

# **The Reluctant Economist**

*Perspectives on Economics,  
Economic History, and Demography*

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PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE  
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS  
The Edinburgh Building, Cambridge CB2 2RU, UK  
40 West 20th Street, New York, NY 10011-4211, USA  
477 Williamstown Road, Port Melbourne, VIC 3207, Australia  
Ruiz de Alarcón 13, 28014 Madrid, Spain  
Dock House, The Waterfront, Cape Town 8001, South Africa

<http://www.cambridge.org>

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First published 2004

Printed in the United Kingdom at the University Press, Cambridge

*Typeface* Times Ten 10/13.5 pt.    *System* L<sup>A</sup>T<sub>E</sub>X 2<sub>ε</sub> [TB]

*A catalogue record for this book is available from the British Library.*

*Library of Congress Cataloguing in Publication Data*

Easterlin, Richard A., 1926–

The reluctant economist : perspectives on economics,  
economic history and demography /

Richard A. Easterlin. – 1st ed.

p. cm.

Includes bibliographical references and index.

ISBN 0-521-82974-7 (cloth)

1. Economic history. 2. Demography  
3. United States – Economic conditions. I. Title.

HC26.E17 2004

330 – dc21      2003055136

ISBN 0 521 82974 7 hardback

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## The Reluctant Economist

At the start, I was not a reluctant economist. 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 among economists. But it is not the supposed 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 many of my 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 in which 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. But it was some years before firsthand experience was to make me a true believer in this philosophy.

### STUMBLING INTO ECONOMICS

Most young people today have a good idea of their prospective work, for only about 6 percent of high school seniors respond "don't know" when

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asked about the kind of work they think they will be doing at age 30 (Bachman, Johnston, and O'Malley 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 6 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 economist's simple model of occupational choice puts the expected rate of return in the forefront of job choice. To my generation, reared in the shadow of the Great Depression, income, along with job security, was certainly very 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 World War II and the veterans' policies associated therewith plus an extremely strong post-World War II labor market for young adults. Eventually, I was to realize that these forces had greatly influenced not only my personal experience but also 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 1968b).

In retrospect, these exogenous forces provided a succession of opportunities for me to explore my interests, and personal preferences determined 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 M.B.A. degree (the combination of business and engineering was said to reap a rich material harvest) and didn't like it. But, incidental to the M.B.A. program, I was required to take economics. Finally, I discovered what I liked.

Why did economics appeal? The analytical requirements suited my abilities, but this also was true 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 are irrelevant or tend to cancel out and economics is interested only in group behavior. But this argument ignores factors that systematically affect group preferences as a whole. It seems likely that more systematic attention to the study of preference formation might enhance the economic modeling of occupational choice – a point to which I will return later.

One lesson from my own job search process may be noted, namely, failed choices sometimes turn out well. The romantic aspirations of my youth to go to the Naval Academy were frustrated by my having failed a physical exam. A subsequent opportunity to experience Navy life, thanks to participation in the Naval Reserve Officers Training Program, 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 M.B.A. 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 the University of Pennsylvania, where the economics department was in the business school, the switch from an M.B.A. 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*. On the basis of my personal labor market experience, the knowledge on which choices are based is highly imperfect, and much "learning-by-doing" goes into finding the niche where one's abilities and interests match job requirements.

#### SOCIALIZATION IN ECONOMICS

Economic theory, as taught to undergraduate and graduate students, starts from the assumption that preferences are given and unchanging. Yet a little reflection by economists on their graduate school experience

should disabuse them of this notion. Graduate school not only teaches subject matter but also the *values* of the economics profession – what are the important subjects of economic research, what is the status hierarchy of the profession, which individuals are the proper role models. Graduate training *is* indoctrination (Klamer and Colander 1990; Reder 1999).

But I will consider subject matter first because that is what sold me on economics. I have already noted the heady atmosphere when I was a graduate student at Penn. There were the superb theoretical synopses and extensions of Keynes in Lawrence Klein's *Keynesian Revolution*, J.R. Hicks's 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 an 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 and partly by the new conception it provided of the world about me. I was lucky to be taught by two excellent microeconomic theorists of the time, Sidney Weintraub and Melvin Reder (the latter regrettably moved on from Penn after only one year).

Two major methodological innovations in economics were under way at this time: the development of mathematical economics and econometrics. Penn, however, was then a backwater of graduate economics 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 nonengineering backgrounds.

Penn's graduate program included some courses not usually offered in graduate economics. One such course, of which I was a beneficiary, was in central banking and was 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). Bopp was a vice president at the Federal Reserve Bank of Philadelphia, and the course also provided his insider's knowledge of contemporary monetary policy, complementing my understanding 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 extradisciplinary journals count for naught. I learned that economic measurement as then practiced by the National Bureau of Economic Research (NBER) 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 pay disproportionate attention to economists' judgments of my work.

