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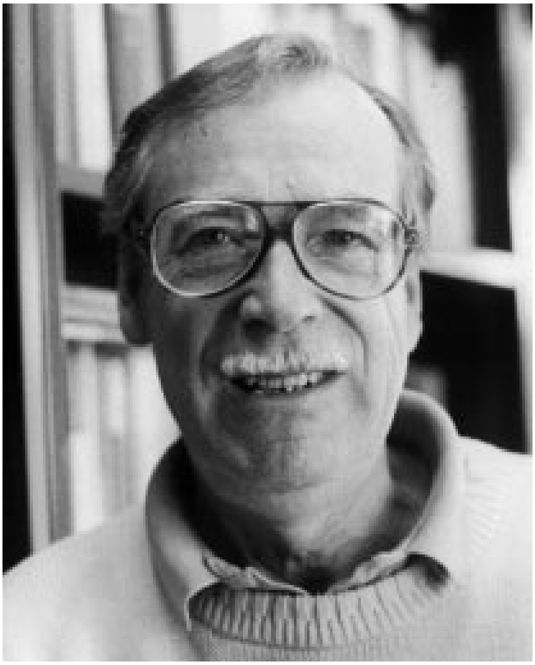

Richard A. Easterlin,
Professor of Economics
at the University
of Southern California,
Los Angeles, USA

A conversation with Richard Easterlin

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Abstract. After an introduction touching on various biographical highlights, this paper summarizes a wide-ranging discussion with Richard Easterlin which occurred in the Autumn of 1996. We considered the Easterlin Hypothesis – its genesis and current status, together with Easterlin's views on attempts to develop measures of relative income – and then moved on to "The Fertility Revolution" and questions regarding the applicability of the theory of household choice in modernizing societies. This was followed by a discussion of his early career development and influences on him at that time, ending with ruminations regarding the current

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state of economics, and the validity of training given to young economists today.

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1. Introduction

It seems appropriate to begin an introduction to Richard Easterlin with a quotation from his own ‘Principal Contributions’ section in *Who's Who in Economics* (Blaug 1986):

“I would like to feel that my work has provided some insight into both the long-term trend and post-World War II swing in American fertility; the factors responsible for the demographic transition in today’s developing countries; the importance of education in the spread of economic development; the interrelations between social conditions and economic change; and the relativity of economic welfare. Perhaps also it may have contributed to a better economic theory of human fertility, and to the promotion of relative income-type concepts in economic analysis (p. 237)”.

Few scholars manage to achieve their life’s goals to the extent that most would agree Dick Easterlin has, with hopefully many productive years still ahead. And in addition, in both the economic and demographic fields he is known best, perhaps, as one of the foremost promoters of a very noble cause: “to reconcile ... important theoretical concepts in other social sciences, such as ‘relative deprivation’ and ‘natural’ (i.e. unregulated) fertility ... with received economic doctrine (p. 237)”.

It was in recognition of these highly valued contributions that in 1993 he was awarded the prestigious Irene B. Tauber Award by the Population Association of America, an organization which had elected him as President in 1978. He was also President of the Economic History Association in 1979–1980. His presidential address to the PAA (Easterlin, 1978), set a new standard for presidential addresses to that organization, with its empirical content and real world relevance. That address was the first comprehensive presentation of his innovative ‘Easterlin Hypothesis’ – that the relative size of a birth cohort determines the labor market outcome of that cohort, which in turn has repercussions on a host of socioeconomic characteristics including fertility, creating the potential for continuing fluctuations in the relative size of birth cohorts.

Born in 1926 in Ridgefield Park, New Jersey, his career began with degrees from the Stevens Institute of Technology (M.E.; Mechanical Engineering, with distinction in 1945), and the University of Pennsylvania (A.M. in 1949 and Ph.D. in 1953, both in economics). It was only after receiving his Ph.D. that he was drawn into demographics through participation in a 1953–1956 project on population redistribution and economic growth with Simon Kuznets and Dorothy Thomas. It was also because of

Simon Kuznets that he developed his affinity with empirical research, and concern with understanding real world situations, which are clearly evident in all of his work.

Prior to his current position at the University of Southern California, which he took up in 1982, he spent nearly thirty years on the faculty of the University of Pennsylvania, where he was William R. Kenan Jr. Professor of Economics from 1978–1982. He has been named a Fellow of the Center for Advanced Study in the Behavioral Sciences (1970–1971), the American Academy of Arts and Sciences (1978), the Econometric Society (1983), and the Guggenheim Foundation (1988–1989), and has been a Visiting Professor and Scholar at the California Institute of Technology, Stanford, Texas A&M, the University of Washington, and the University of Warwick in England.

In addition to the 'Easterlin Hypothesis', which incorporates his 'relative economic status' hypothesis, he is well known for developing – in collaboration with his wife, Eileen Crimmins – the 'Easterlin synthesis framework', or Easterlin-Crimmins model: a comprehensive framework for analyzing social and economic aspects of the fertility transition. The supply/demand framework underlying the Easterlin-Crimmins model has become an accepted way of categorizing fertility variables, for most researchers. We begin our interview with a discussion of the first of these models.

