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The Story of a Reluctant Economist

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I was not a reluctant economist at the start. In the beginning, economics opened up a new and exciting world. The Keynesian Revolution was in full swing, and like other graduate students, I was caught up in it. The message of the Revolution was new and straightforward -- major depressions and staggering unemployment were not an inevitable evil of industrialization. Societies had the power through public policy to prevent and correct serious depressions.

Today disillusionment with this message prevails. But it is not the failures of the Keynesian Revolution that have made me into a reluctant economist. As a teacher of introductory macroeconomics, I am still more Keynesian than my younger colleagues. Rather, my reluctance stems at bottom from a research philosophy forged at the hands of my mentor, Simon Kuznets, the third Nobel laureate in economics. In a field where theory was and is the be-all and end-all of intellectual accomplishment, Kuznets taught that the touchstone of achievement is insight into empirical reality. Moreover, other social sciences might, along with economic theory, contribute to one's understanding. It was some years before first-hand experience was to make me a true-believer of this philosophy -- and that is the story of a reluctant economist.

Stumbling into Economics

Most young people today have a good idea of their prospective work -- only about six

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percent of high school seniors respond “don’t know” when asked “what kind of work do you think you will be doing when you are 30 years old?” (Bachman et al., 1988) My problem was that I liked almost everything I studied -- English, math, history, foreign languages -- perhaps natural sciences least, but even that was not bad. I loved to read. Throughout my high school years, I was one of today’s six percent “don’t knows.” What followed was a trial-and-error period that led me eventually to economics. The path to economics was shaped partly by my own choices, but even more by factors beyond my control.

The simple model of occupational choice puts the expected rate of return in the forefront of job choice. To my generation, reared within memory of the Great Depression, income and job security were extremely important. In my personal experience, however, this factor operated largely to rule out certain choices, most notably, a youthful ambition to be a writer. But it left open a wide array of options that appeared to my limited knowledge to have quite acceptable returns.

In fact, it was events beyond my control, along with personal preferences, that led me eventually to economics. The external events were War II; government policies with regard to the draft, officers’ college training programs, and G.I. benefits; and an extremely strong post-World War II labor market for young adults due to the combination of rapid growth of aggregate demand and unprecedentedly small numbers of labor force entrants. Ultimately, I was to realize that these forces had shaped, not only my personal experience, but that of my entire generation. This revelation provided powerful confirmation for me of the insights that economics could provide into the forces shaping our lives, and led eventually to a research monograph on population and labor force that put the post-World War II boom in the perspective of past long term swings in the economy (Easterlin 1968).

In retrospect, these exogenous forces provided a succession of opportunities for me to explore my interests, with personal preferences determining where I ended up. I tried engineering, and didn’t like it. I served as a deck officer on a U.S. Navy cruiser, and didn’t like it -- although such a career had, in fact, been a serious aspiration when I was young. I tried

farming and didn't like it. I studied for an MBA degree (the combination of business and engineering was said to reap a rich harvest), and didn't like it. But, incidental to the MBA program, I was required to take economics. Finally I had discovered what I liked.

Why did economics appeal? The analytical requirements suited my abilities, but this was true too of engineering and business. In the case of economics, however, these analytical abilities were being applied to the solution of urgent social problems. My interest in these problems had been nurtured by outstanding history and English teachers in a large New York City public high school. Though I didn't realize it at the time, these teachers were forming interests that would help shape my future.

Should economic models of occupational choice give more attention to preferences? Some may say no, that individual differences in tastes tend to cancel out, and economics is, after all, interested only in group behavior. But this argument ignores factors that systematically affect group preferences as a whole. Today, when schools eliminate programs in the musical and fine arts, they are eliminating opportunities for young people to explore and develop such interests and to come in contact with possible role models in the arts. It is hard to believe that this will not affect adversely group preferences for the arts. When the media and adults make the legal profession and politicians the butt of contemptuous jokes, isn't it likely that this will affect the willingness of young people to pursue such careers? It seems likely that more systematic attention to the study of preference formation might enhance the economic modelling of occupational choice.