#### SCHOOLING 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's 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 NBER 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's basic point was simple: the "givens" of economics – technology, tastes, and institutions – are the key actors in historical change, and hence most economic theory has, at best, only limited relevance to understanding long-term change. In Kuznets's view, what was then called "development theory" – even the widely hailed work of Schumpeter – lacked concrete empirical reference.

I was impressed by Kuznets's 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's 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's 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," 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 1955, see also Kuznets 1966).

Kuznets also believed that it is 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 macroeconomic policy, Kuznets channeled me into an interdisciplinary seminar on South Asia, where I came into contact with scholars doing humanistic and social science research on India and came to know some leading Indian scholars such as N. V. Sovani. Kuznets also encouraged my tutelage in the literature of economic history by Daniel Thorner, who was himself an eminent scholar of Indian economic history.

It was my good fortune that Kuznets and sociology professor Dorothy S. Thomas, a renowned demographer and the first woman

president of the American Sociological Association, were starting a collaborative research project just as I was finishing graduate school. Thomas's period of graduate work in sociology at Columbia University had overlapped Kuznets's in economics, and like Kuznets she had been strongly influenced by Wesley C. Mitchell. Mitchell, an institutional economist at Columbia, was head of the recently founded, privately financed NBER. The Kuznets–Thomas project reflected this heritage. It aimed to use the U.S. 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 firsthand what had already been clear from Kuznets's writings: that there is no measurement without theory (Kuznets 1948a,b). I also came to respect the mission of the NBER as originally conceived by Mitchell. This was to build a broad quantitative base of economic measures that would further the “cumulation of economic knowledge” (Burns 1948; Kuznets 1947, 33–4). 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 in the 1950s 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 economic theorist 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's encouragement) was Dorothy Thomas, who in numerous coffee klatches during the project expounded on the field of demography and its practitioners and 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 core of the requirements. 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, which was exemplified for me at the time by several 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 NBER.

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 1968b, Chap. 4; see also Chapter 11 herein) was replete with the usual arrogant economist's jibes at demographic research. On reading this, 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 was shamed into remembering a characteristically pertinent maxim of J. M. Keynes: "If economists could manage to get themselves thought of as humble, competent people, on a level with dentists, that would be splendid" (Keynes 1932, 373). The outcome was that I changed the tone completely. One benefit, beyond my personal training, 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 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 rewrite 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's 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 education 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 the period of my dissertation training and my early postgraduate years provided a serious counter to this. I wish that such opportunities were more generally available to young economists today.

#### THE MAKING OF A RESEARCH PHILOSOPHY

Several years ago I was the chair at the University of Southern California of the economics department's recruitment committee for newly minted Ph.D.s. In this capacity, I had the opportunity to read abstracts of dissertations from many students from the nation's leading graduate economics departments, which was an experience that revealed a great deal 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 use or knowledge of the work done in 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 post–World War II 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, drawing, as needed, on relevant work not only in economics but in other social sciences as well. 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 brought into the theoretical analysis and also supply pertinent facts. Simple empirical methods provide an initial check on the consistency of theory and data; more rigorous methods are used subsequently to formally test 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 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. For example, I referred earlier to the substantial economics literature hypothesizing that occupational choice is determined by prospective returns. The goal of this literature is largely 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, one of the most dramatic occupational developments – the shift toward business careers in the 1970s and 1980s – was driven chiefly by a marked change in preferences as evidenced by life goals of the young (see Chapter 12).

I have already emphasized the importance of instruction by 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” in population and the economy. 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, and international trade and payments. Some series were annual, and 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 1968b; see also Chapter 5 herein). 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 several years of working through data and exploring various causal speculations before I arrived at what seemed a satisfactory understanding 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 theoretical analysis. To avoid this, data collection and analysis must proceed in tandem, not sequentially.

#### 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 1970s. If children are a “normal good,” how does one explain the marked rise and subsequent fall in childbearing in a period when income moves sharply upward? “Prices” won’t do it, for the opportunity cost of young women, the factor stressed most in current economic literature, was demonstrably higher during the baby boom than the subsequent 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 experience 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 them to differ in their willingness to have children because of disparities 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 material aspirations relative to income, felt poorer and less able to have children. By recognizing the role of changing material aspirations (preferences) along with growth of income, I was able to arrive at a plausible interpretation of the baby boom and bust – one consistent with the evidence (Easterlin 1980; Chapter 11 herein).

Another empirical problem undermined my conviction that behavior could always be explained as being deliberate choice. In this case, the problem was the shift from large to small 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 the University of Pennsylvania, demographer John Durand, about the irrelevance of a deliberate choice model to the observed fertility of permanently or partially sterile women, but I thought