2. The Easterlin hypothesis

DJM: You are, of course, probably best known for your formulation of the 'Easterlin Hypothesis'. What is the essence of that hypothesis?
RAE: The basic idea is that relative income determines behavior. By relative income, I mean one's assessment of earnings prospects in relation to an internalized norm of one's desired living level – what one might think of as a socially defined subsistence level.
DJM: How would you respond to comments I recently received from a referee asking: "Why would people base decisions on consumption relative to expectations rather than the actual level of consumption?"
RAE: Whether behavior is governed by absolute income or relative income cannot be decided *a priori*, as the referee implies, but is an empirical question. I believe that there is growing evidence for, and interest in, the relative income concept. This appears to be more true in Europe where scholars are less paradigm-bound than in the United States.
DJM: How do you think the concept of relative income should be embodied empirically? How can we measure an individual's preferences regarding a desired standard of living?
RAE: The biggest issue in formulating a measure of relative income is how material aspirations are determined. The general notion suggested in sociology that as one grows up there is an economic socialization process that creates internalized norms seems to me to be correct. But the specifics of this mechanism, and its operation over the life cycle, remain uncertain, and I do not have a nice new formal model to offer. When I began working along these lines, I made some tentative guesses, but my hope was that

new empirical research might be induced on preference formation. There have been some developments of this type, but surprisingly few, given that my work was initially published about three decades ago.

DJM: In the absence of any definitive research on preference formation, there has been a wide diversity of measures which have been developed empirically and tested in the name of the 'Easterlin Hypothesis'. These range from unemployment ratios to responses to survey questions such as 'How well off are you?' Often the measures used seem highly inappropriate, given the spirit of the 'Easterlin Hypothesis', and it seems hardly surprising when such studies produce insignificant results. Do you think this use of inappropriate measures of relative income is due to constraints imposed by data availability?

RAE: I think it's just a lack of thought. The simplest thing is just mechanically to try various measures rather than reflect on whether the measures capture the conceptual idea.

DJM: Some studies, for example, have compared the current average income of all males, to lagged values of that same variable – demonstrating a serious misunderstanding of the concept. Do you find this frustrating?

RAE: Yes, I do. I've tried to stress that it is important to look at the particular circumstances of young people, and measures based on data for all adults fail to embody this essential notion.

DJM: In addition to the relative income concept, I think that most people associate the 'Easterlin Hypothesis' with ideas of birth cohort size, and the American post-World War II baby boom. Could you explain a bit about that connection?

RAE: The basic idea is this. If there is a fairly stable growth and composition of aggregate demand, then the labor market success of birth cohorts will vary inversely with their size. The decade or so after World War II witnessed the labor market entry of the smallest birth cohorts on record, echoing the birth rate lows of the 1920s and 1930s, and the relative affluence of these cohorts produced the baby boom. The large baby boom cohorts, in turn, suffered from a relatively adverse labor market by virtue of their size, and produced the baby bust.

DJM: Do you think population cycles will simply repeat themselves: that, if the Hypothesis holds, we will return to the United States of the 1950s?

RAE: No. In the last chapter of *Birth and Fortune* (Easterlin, 1987) I discuss secular and other factors that are likely to modify future fluctuations as compared with the experience of the 1950s.

DJM: How does the Hypothesis hold with regard to the United States prior to WWII – and to other countries?

RAE: The argument of *Birth and Fortune* is premised on governmental monetary-fiscal policies that stabilize growth of aggregate demand, conditions that did not prevail prior to World War II. Although the general reasoning would apply to other developed countries with similar macroeconomic policies post-World War II, labor market institutions, such as income or employment guarantees, can obviously modify the mechanism. The United States is at the extreme in terms of the role of free market forces in determining labor market conditions.

DJM: Have you found it a problem that people often assume the 'Easterlin Hypothesis' predicts continuing cycles? Do you think that draws attention away from the basic meaning of the 'Easterlin Hypothesis', or the most important points of the Hypothesis?

RAE: Some reactions have been along fairly simple and mechanistic lines. But Fred Pampel's recent work is an example of a thoughtful attempt to consider how institutional differences might modify the effect of cohort size. And your work exploring different relative income measures is another example of a direction in which I would like to see people move.

DJM: And Ron Lee's work? He [and Ken Wachter] have produced several articles in which the attempt was to see whether the cycles would just repeat themselves, or whether they'd dampen out.

RAE: Yes, certainly that's useful, because it is an attempt to extend the Hypothesis. What I'm disappointed in is simple replication in different countries or times of the particular measure that I used, without attention to the whole labor market situation, such as institutional variations or the assumption about labor demand conditions.

DJM: Why wasn't 1984 as you predicted in 1978 it would be?

RAE: "1984" was a term used as a proxy for the future, à la George Orwell. I think, in fact, that *Birth and Fortune* gives a good analysis of the forces that have shaped the experience of the baby boom generation. It's less clear how well it anticipates the fate of their successors. Clearly, there have been several important developments not foreseen in the book – the general slowing in the rate of economic growth after 1973 and the widespread backing away from full employment as an objective of macro-economic policy. Probably also the saturation effect of the boomers in the labor market is not given enough weight, a point that you have made clear.

DJM: Many feminists seem to view your Hypothesis as demeaning to women. I have known women to ask, "Why are women so passive in your model? Why are they assumed to focus on male income, instead of their own?"