One lesson from this job search process might be noted, namely, failed choices sometimes turn out well. The romantic aspirations of my youth to go to the Naval Academy were frustrated while I was in high school by failing a physical exam. The subsequent opportunity to experience navy life demonstrated that it was not for me. Moreover, if I had had my way, when I did go into the navy I would have been an aircraft carrier pilot. My father, however, forced me to opt for engineering. If I'd had free choice, I probably wouldn't be writing this now. Similarly, when I decided to study for an MBA degree, my first choice was Harvard.

Had I gone there instead of being turned down, I would probably never have made it into economics. At Penn, where the economics department was in the business school, the switch from an MBA to economics was easy. I'm not sure what this means for the theory of revealed preference, but it certainly seems that ex post outcomes can be much different from those envisaged ex ante. Based on my personal labor market experience, the knowledge on which choices are based is highly imperfect, and there's a lot of "learning-by-doing" that goes into finding the niche where one's abilities and interests match job requirements.

### Socialization into Economics

Economic analysis starts from the assumption of given preferences. Yet a little reflection by economists on their graduate school experience should disabuse them of this notion. For graduate school not only teaches subject matter, it teaches the values of the economics profession -- what are the important subjects of economic research, the status hierarchy of the profession, which individuals are the proper role models. Graduate training is indoctrination.

Subject matter first, because that is what sold me on economics. I have already noted the heady atmosphere when I was a graduate student. There were the superb theoretical synopses and extensions of Keynes in Lawrence Klein's Keynesian Revolution, J.R. Hicks' LM-IS analysis, and Paul Samuelson's multiplier-accelerator interactions. There were the insights into the Great Depression in Alvin Hansen's Fiscal Policy and Business Cycles, the classic statement of the secular stagnation thesis. Moreover, as a graduate economics instructor, I had the opportunity to choose and use Paul Samuelson's brilliant introductory text when it first appeared. By comparison with the other texts then available, it was a quantum advance. It brought the Keynesian Revolution into the classroom. And it was written in a way that conveyed persuasively to students the new power of economics to work for human betterment.

I was much taken with economic theory, micro as well as macro, partly by the pure pleasure of theory for theory's sake, partly by the new conception it provided of the world about me. I was lucky to be taught by two excellent micro-theorists, Sidney Weintraub and Melvin Reder (the latter regrettably lasted only one year at Penn).

Two major methodological innovations in economics were underway at this time, the development of mathematical economics and econometrics. Penn, however, was then a backwater of graduate economic study, and my exposure to these subjects was limited. Moreover, I had had a full dose of math in undergraduate engineering, and though I liked it and did well, its novelty had worn off. So the mathematical feature of these developments did not appeal to me as it did to some from non-engineering backgrounds.

Because Penn was a backwater, its graduate program included some courses not usually offered in graduate economics. One such course, of which I was a beneficiary, was in central banking, offered by a gifted teacher and practitioner, Karl Bopp. This course helped teach me respect for historical perspective (it traced the evolution of central banking in Western Europe and the United States), and provided an insider's knowledge of contemporary monetary policy, complementing the knowledge of fiscal policy developed in Keynesian analysis.

And then there was my education in the values of the economics profession. I learned that economics is the queen of the social sciences. I learned that theory is the capstone of the status hierarchy in economics. I learned the brand names whose research I was to revere and respect. I learned that tastes are unobservable and never change. I learned that subjective testimony and survey research responses are not admissible evidence in economic research. I learned that what was then called "institutional economics" (Commons, Veblen, etc.) was beyond the pale, as were other social sciences more generally. I learned that there is a mere handful of economics journals really worth publishing in, and that articles in inter- or extra-disciplinary journals count for naught. I learned that economic measurement as then practiced by the National Bureau of Economic Research was to be denigrated as "measurement without theory."

