if any), and then if I believe the data I am inclined to make up my own theory rather than fight my way through long algebra-laden abstractions.

Another element: I enjoy the tussle of hard learning, in exactly the same spirit that Hume describes the pleasures of philosophy being like the pleasures of the hunt and the chase. (Hume, 1739–40/1969, p. 498)

Still another element in the courses my life has taken—a complementary element to all the others, but perhaps also the dominant element—is just plain chance. After I got out of the Navy and decided to postpone going to medical school for a year, I decided to look for a job in the advertising business simply because a close friend was working in an advertising agency, and I could imagine myself being a copywriter. In the course of our discussing the advertising industry I asked him—while hanging from straps in the New York subway one afternoon—about the effects of advertising on the society. He told me the little he had learned in a course at college. So when I decided to become a professor, I parlayed my experience into a job teaching people how to do advertising, and I decided to study the economics of the subject because my casual question to my school friend was the only open research question in my mind. That's a very chancy sequence of events, with very little design in it.

As noted earlier, the only reason I came to write my deepest theoretical paper—on causality—is because Herman Wold happened to send me a couple of his preprints on the subject when we corresponded about an unrelated article that he refereed. This elicited my curiosity, aroused by an idea I could not understand: my enjoyment of trying to crack a tough intellectual nut; and a willingness to follow my nose into professional work on a topic that was completely unrelated to my "fields"—that is, outside the subjects I had been working on and that were within the boundaries of the department (advertising) and college (journalism) in which I was then teaching.

The work on causality also typified the pattern of my seeing an open door and an interesting subject, and walking through the open door even though I knew that it was not prudent to do so.

XIII

I wondered one day: If asked who among economists might make good models for a young person, whom would I recommend? Theodore Schultz and Milton Friedman came quickly to mind for the following reasons:

1. Hayek and Kuznets, whose work I learned most from, and whom I revere as economists, seem too far from American tradition to serve as models.
2. A young person can at least *begin* on the same path as Schultz and Friedman, even if he or she does not get to the same places they did. That is, one can start with workmanlike empirical studies in important areas of the economic domain, even if the work does not evolve into the kinds of theoretical advances that Schultz and Friedman achieved. And *one can tell a student where to put her or his feet to start on such a path.* But one cannot tell anyone where to put his or her feet to start on the path of an Irving Fishier, an Alfred Marshall, or a Hayek (though Marshall would recommend that one begin by reading a trade paper from any one industry).
3. There are many important economists who have done important work with the aim of impressing their colleagues, as Samuelson explicitly recommends as a criterion. I would want as models economists whose work is fueled by moral fervor, as is Schultz's and Friedman's, and that is marked by an unswerving devotion to choosing topics and publishing results without reference to funding agencies or the opinion of various publics. I also would not want for a model someone who believes that economists should not recommend governmental policies, as George Stigler so often said about himself.

XIV

The brief assessment of my life as a whole: I have lived an extraordinary lucky life.

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