RAE: This question comes as a surprise to me. The statement of the relative income hypothesis in *Birth and Fortune* is in terms of a *couple's* aspirations. My empirical approximation in terms of male relative income is explicitly a simplification, and viewed as a conservative picture of a *couple's* prospects. Women are no more passive than men in my model.

DJM: Some of my students have objected to the tone of *Birth and Fortune:* they feel that you are a social conservative who wants us to return to the 1950s. For example, your discussion there of social attitudes toward gender roles relies largely on statements made in the 1970s: students today don't identify, and think things have changed dramatically since then. Have they? And throughout the book, the assumption is made that one of the first things couples will do when relative income rises, is marry and have children. Does this still apply to today's young people?

RAE: What I *want* with regard to gender roles has nothing to do with *Birth and Fortune.* In my opinion, we are all prisoners of the social conditions that produced us – men and women alike. I see no reason to feel sorrier for the roles women have been assigned than for those given to men.

The discussion in *Birth and Fortune* of gender roles is based on what I took to be the facts at the time, as given by the evidence I cite. I think there has since been a shift in life goals that started around the time that the book was written. Women have become almost as materialistic as men, and have come to feel that they must be prepared to provide for themselves. This change in attitudes dates from what Frank Levy calls the Quiet Depression that started in 1973, and is analyzed in my *JEBO* 1995 article on the shift to business careers (Easterlin 1995). The adverse labor market circumstances of the baby boom generation described in *Birth and Fortune* have clearly played an important part in the growth in young women's attitudes about the need to be able to fend for themselves. The shift in attitudes has mainly put women in the position of being breadwinners as well as mothers, and is testimony to the immense pressure of rising material aspirations. As far as I'm aware, there is little evidence that men's roles have altered. Women, for example, remain responsible for household management, and men have done little to relieve women of these duties.

DJM: Have you seen the recent article by Rindfuss and Brewster (1996) in *Population and Development Review?* It attempts to determine whether the change in attitude toward women's roles preceded or followed the change in women's behavior.

RAE: Yes, they conclude that behavior changed first, which is, I believe, the usual finding.

DJM: It is sometimes said that you devote very little time in *Birth and Fortune* to examining effects of changing relative income on female labor force participation. Why?

RAE: Actually, I thought that one of the distinctive features of *Birth and Fortune* is that labor force participation of *both* younger and older women is taken as a dependent variable along with fertility. In contrast, the Butz-Ward model basically explains fertility in terms of young women's labor force participation, and disregards the movement in older women's participation, which is virtually the inverse of that of younger women. It has always seemed to me that in generalizing about the strength of the labor market for women, it is essential to explain the disparate developments between older and younger women.

DJM: Are there changes or deletions you would make to the theory if you were writing *Birth and Fortune* today? To your method of presentation? What do you consider to be the most interesting or pressing research issues currently?

RAE: I think the theory correctly characterizes what I once termed the conflict between material aspirations and resources that is everyone's experience.

The most challenging issue for research remains the formation of material aspirations – both measuring aspirations and determining their causes. For example, I believe there are useful survey data on aspirations that have been collected by market research organizations, and could be used for scholarly analysis. Identifying who has these data, assembling them, and putting them in usable form is a sizeable task, but the potential payoff is substantial.

DJM: What type of developments over the next decade – if any – would cause you to reconsider or alter your hypothesis?

RAE: Well, for one, a revolutionary shift in young people's perceptions of their life goals in an anti-materialistic direction, but this is not likely to happen, Ron Inglehart's work notwithstanding.

DJM: You've presented a number of articles in recent years establishing the economic basis for individuals' demographic adjustments (postponed marriage, reduced fertility, the emergence of two-earner families, and individual living arrangements): is it more important to you to establish these economic foundations of demographic behavior at the micro level, or to explain cycles at the macro level? That is, do you consider the concept of male relative income and its effects, or the effect of cohort size on relative income, to be the central feature of your work?

RAE: The concept of relative income is more fundamental; the significance of cohort size for relative income is a product of special post-World War II circumstances.

But relative income does not play a central role in my interpretation of the transition in fertility and mortality from high to low. I guess I'd like to feel that the central feature of my work has been that it provides plausible interpretations of certain aspects of historical experience consistent with the evidence.

DJM: What was the genesis of your interest in relative income? What part, if any, was played by Duesenberry's 1949 dissertation? Did you see a direct connection? Can you describe the evolution of the concept?

RAE: My initial formulation of the relative income hypothesis was in a paper I gave at the PAA in the mid-sixties. In economics a major issue since World War II had been the explanation of saving rates, and there was considerable controversy regarding the role of absolute versus relative income, the latter being represented by the work of Duesenberry and Modigliani. These relative income arguments were no doubt in the back of my mind as I wrestled with the problem of the downturn in fertility. Modigliani's formulation seemed the most relevant, because it focussed on behavior over time. But my thinking was influenced also by the concept of socialization that figured in the literature of demography and sociology. In practice, I spent relatively little time on theory, and most of the time I was trying to mobilize data that would help quantify and test the hypothesis.

DJM: Did you realize immediately the importance and implications of the concept?