It was years before I could shake off some of the tastes that graduate economics education had inculcated, and begin to think for myself. Some I have never overcome; thus I still hold the regard of economists above that of other social scientists.

#### Socialization beyond Economics

At Penn Simon Kuznets was a remote figure. He came in one afternoon a week to teach a graduate class and meet with his few thesis students. The courses he offered were in economic development, business cycles, and statistics; curiously, there were none that related to his pioneering research on national income. Kuznets' appointment was not in the economics department, but in the even weaker statistics department, and he participated hardly at all in the affairs of either, or in those of the university. Most of his time was spent on research off-campus at his home with occasional visits to the National Bureau of Economic Research in New York.

I took two courses from Kuznets, one in statistics which chiefly conveyed a strong skepticism toward the field and urged the use of simple, understandable methods, and one in economic development, which was essentially a course in general economic history. This development course too transmitted a strong sense of skepticism, not, however, toward economic history, but toward economic theory. Kuznets' basic point was simple: the "givens" of economics -- technology, tastes, and institutions -- were the key actors in historical change, and hence most economic theory had, at best, only limited relevance to understanding long term change. In his view what was then called "development theory" -- even the widely-hailed work of Schumpeter -- lacked concrete empirical reference.

I was impressed by Kuznets' intellect, as were graduate economics students generally, but these courses did not make me into a Kuznetsian. Rather, it was chiefly what Kuznets wrote. As a graduate student, I collaborated on several studies of national income with Raymond T. Bowman, the economics department chairman and a great admirer of Kuznets. Thanks largely to Bowman's urging, I also did a thesis under Kuznets' direction, on conceptual aspects of the measurement of economic growth. As a result of these two lines of work, I read virtually

everything Kuznets had written on national income and economic growth. It was this reading that demonstrated for me the scope, depth, and brilliance of Kuznets' mind.

Kuznets believed that insight into other times and places started, not from economic theory, but from knowledge of the facts, especially quantitative facts. It is typical of Kuznets that one of his rare speculative pieces, "Towards a Theory of Economic Growth" (1955), is mostly devoted to summarizing the facts that growth theory must explain. In the present age of endogenous technical change and the "new" growth theory, this article remains well worth reading.

Kuznets also believed that it was important to know the scholarly literature of specialists in the study of other times and places. As work on my dissertation led to a growing interest in economic development and away from macro-economic policy, Kuznets channeled me into an interdisciplinary seminar on South Asia, where I came into contact with scholars engaged in humanistic and social science research on India, and came to know some leading Indian scholars, such as N. V. Sovani. He also encouraged my tutelage in the literature of economic history by Daniel Thorner, himself an eminent scholar of Indian economic history.

It was my good fortune that Kuznets and sociology professor Dorothy S. Thomas, a renowned demographer, were starting a collaborative research project just as I was finishing graduate school. Thomas' period of graduate work in sociology at Columbia had overlapped Kuznets' in economics, and like Kuznets she had been strongly influenced by Wesley C. Mitchell. The Kuznets-Thomas project reflected this heritage. It aimed to use the United States decennial censuses from 1870 to 1950 to develop estimates of internal migration, labor force, and income by state (Kuznets and Thomas 1957, 1960, 1964). I was invited by Kuznets to do the income estimates as well as estimates of manufacturing activity.

This three-year project affected my development in two ways. For one thing, it gave me my first practical experience in economic measurement. I learned first-hand what had already been clear from Kuznets' writings, that there is no measurement without theory (Kuznets 1948ab). I also came to respect the mission of the National Bureau of Economic Research as

originally conceived by Wesley Mitchell. This was to build a broad quantitative base of economic measures that would further the “cumulation of economic knowledge” (Burns 1948, Kuznets 1947, 33-34). In my personal experience, the value of this philosophy is demonstrated by the fact that in economic history the most often cited work of mine is still my estimates of state income done as part of the Kuznets-Thomas project.

But these notions about the importance of economic measurement ran strongly against the tide of mainstream economics. I can still remember the shock and sense of betrayal I felt one day when George Stigler, himself an NBER staff member and eventual Nobel laureate, opined that a doctoral dissertation providing historical estimates of the U.S. balance of payments was not appropriate for a Columbia University Ph.D. in economics.

The other effect of the Kuznets-Thomas project was to introduce me to the field of demography. My mentor here (with Kuznets’ encouragement) was Dorothy Thomas, who in numerous coffee-katches during the project expounded on the field and its practitioners, and who forced me to attend meetings of the Population Association of America and observe and meet real demographers. Thanks largely to her influence, I acquired an education in a field outside of economics, one with quite different values. In demography, careful measurement is extolled, and those who develop techniques for making something out of fragmentary data are highly regarded. In graduate study in demography, a course in techniques of measurement is the central part of the core. In economics there has never been a methodology of measurement, and it is doubtful that a course in measurement could even make it into the graduate economics curriculum as an elective, if there were anyone with the temerity to propose it.

Demographers also place high value on establishing the factual record, exemplified for me at the time by a number of now-classic studies associated with Princeton’s Office of Population Research (Davis 1951, Durand 1948, Kirk 1946, Taeuber 1958). Such work is customarily dismissed by economists as purely descriptive. To me, however, the demographer’s respect for facts resonated with the goals of Wesley Mitchell’s National Bureau of Economic Research.

I do not wish to imply that my appreciation of demography was an overnight thing. The first draft of my paper analyzing the causes of the American baby boom (Easterlin 1968, ch. 4) was replete with the usual arrogant economist's jibes at demographic research. On reading this draft, Dorothy Thomas took me aside and said, "Look, Dick, this paper would not have been possible without all the prior demographic research that it builds on -- why not be more charitable?" I realized how right she was, and changed the tone completely. One benefit, beyond my personal education, was that the paper, when published, attracted favorable attention from demographers and established my credentials in the field.

In addition to demography, I became increasingly involved in the discipline of economic history, a field that at the time was dominated by historians. The welcome extended by both historians and demographers to the incursion of economists in their fields has always been a source of wonder to me, because my own discipline of economics has hardly reciprocated.

The situation in economic history, however, was different from that in demography. The field was astir with the potentials of the "new" economic history, whereby economists aspired to re-write history through the application of economic theory and econometrics to historical problems. I am regarded as a member of this school, and I do feel that these tools contribute to historical study. But I also believe that the traditional approach of historians was of great value, and I regret very much that they have now largely been driven from the field. Indeed, I have long felt that my early work on state income estimates would have been better if I had known more traditional American economic history. It sometimes seems these days as if the new economic history is more interested in using historical data to test economic hypotheses than in using economics to understand history. To my mind the field would have been richer if it had followed Kuznets' agenda for a comparative worldwide study of the economic growth of nations based on measurement and multidisciplinary theory (Kuznets 1949).

In any event, my experiences in both demography and economic history did much to further my socialization beyond economics. Training in economics has always been chock-full of requirements that leave little time to gain an appreciation of other disciplines. This is bad

enough, but most aspiring economists are indoctrinated in the view, as I was, that such knowledge is not even necessary, and are taught to look on other disciplines with contempt. I was lucky that both the period of my dissertation training and my early post-graduate years provided a serious counter to this. I wish that such an opportunity were more generally available to young economists today.

### The Making of a Research Philosophy

Several years ago I was the chair of the department's recruitment committee for newly-minted Ph.D.'s. In this capacity I had the opportunity to read abstracts of dissertations from a large number of students from the nation's leading graduate economics departments, an experience that reveals a lot about the discipline.