RAE: As for the importance of this line of research, it meant a lot to my personal thinking about economic theory. Previously I had sought faithfully to take preferences as a given. This experience with explaining real world fertility behavior led me to abandon that notion. However, it seemed to me then, and still does, that the *framework* of economic theory lends itself readily enough to incorporating the study of preference change. I think it is unfortunate that there are economists who arbitrarily define the study of preference change as not 'economics' and hence exclude the study of preferences from the field. This has the unfortunate effect of limiting the potential scope of economic analysis, and erecting a barrier between economics, on the one hand, and sociology and psychology, on the other.

Aside from my personal thinking, I knew that the relative income concept had implications for mainstream welfare economics, and when in 1970–1971 I had the opportunity to spend a year at the Center for Advanced Study in the Behavioral Sciences, I was fortunate to find data enabling me to pursue these implications empirically. On this subject, Duesenberry's prior work was helpful, but so too was the substantial amount of research outside of economics on relative deprivation, to which I was introduced by scholars at the Center in sociology and psychology.

DJM: Do you consider your work to be revolutionary?
RAE: No. In economics, revolutions occur in theory, and my work is simply an attempt to use established social science theories to interpret certain aspects of human experience in a particular place in a particular period of time. The real revolutionaries were people like Duesenberry and Modigliani in economics, Durkheim and Merton in sociology, and Stouffer and Festinger in psychology. The discipline of economics, unfortunately, has yet to recognize the reality of this revolution.
DJM: I was thinking here specifically about the idea that preferences are not immutable: you would trace that back to Duesenberry and Modigliani?
RAE: Yes, at least for starters.
DJM: And yet has anyone ever questioned that aspect of their hypotheses – the concept of shifting preferences? Was their work dismissed on that basis in the way some of your work has been?
RAE: I think their relative income work has suffered largely from neglect, not from serious criticism. Most of the attention paid to the Duesenberry book was in regard to savings rates, though the book covered a much wider range of subjects. Then Friedman came along with the permanent income hypothesis in his *Theory of the Consumption Function* and that largely supplanted the Duesenberry *relative* income explanation of savings. Relative income was replaced by permanent and transitory income components, and after that people just stopped paying attention to relative income. Perhaps there's some revival going on now – Frank's work is an example (Frank 1985) – but my sense is that mainstream theorists prefer not to have their lives complicated by the issues of interdependence brought out in the relative income approach.
DJM: You moved from the relative income concept in developed countries, to issues surrounding the demographic transition. What led to that change?
RAE: During much of my career the two problems in the forefront of the demographic literature on fertility have been the post-World War II baby boom and bust and the demographic transition. I was initially convinced that economics could contribute to understanding these problems. It was some time before I came to appreciate that demography would lead to a rethinking of economics.

3. Fertility revolution

DJM: Your book with Eileen – *The Fertility Revolution* (Easterlin and Crimmins 1985) – generated a heated debate between you and Paul Schultz. What were the main issues in that debate?
RAE: As I see it, one issue is the relevance of the theory of household choice to the explanation of demographic behavior in all times and places. While Paul continues to be convinced of its universal relevance, I have come over the years to see this theory as applicable chiefly to demographic behavior in modernized or modernizing societies.

The other principal issue is the admissibility of subjective testimony of the type generated by social scientists outside of economics. Paul dismisses such testimony, falling back on the disciplinary paradigm of behavioral positivism. This methodological position, I feel, creates an unnecessary barrier to better understanding of human behavior via the study of self-testimony.

DJM: Questioning the universal relevance of the theory of household choice – could you elaborate on that a bit?

RAE: The issue relates to both fertility and mortality. Let's take fertility first. Like most economic demographers today, I had assumed that throughout history fertility behavior was the result of conscious choice. However, demographic surveys conducted in developing countries after World War II found that most people said that they did not deliberately limit family size. This implied that observed fertility behavior in these societies was what demographers call 'natural fertility' – most parents have as many children as they can, given the prevailing social customs and health conditions. Moreover, application of the Coale-Trussell technique to historical data suggested that natural fertility had also been the common condition in the historical experience of the developed countries prior to the demographic transition.

This implies that if you look at the broad sweep of history, and the world generally, the theory of household choice comes into play chiefly as countries go through the demographic transition. But *before* the demographic transition, the theory really can't tell you much about why fertility varies across countries or population groups, or why it changes over time.

DJM: Could we clarify that? Are you saying that before the demographic transition, we're at a corner solution – so we really can't predict anything using the model? But you're not saying that the *underlying theory* is necessarily wrong – it's just that you're in a constrained situation in which a change in a variable doesn't produce any predictable result?

RAE: I'm saying that the theory is seriously incomplete, and may be misleading. Even in cases where a variable seemingly produces a 'predicted' result, the theory may not correctly identify the underlying mechanisms. Let me give an example. An inverse association between duration of breastfeeding and fertility would be read by proponents of the theory of household choice as indicating that some households are deliberately restricting family size by extending the length of breastfeeding. By implication these households have a demand for family planning and would be likely to respond to a program intervention offering a less costly technique of fertility control. But if a natural fertility regime prevails, the observed association may be due simply to the fact that some mothers are physically unable to breastfeed their children as long as others, and, in consequence, become pregnant and bear children more often. In this regime, there is no *deliberate* reduction of family size via longer breastfeeding, and hence no demand for family planning. Put differently, it is conditions relating to the supply of, not the demand for, children that govern observed fertility patterns.