Model-building is the name of the game. Empirical reality enters, if at all, chiefly in the form of "stylized facts." Econometrics, though a formal course requirement everywhere, plays a surprisingly small part in economic research -- showing up in perhaps one dissertation in five. There is no such thing as descriptive dissertations or theses devoted to the measurement of economic magnitudes. Although topics in disciplines other than economics are not uncommon, there is little or no use of the work done in the other disciplines.

From what has gone before, it will be clear that this is a philosophy that makes me uncomfortable. I see the point of departure of research as some empirical problem, such as the American baby boom and bust. One is likely to have some theoretical preconceptions about causation, but the first step is to establish facts, both quantitative and qualitative. These facts will inform the investigator more fully about what needs to be explained, and may also suggest new possibilities regarding causation. Economic theory enters by providing a systematic framework for theorizing, but other disciplines may suggest relevant causal factors that need to be included, and supply relevant facts. Simple empirical methods provide an initial check on the consistency of theory and data; more rigorous methods are used largely to formalize one's conclusions. Qualitative evidence such as subjective statements of the actors, as found in social

science surveys or the materials of historical research (diaries, letters, etc.), should be consistent with the model.

This is not the usual approach to economic research, nor do I have any illusions that it will become more common. And it was not the approach that I started with. But it is one that has helped me to understand a little bit about the world in which I live.

There is hypothesis-testing in this approach, but a finding of support for a hypothesis is not the end of research. The goal is to explain reality, and typically this involves more than one hypothesized causal factor. With regard to occupational choice, for example, there is a substantial economics literature hypothesizing that job choice is determined by prospective returns. The goal of this literature is to establish the validity of this hypothesis. If, however, one's research goal is to explain observed job choices in a particular place in a particular period of time, it is likely that expected returns will prove to be only one factor at work, and not necessarily the most important. Thus, although expected returns have demonstrably played a part in the changing occupational choices of American college students in recent decades, the most dramatic change -- the shift towards business careers -- appears to have been driven chiefly by a marked change in life goals of the young (Easterlin 1995).

I have emphasized above the importance of instruction by the data, and the interaction between empirical study and hypothesis formulation and testing. Let me illustrate from my early experience in the study of long swings or "Kuznets cycles." Then, as now, there was the issue of whether such fluctuations were real or simply a statistical artifact. To study these swings I assembled a vast number of time series from widely differing sources -- population and its components, commodity output of various types, capital stock, labor force and employment, building permits, patents, land sales and prices, financial series, new incorporations, international trade and payments. Some series were annual, many were confined to the intermittent dates of the population and industrial censuses. The time spans differed widely. I also knew (or learned about) possible causal relationships among various subsets of these series from work by others not only on long swings, but also on building cycles, urban growth, immigration, and the like.

Ultimately it was the consistency in movements among a wide variety of series, many of which were fragmentary, and the consistency of these movements with theoretical expectations that convinced me of the reality of long swings, and led to the formulation of a broad model of economic-demographic interactions during long swings (Easterlin 1968, chs. 1-3). Perhaps someone else might have more quickly conceived such a model a priori and tested it with the few long annual time series available. For me, it took quite a few years immersed in data and various causal speculations to arrive at what was a satisfactory solution of this empirical problem.

The notion of “instruction by the data” has its pitfalls. The biggest is that the pursuit of data becomes an end in itself and an excuse for postponing serious analysis. To avoid this, data collection and analysis should proceed in tandem, not in sequence.

Concern for empirical relevance has guided my teaching as well as my research. I have taught and still teach courses ranging from introductory macroeconomics for undergraduates to the history of economic development for graduates. At the undergraduate level, I believe emphasis on empirical relevance helps students to acquire respect for economics and to appreciate its value to society. At the graduate level, I believe such emphasis is important in enabling students to understand the limitations as well as the value of economics.

#### Letting Go of Economic Theory (Mainstream Version)

It is hard to overcome the preconceptions indoctrinated by graduate economics training. In the early years of my career, I sought faithfully to explain childbearing behavior on the basis of income and prices, and to eschew appeal to preferences. I was also a devoted follower of the doctrine that behavior is always the result of deliberate choice. Reality led me to retreat from both views.

Fixed preferences went first. The empirical problem was the American baby boom and bust from the end of World War II through the 1960s. If children are a “normal good,” how does one explain the marked rise and subsequent fall in childbearing in a period when income trends

sharply upward? “Prices” won’t do it -- the opportunity cost of young women, the factor stressed most in the economics literature, was demonstrably higher during the baby boom than the baby bust.

The answer came ultimately from sociology via the concept of economic socialization. One’s notions of a desirable living level are initially formed from one’s personal experiences while growing up. The parents of the baby boom came from the economically-deprived environment of the Great Depression and World War II; the parents of the baby bust came from the economically affluent post-World War II period. Even with incomes and prices the same for the two sets of parents, one would expect differences in the willingness to have children, because of differences in the material aspirations they had formed as they grew up. The parents of the baby boom with low material aspirations and good income prospects felt relatively affluent; their children, the parents of the baby bust, with much higher aspirations relative to income, felt poorer and less able to have children. Thus, by recognizing the role of changing material aspirations (preferences) along with growth of income, a plausible interpretation was suggested of the baby boom and bust, one that proved consistent with the evidence (Easterlin 1980).

The issue of deliberate choice in demographic behavior first arose when I sought to understand the shift from high to low family size that occurs in the course of what demographers call the “demographic transition.” Like most economic demographers today, I had assumed that throughout history fertility behavior was the result of conscious choice (cf. Schultz 1981). I had already been made uncomfortable when chided by my colleague and friend at Penn, demographer John Durand, about the relevance of a deliberate choice model to the observed fertility of permanently or partially sterile women, but I thought I could get by on the grounds that quantitatively this was not very important. Matters became much worse, however, when I was confronted with a large body of survey research data in the demographic literature indicating that prior to the demographic transition most couples in developing countries reported that they did not deliberately limit family size. This implied that observed fertility behavior in these societies was what demographers call “natural” or unregulated fertility. Moreover, by the use of

an ingenious and innovative technique developed by demographers Ansley Coale and James Trussell (1974, 1975), a persuasive case was made that natural fertility had also been the common condition in the historical experience of the developed countries prior to the shift to low fertility. Eventually, aided by colleagues at Penn, I arrived at an explanation of these pre-transition conditions consistent with economic theory and rational behavior -- an explanation that incorporated the demographers' concept of natural fertility (Easterlin 1978, Easterlin, Pollak and Wachter 1980, Easterlin and Crimmins 1985). But the result led me to recognize that the mechanisms underlying observed fertility-income patterns in pre-transition societies might have nothing to do with deliberate household decisions about family size, and reflect, instead, social norms or physiological relationships. Thus, variations in fertility in pre-transition societies might result from variations in breastfeeding behavior that arose, not from interest in or even awareness of the effect of breastfeeding on family size, but simply from different societal conceptions of the link between breastfeeding and the health of mother and child. In such circumstances, fertility was being determined, not by conscious choice, but inadvertently by household decisions directed towards other objectives.

Recently, some economists have sought to bring the subject of mortality, as well as fertility, under the dominion of the theory of household choice (see, for example, Schultz 1981 and the survey by Behrman and Deolalikar 1988). The leading empirical problem with regard to mortality is the amazing decline that has occurred in both developed and developing areas over the past century (this decline is the other component of the demographers' demographic transition). The suggested explanation is straightforward: health is the product of conscious household decisions. Growth in income associated with economic development induces an improvement in the quantity and quality consumed of food, clothing, shelter, medical care, etc., and this, in time, improves health and reduces mortality.