More generally, the theory of household choice is a demand theory, and is, in consequence, incomplete. The supply-demand framework that I developed with colleagues at Penn suggests that the determinants of fertility shift from supply to demand in the course of the demographic transition.

DJM: But even in developed countries, you hear people repeating that old wive's tale: that if you're breastfeeding, in the first three months or so, you don't have to worry about contraception. You're saying that in surveys of people in developing countries, you don't get that kind of response?

RAE: Well, when John Knodel put to focus groups in Thailand the question of how breastfeeding affected fertility, most women said that they did not see a link between the two. The reason was that often women became pregnant while breastfeeding; hence, the biological link wasn't apparent. That's stronger evidence than the anecdote to which you refer; the focus group approach is an attempt to be more systematic. But obviously we need much more data of this sort, unless you believe that self-testimony is irrelevant to modeling behavior.
DJM: Well, if that's the situation with regard to fertility, how does the household choice model hold up in terms of mortality?
RAE: If you look at the theoretical literature on mortality in developing countries, again it starts with the theory of household choice, so it's similar to fertility in that regard. The idea is that people make decisions knowingly about health and life style expenditures, and that largely determines their mortality. How true that is seems to me to be a matter of debate. If one's interest is in the period before the demographic transition occurs (that is, most of human history), the question is, do these models help you understand anything about mortality trends, mortality differentials, or mortality differences among countries? The answer seems to me to be no, for the very good reason that the prevailing state of household knowledge on the determinants of mortality was until fairly recently abysmal.
DJM: So you're not saying that people were *not* trying to reduce mortality? It's just that they didn't have the information to do it properly?
RAE: Yes, if you believed that a witch doctor would help you, sure you would use him. People acted according to the prevailing state of beliefs. But if one looks at what those beliefs actually *were* and the decisions based on them, they were likely to be ...
DJM: ... ineffective or even counter-productive.
RAE: Exactly. So, if there were fluctuations or differentials, they had much more to do with developments with regard to *exposure* to disease, rather than households' success in raising resistance to disease. And the exposure to disease occurred in ways that were often not understood. So there were epidemics ...
DJM: ... and they just didn't understand how they were being transmitted.
RAE: Right. For example, in nineteenth century England, there was debate about whether or not diseases were *contagious*. Anti-contagionists would point out that there were many cases of personal contact between non-diseased and diseased persons that did not result in disease – and this was contrary to what the then-prevailing theory of contagion asserted. It wasn't until scientists found out that the vectors of disease were not always persons, but could be mosquitoes, water, or the like, that the theory of contagion became generally accepted, and that was not until the end of the nineteenth century. So there were all kinds of conflicting ideas, ranging from demonic theories to the theory of contagion, including the miasmatic theory, which said disease had to do with ...
DJM: ... vapors.
RAE: Yes, with vapors in the air, and putrefying matter. Another common argument was that there are hot and cold imbalances in the body: you get diseased because there's too much hot in the body, and you've got to counteract it by having cold things, or vice versa. These notions are indicative

of the state of household knowledge before the demographic transition, and individual choice in such circumstances is as likely to be detrimental to health as conducive. The great decline in mortality starting in the late nineteenth century that has occurred throughout the world is due chiefly to the breakthrough in knowledge stemming from the work of persons like Snow, Pasteur, Koch, and Lister, and governmental interventions based on this knowledge, including attempts to improve the state of household knowledge.

DJM: Have other people talked about this issue at all?

RAE: Well, as far as knowledge is concerned, the medical anthropology literature is full of information about folk beliefs about the nature of disease: what the symptoms of diseases are, what the causes are, what the proper treatments are. John Caldwell has spurred a lot of recent work along these lines. Among demographers too there's attention paid to empirical concerns of this sort, and awareness by scholars that people may not know what to do to improve their health. The literature on the history of public health is also very helpful on these matters.

So with regard to both mortality and fertility, I feel that study of subjective evidence – what people say and think – is important. With regard to the debate about deliberate control of fertility, subjective evidence that people are not looking at breastfeeding, *or* at age at marriage as ways of limiting family size is important, because if you think they *are*, and you go in with a program for controlling fertility, you think you're offering them a better way of doing what they're already doing, that is, controlling fertility, and there should be a demand for these better techniques. But if they aren't even considering the family size implications of their breastfeeding and marriage decisions, then there's no demand, and the success of an intervention is much more problematical.

Similarly, with regard to mortality: if you go in with modern ideas that conflict with existing beliefs, you're going to have a much tougher time selling them, than if you fit them into those beliefs. One example of this is with regard to oral rehydration therapy (ORT). A major consequence of diarrhea is dehydration – you have to get fluids into the patient. A common treatment in the folk literature, both in the historic past and in contemporary conditions, is that you should use purgatives. So it's been suggested that, if you tell people the ORT solutions are purgatives, they're more likely to use them, than if you go in and say 'your kids should take this, because they are getting dehydrated'. The problem is that in most times and places, households don't understand the concept of dehydration. Thus, if you know something about the way people think, your policies may be more effective, *or* you'll find out why your policies are ineffective.