I cannot plead innocence of this view; at one time I used it explicitly to infer, in the absence of direct measures of mortality, the probable course of American mortality in the nineteenth century (Easterlin 1977). As in the case of fertility, however, the more I studied the

literature outside of economics, the more I was led to question such a simple economic model. Before the latter part of the nineteenth century, households, governments, and those in the healing arts had little knowledge of how to prevent or treat disease, a situation that prevails even today in large parts of the Third World (Caldwell et al., 1989). In such circumstances, actions by households or others to prevent or treat disease -- however well-intentioned -- were largely ineffective. It was not until the growth of epidemiological knowledge and the validation of the germ theory of disease in the middle and latter part of the nineteenth century that truly effective action started to become possible (Easterlin 1996, chapter 6). The leadership in implementing this knowledge was provided, not by households, but by public entrepreneurs, the leaders of the new public health movement. Initially, in the mid- and latter part of the nineteenth century the focus of the public health movement was on water and sanitation measures to clean up the environment, measures that significantly reduced mortality independently of household decisions. As time went on and knowledge continued to grow, the emphasis of public health officials shifted towards measures to assure a purer food supply, and education of the public in personal hygiene, maternal and childcare practices, good nutrition, and the importance of immunization. This new knowledge made it possible for the first time for households generally to make informed decisions to prevent disease. Eventually, as the continuing advance in knowledge led to the development of chemotherapy in the 1930s and thereafter, the medical profession became equipped with drugs that made possible an effective response to household demands for the cure of disease.

Thus, as knowledge has advanced, the determination of health and mortality has been brought increasingly within the province of human control. Today in developed societies deliberate household decisions, along with those of medical practitioners and governments, can improve health and reduce mortality. But this was not always true; nor is it true even today in large parts of the Third World.

Letting go of the preconceptions of economic theory did not come easily. It was brought about chiefly by two fundamental beliefs instilled in me by Kuznets. One was taking an

empirical problem as the point of departure for research. The other was respect for the evidence accumulated by specialists on the subject -- in the case of fertility and mortality, chiefly that of demographers, public health specialists, historians, and anthropologists. Survey evidence that demonstrated the widespread absence in time and space of deliberate control of family size had to be accepted as reflecting empirical reality, and a plausible model had to explain such behavior. Regrettably, many economists define such observations away by dismissing subjective testimony as inadmissible (Easterlin 1986). Had I not had the benefit of schooling in demography, I would have missed out on an opportunity better to understand observed behavior. Similarly, in regard to mortality, evidence had to be recognized on the immense advance in knowledge regarding the control of communicable disease, and the key role of public entrepreneurship in implementing this new knowledge. Economists' insistence on starting with household choice put the cart before the horse.

One reason why most economists start with the theory of household choice is that its relevance to behavior in contemporary developed societies has been well-demonstrated. As mentioned earlier, my own model of the American baby boom and bust employs the theory of household choice, although expanded to allow for systematic variation in preferences. Similarly, in the low mortality regime of the United States today, understanding household choices regarding life style and health care utilization help provide insights into American health and mortality differentials and trends. Thus, with regard to both fertility and mortality there has developed in recent decades a substantial literature focussed on the developed countries, and especially the United States, in which the theory of household choice plays a central role. Unfortunately, this literature has become the point of departure for economic research on fertility and mortality in other times and places. In effect, a theoretical predisposition has become the starting point of research, rather than empirical study of other societies. Such empirical work is readily available in the scholarly work of other social scientists and area specialists, but economists have been taught to dismiss such work and trust to the wisdom of economic theory. As a result, economics approaches the study of other times and places through glasses tinted by

preoccupation with the study of contemporary American society. This is not the way Simon Kuznets would have had it. By word and example, Kuznets taught a respect for facts, and for other social sciences.

As I have indicated, making the break with the assumption of given preferences wasn't easy. But once made, the effects ramified. The relative income model used to explain the baby boom and bust was based on recognition that one's material aspirations were shaped in important part by one's material environment. Previously, I had confidently assumed that higher income and greater subjective welfare go hand-in-hand. But the relative income model implied that a generation raised under more affluent circumstances would, as a result, have higher material aspirations -- in short, that aspirations varied directly with society's income. Thus the positive effect on material welfare of income growth would be negated by the adverse effect of increased aspirations. Subjective testimony on personal happiness provides striking confirmation of this expectation: as per capita income grows over time, subjective welfare remains unchanged, despite a marked growth in material possessions (Easterlin 1974, 1996, ch. 10). Again, mainstream economics has spared itself confrontation with the evidence by its dogmatic rejection of subjective testimony on well-being -- this, despite a large research literature in psychology and sociology pointing to the meaningfulness of such measures. At the present, hypothesis-testing in economics regularly incorporates collateral evidence such as prices and wages; one can perhaps hope that in time the bounds of admissible evidence come to embrace as well measures devised in psychology and sociology relating to circumstances such as expectations, values, and subjective feelings. Although such factors are sometimes posited in economic models, evidence relating directly to them is almost never considered.

In the study of subjective well-being, as in my work on demographic topics, I again learned the value of research in disciplines outside of economics. My work on subjective well-being was initiated in 1970-71 while I was a fellow at the Center for Advanced Study in the Behavioral Sciences. This fellowship provided an opportunity for extended contact with scholars in psychology and sociology who introduced me to relevant work in these fields. Thus,

during my career, I have been fortunate to have the opportunity for serious contact with scholars and work outside of economics -- first in demography and history, then in sociology and psychology -- that taught me at first-hand the value of Kuznets' stress on the relevance to reality of social science generally, not just economic theory.

### An Historical Perspective

We live today in the midst of two great revolutions that are sweeping the world and have changed human life forever. The Industrial Revolution of the late eighteenth century marked the onset of modern economic growth, a phenomenon that has raised material living levels by ten-fold or more among the leaders in the process. The Mortality Revolution that started in the late nineteenth century has already more than doubled life expectancy at birth in many parts of the world. Together, these revolutions portend as early as a half-century hence a world largely freed from hunger and starvation, and from enormously high rates of infant and child mortality (Easterlin 1996).

The origins of these revolutions lie in the development and growth of natural science since the late seventeenth century. Scientific knowledge grew earliest in the fields of mechanics, astronomy, chemistry, and electricity, and had its payoff in widespread and continuing improvements in methods of production that raised productivity and per capita income. Scientific knowledge came later in the biological and medical fields, and because of this, the Mortality Revolution, based on new methods of disease control, started later than modern economic growth. The factor input and institutional requirements of the Mortality Revolution are less than those for modern economic growth, and because of this the Mortality Revolution has spread more rapidly.

Thus, a new world is being erected on the advances in natural science. This world is richer and healthier, but it is also much more complex and incredibly interdependent. It is a world of staggering new problems -- abrupt shifts in political power as modern technology

spreads, new environmental concerns arising from the side-effects of this technology, and internal conflict within nations between gainers and losers in economic growth.

Ultimately, the solution to such problems depends on the newly emerging social sciences. I am an economist because I believe that economics is essential to understanding the world and that the framework of economic theory enables one to think systematically about many interrelationships. Indeed, the first major pay-off to the advance of social science knowledge was, as I have noted, the insights of the Keynesian Revolution into one of the major new problems of economic growth, mass unemployment. It is unfortunate that the profession of economics has retreated from this belief in the ability of economic science to help us control our destiny, because the need for relevant research is greater today than ever before.

But economics alone is not enough -- and this is why I am a reluctant economist. We cannot comprehend the world about us without knowledge of the facts and insights provided by the other social sciences. Economics is a starting point, but only a starting point, in the application of social science to the world's problems. As I reflect on my own philosophy, instilled by Kuznets and molded by experience, it boils down to a few words -- it is good to be an economist, it is better to be a social scientist.

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