Many advocates of the theory of household choice refuse to look at subjective testimony. So they rule out survey findings on whether people are controlling their fertility, and they rule out information about the folk beliefs of the population. They construct theories which model the data, but may not correctly describe the underlying mechanisms as suggested by subjective testimony. Such theories may, in consequence, be poor guides to policy interventions.

Have you seen the article in the September *JEL* – a nice article by Shira Lewin (1996) – that has to do with psychology and economics and de-

scribes how economics got into this bind? Lewin goes back to Samuelson's theory of revealed preferences, and Friedman's argument about 'you don't care whether the assumptions are right.'
DJM: Yes: as longs as it predicts well.
RAE: Right. In Shira Lewin's view these two developments, in effect, cut economics off from all sorts of subjective testimony, and with adverse consequences. It's obviously a point of view with which I agree. If you pay attention to how people think, what they say, and what they know, you will be able to develop more effective models of behavior. The dogmatic rejection of subjective testimony by many economists creates an unnecessary limit on the channels through which we may gain insight into human behavior.

4. Early career development

DJM: You stated in an earlier interview that you didn't go to the University of Pennsylvania in order to work with Simon Kuznets: you went there initially to study for an MBA – how did you come to work so closely with him, then?
RAE: Largely as a result of reading most of his work in the areas of national income and economic growth. This reading impressed me with the scope, depth, and brilliance of Kuznets' mind, and gradually changed my view of the discipline of economics and the nature of economic research. I came to see economic theory as one tool for understanding empirical reality, and the object of research to be to obtain insight into reality.
DJM: In earlier biographies and interviews, you have characterized your early economics career as a period of disdain for demography and the other social sciences. You have said: "economists too often have little knowledge of the field of demography and little respect for non-economists. Early in my career, I was prone to the latter shortcoming ..." (Parkin 1993). Can you characterize your transition: was it gradual and deliberate or sudden and revelatory – and what was/were the most important influence(s) on that transition? Was it pure serendipity?
RAE: Giving up the 'tastes' implanted by graduate economics training was not easy, although Kuznets' tutelage provided valuable impetus. If I had to cite one circumstance that made for a broader view, it was the desire to gain insight into reality, and the growing realization that other disciplines could contribute to this. This realization stemmed largely from firsthand exposure to other disciplines. For example, the influence of close colleagues in the interdisciplinary program in demography at Penn, especially Dorothy S. Thomas and John Durand, was very important in broadening my education.
DJM: Are you aware of any contribution to your professional life from your initial training as an engineer? Do you think, for example, that it may have influenced your choice of economics over other social sciences?
RAE: Analytical reasoning in engineering is much like that in economics, so engineering training made the transition to economics easier, and probably enhanced the appeal of economics. Although I never capitalized directly on my engineering background, indirectly it gave me a better feel for

what was inside the economist's 'black box', that is, a better feel for the substantive nature of technology and natural science, and thereby of their direct relevance to understanding economic growth as well as the reduction in mortality.

DJM: You progressed from a new PhD (1953) to a full professor at U. Penn in just seven years (and during that time served as department chair for two years). Was such rapid progress typical of the times, or were there extenuating circumstances in your case?

RAE: I think the rapid rate of promotion in my early career was fairly typical of my cohort generally. I had confidence in the story told in my long swings book and *Birth and Fortune* partly because it was confirmed by my personal experience.

DJM: At the same time, you were a research fellow at NBER. How did that happen, and what were you working on there?

RAE: After the Kuznets-Thomas project at Penn on population redistribution ended, Kuznets arranged a research associateship for me at the NBER to finish up work on the project. Subsequently, I was invited to join the research staff to work on demographic aspects of the long swings project directed by Moe Abramovitz, and that led eventually to my book on long swings.

DJM: You received your PhD together with Charles Westoff in 1953, and then worked at the NBER when Gary Becker was there. Becker (1960), in his paper "An Economic Analysis of Fertility" added the following note: "I am indebted to Richard A. Easterlin and Eugenia Scandrett for helpful comments ...". Much of the work you have produced since then is often thought to be at odds with the work of these two men: what was your relationship back in the 1950s and early 1960s?

RAE: I think the differences have been exaggerated. Speaking personally, my work on fertility behavior has benefitted from both Gary's work in economic theory and Charlie's empirical work in demography. Gary Becker and I were contemporaries and good friends at the NBER in the early 'sixties' and frequently discussed our work. In one of my brief stints as chairman at Penn, I tried to hire Gary. Although our contacts have been quite limited since the NBER period, we remain good friends. I've always admired Gary and feel his Nobel Prize was well-deserved. He has certainly done much to advance the study of fertility behavior by economists, though I obviously have important differences from his views.

Charlie Westoff and I have always been on friendly terms, but have never really been in close contact. Although we graduated together, I had not yet gotten into demography, so there was little basis for discussion between us. Moreover, Charlie spent much of his time at the Office of Population Research in Princeton even before receiving his degree. But the survey work that he and Norm Ryder (Ryder and Westoff 1977) did at OPR on fertility and fertility control both in developed and developing countries educated me considerably, and provided a challenge for explanation.

5. General economics topics

DJM: Do you enjoy economics? Is there anything else you would rather be doing?
RAE: I enjoy the freedom of a scholar to study and try to understand the world about him. Economics is helpful, but not nearly as much as it would be if empirical relevance rather than theory were the criterion of accomplishment. Given free choice and plentiful resources, I'd probably try to get more educated in history and other social sciences.
DJM: How would you characterize your work generally: what are the important themes?
RAE: My work has chiefly focussed on the explanation of empirical problems that I found interesting – long swings in population and the economy, the American baby boom and bust, the demographic transition (both fertility and mortality), the spread of economic growth, and the relation of economic growth to subjective welfare. As for themes I believe that economic research calls for careful study of the facts, especially quantitative facts, which may themselves provide insight into causation. And I have come to realize that economic analysis needs to be complemented by the specialized theoretical and empirical knowledge developed in other social sciences.
DJM: What is the one book or paper you are proudest of: if you were to be remembered for only one, which would it be, and why?
RAE: Perhaps I'm unduly influenced by my recent concerns, but the answer is my new book, *Growth Triumphant* (Easterlin 1996). It incorporates a lot of what I've worked on over the years with regard to long term change, past and future. It does not, however, include the economic and demographic swings that I treated in other studies.
DJM: Are there other issues that intrigue you, unrelated either to the Easterlin Hypothesis or the fertility revolution?
RAE: Yes. As is clear from *Growth Triumphant* I've been more interested lately in the worldwide spread of two phenomena, the 'Mortality Revolution' and modern economic growth. I suspect that many generalizations regarding economic growth, such as the vital role of market forces, need to be re-examined in the light of the Mortality Revolution. More generally, I'm intrigued by the impact on human experience of the emergence and growth of modern science, of which the Mortality Revolution and modern economic growth are but two examples, though important ones.
DJM: What do you think should be the role of economists?
RAE: To understand and explain human experience in particular times and places.
DJM: Have you ever been wrong to the extent that you have reversed your position on an issue?
RAE: Sure, two instances come quickly to mind. For a substantial part of my career I assumed with most economists that the theory of household choice was sufficient to understand fertility behavior in all times and places. Similarly, I assumed that economic growth increases subjective well-being. I've reversed my view on both matters.
DJM: Do you have any regrets, professionally?

RAE: Only that I haven't had more time to read widely in the scholarly literature of social science and history, and to have contact with scholars in those areas.
DJM: Can you identify some living economists – or other social scientists – whose work you most admire?
RAE: Frankly, there are so many scholars from whose work I've benefitted – in economics, economic history, demography, sociology, and psychology – that to name a few would be invidious.
DJM: Several of your early pieces were published in the *AER*, while at the same time you were publishing in *Demography* – in fact, Chapters 2 and 5 of your *Long Swings* book (Easterlin 1968) were published in *Demography*, and 3 and 4 in the *AER*. Nowadays it's unlikely that these pieces would be accepted by *AER:* what happened – to you or to the profession?
RAE: I guess the biggest single factor today against economists publishing in demographic journals is that publications in inter- or extra-disciplinary journals tend to count for nothing when the discipline evaluates accomplishment. On the other hand, it seems to me that demographic journals today are much more receptive to articles couched in the jargon of economics, so this makes for greater publication by economists in non-economics journals.

I think that what are called the mainstream economics journals are much more insistent on adherence to a fairly narrow paradigm in economics – in the area where I work, household choice and utility maximization. I think the *AER* has gotten much narrower, and much more theoretical in its focus. Those articles that I published were essentially attempts to interpret empirical patterns – the baby boom, long swings in population and economic growth – and, though I interpret them in terms of economic relationships, still a lot of the emphasis is on the empirical. These days, I think you've got to start with a hypothesis ...
DJM: ... and the data are only there, almost incidentally.
RAE: Yes, to confirm the argument. So I think the editors of the mainstream journals conceive their purpose primarily as the development of economic theory, but economic theory narrowly defined. For example, the first article I wrote on happiness, which was published in the Abramovitz collection over two decades ago, and is still cited, was submitted to the *AER*, and was rejected outright. It seemed to me that whether the little bit of theorizing I did was right or not, the article provided a pretty clearcut case of empirical contradiction of established theory – which *ought* to contribute to the advancement of theory. But the reviewer simply said there's nothing new here. My feeling is that the reviewer's conclusion stemmed from his conviction that subjective testimony is meaningless, and, in consequence, he hardly even read the article. Being able to dismiss subjective testimony certainly makes theorizing easier, but not more relevant.
DJM: Very depressing! What criteria do you think should be used to judge economic analysis?
RAE: That's easy – whether it helps us to understand actual human experience, not 'stylized facts.'
DJM: In your opinion, what are the major problems with economics as a discipline today? Where do you see economics going in the next two decades? Where would you like to see it go? In this regard, you might like to consider the following two exerpts:

Robert Pollak and Susan Watkins (1993) recently wrote that “many economists still regard preference formation and change as the business of other disciplines”, and emphasize that they find “this intellectual division-of-labor argument unpersuasive.” Pollak and Watkins go on to state that “because estimation presupposes a correctly specified model, empirical estimators cannot ignore preference change. If an investigator assumes fixed preferences when in fact they are changing, then all of the coefficient estimates, even those of the narrowly specified economic variables, are inconsistent. Thus, if preference change is taking place, economists cannot ignore it unless they are prepared to abandon empirical analysis and reconstitute economics as a purely deductive enterprise.” (p. 491)

Robert Heilbroner and William Milberg (1995) argue in a recent book: “It is that politics and sociology – and beneath them, psychology in all its forms – do not possess the lawlike regularities of behavior that demarcate economics as a field of social analysis, investing it uniquely with the characteristics of a social ‘science’. Consequently, chains of reasoning play a relatively minor role in political, sociological, and psychological inquiry compared to that which they play in economics. In no way does this difference make economics prior to, or deeper than, its neighboring approaches, but it does endow it with the capability of developing causal sequences that are often their envy and despair. Analysis has thus become the jewel in the crown of economics. To this we have no objection. The problem is that analysis has gradually become the crown itself, overshadowing the baser material in which the jewel is set.” (p. 5)

They go on to state: “The challenge is the inescapable requirement that economics must come to regard itself as a discipline much more closely allied with the imprecise knowledge of political, psychological, and anthropological insights than with the precise scientific knowledge of the physical sciences. Indeed, the challenge may in fact require that economics come to recognize itself as a discipline that follows in the wake of sociology and politics rather than proudly leading the way for them.” (p. 126)

RAE: The main problem is the calm assurance that mainstream economic theory is sufficient to understand the world about us. As evidence, I cite the virtually unchanged nature of the core requirements in graduate economics training over the years. Sure there are, and always have been, a few heretics working along the lines suggested in your quotations. I see no evidence, however, that such work has altered the long term trend of the discipline. Hence, my projection for the future is more of the same.

Since graduate training is crucial to determining the future of the field, let me state my desires for future directions in those terms. First, I’d like to see serious attention paid to measurement, not just economic measures, but relevant measures in psychology and sociology, such as those relating to expectations, values, and subjective well-being, concepts that figure frequently in economic theorizing. Second, I’d like to see core theory less as a delivered body of knowledge with normative implications, such as the beneficence of the free market. Instead, I’d like to see theory approached as a framework for understanding reality, with attention paid *in the core* to a wider range of theory (such as Pollak’s work), and respect accorded the theoretical insights of other social sciences. Third, I’d like to see real empirical content in 80% of doctoral theses instead of 20%.

At the undergrad level, I want to distinguish between introductory level and other undergrad level courses. I feel that there's been a serious effort by some members of the economics profession to do a better job at the introductory level, where the emphasis is on conveying the relevance of economics to understanding what's going on in the world about us. But at the advanced undergrad level, I think you start getting an emphasis more like that in graduate school economics.

DJM: Yes, because they're trying to prepare them for graduate school economics.

RAE: Yes, that's the problem. Too often undergraduate programs are aimed at the professional training of economists, rather than teaching the subject so as further to develop its relevance to special areas. So, if there's a course, say, in industrial organization, it can be taught primarily in terms of the theory of industrial organization, which is almost devoid of empirical content. Alternatively, it can be taught in terms of the policy problems in the area, such as control of monopoly and regulation of markets, and what economic theory has to contribute to understanding these problems. My experience is that the trend has been toward less relevance at the undergraduate level. In fact the great majority of undergrad majors in economics do not go on into graduate economics, but go into business, law, international relations and other fields. These students need to know as much as possible about what economics has to say about the real world.

Indeed, even graduate economics students need such instruction. For example, I had a recent conversation with a Chinese graduate student, a teaching assistant in my introductory course. He was saying as we were walking back from class, "Oh I've really learned a lot! I take the graduate courses, and it's all mathematics – that's all well and good, but then I TA in this introductory course and I learn about what's really going on in the American economy". And it's true that that's how the graduate students chiefly *learn* something today about the application of economics – by TA-ing undergrad courses that have some applied content.

DJM: But in the amount of time you have for courses, in the graduate program, given the emphasis that they place on math, there just wouldn't be any time ...

RAE: The pressures are enormous, but I believe there is a basis for change. For example, virtually all programs currently require two years of theory, but the marginal productivity of the second year is pretty small compared to the alternatives.

DJM: Your work on money and happiness establishes pretty conclusively that economic development per se doesn't actually make people any happier. Should we be redirecting our energies – and if so, in what way?

RAE: That work is diagnostic; not prescriptive. I'd like to feel that it might raise in people's minds whether we should redirect our energies. It would be nice to have some discussion of this, but it doesn't seem to happen.

DJM: What would you say have been the most important issues of debate in economics during your career?

RAE: From my limited perspective, which reflects my specialized interests, an important issue has been that of the universal empirical relevance of the theory of household choice. Over the years, I have come increasingly to doubt that it is a good starting point for modeling demographic experience in most times and places. Whether this is an issue of debate, however, is uncertain – the economics paradigm these days seems to mandate the theory of household choice as a point of departure.

A second issue is the admissibility of subjective evidence of the type gathered by demographers, sociologists, and psychologists. Again, it is not clear whether this is an issue of debate among economists, although I have seen a few hopeful signs recently.

The final issue relates to the normative effects of free markets. I would have thought that the progress of knowledge amply demonstrated that the miseries of ill-health and serious unemployment were not due to failings of the individual, as was so long-believed, and that it was social action based on the growth of knowledge, not market forces, that ultimately corrected these conditions. But the economics literature these days – unless I misread it – seems dedicated to the simple proposition that government is evil and market forces benign.

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