As the children of immigrants, my parents were raised in Scandinavian Minnesota. My mother, Verna Ecklund, was a university student for only one year but my father, Thomas Peter Mortensen, graduated from the School of Forestry at the University of Minnesota in 1936. They were married shortly after and moved to Enterprise, Oregon, where I was born in 1939. Enterprise, located in the far north-eastern corner of the state, was in cattle ranching country surrounded by one of the most beautiful mountain ranges in the U.S. In these mountains, my father began his career as a lookout officer for the U.S. Forest Service. In the war years, they migrated further west to the Portland area where Dad help build Liberty ships in Mr. Kaiser's ship yards and Mom provided day care for the children of Rosy the Riveter. After the war, the family, which now included my brother Arne born in 1942, moved to the Hood River Valley 60 miles east of Portland where again my father returned to the practice of forestry. There my brothers and I, who included Irving born in 1947, were raised.

The valley of the Hood River runs north-south between the majestic volcanic peak of Mt. Hood and the stunning Columbia River Gorge. Nestled as it is in the beauty of the Cascade Mountains, it divides the wet western part of the state of Oregon from the dry highlands of the eastern two thirds. As such it contains all of the commercial forest species of the Northwest on the slopes of the mountains: the Douglas fir, the hemlock, the true firs such as noble and white on the west and several pines, including white and yellow, on the eastern slopes. The valley itself is a garden of fruit trees that include bartlett, anjou, and bosc pears as well a variety of apples and cherries. Since I grew up there, the valley has also developed many vineyards and the town of Hood River on the Columbia has become a major destination for wind surfers.

My parents, as one of a few with any advanced education among the fruit growers and loggers of the valley, entertained the intellectuals of their community, their colleagues of like mind, the local school teachers, and a few eccentrics. I grew up listening to my father argue politics into the night and taking trips every Saturday to the Hood River library where my mother maintained her interest in reading and encouraged the same from her sons. This library was one of those small town institutions that benefited from the largess of Andrew Carnegie, the founder of U.S. Steel and the great "robber baron" of Pittsburgh. In the days of my childhood in the 40s and 50s, long before the internet, it was these institutions and the occasional inspirational teacher that provided windows to the world of ideas to those living in the small towns and rural countryside of America. I benefited greatly from having been exposed to excellent examples of both sources of enlightenment.

I was a good student with mathematical ability and interests. As such, I took the usual college preparatory program in high school for one looking to become an engineer, all the available courses in mathematics and science. In my last two years of high school and all through college, I also used these skills in summers as an assistant to my father who managed the forest holding of a local timber company, a job that included assisting in surveying the company's land, cruising its timber, and designing the roads used in the logging operations. I was also "well rounded" in the sense of that cliché in the 1950s. I had participated in boy scouts and 4H while in elementary school, had a bass baritone voice which I used in school and church choirs until 15 years ago, dabbled with acting and musical theater in both high school and college, and lettered three years as a member of the varsity football team of Wy'east High School, the new combined high school that served the valley students. I was even named all-league in my senior year high school, an achievement that I attribute to my cleverness more than my athletic ability.

In my last year at Wy'east, I became interested in American social history of the late 19th and early 20th centuries and the related literature, much of which concerned the industrialization of America. My grandfathers had been immigrants in the period; one had done reasonably well but the other lost his wife to tuberculosis in the twenties and his farm to the Depression of the thirties. From Sinclair Lewis, Hemingway, Faulkner and Upton Sinclair as well as the stories that my father's friends told about their experiences, I expanded my knowledge of the human condition during this period. From the progressive historians of the early twentieth century, I formed my own picture of the industrialization that had transformed the country at that time and developed an understanding of how it had affected the lives of my elders for both better and worse. By the end of my senior year in high school, I faced a dilemma… how could I combine my interest in analytical problem solving with a concern for social issues?

A high school friend, Rusty Beaton, who had graduated the year before my last year, told me about his economics professor at Willamette University, Richard Gillis, who had inspired him in his first year at university. By chance, I had also found at the Hood River Library *The Theory of Games and Economic Behavior* by von Neumann and Morgenstern, the seminal text on what was to become game theory as well as a major application of mathematics to social analysis. I realized that economics was a possible way to combine my two principal interests. The next year I enrolled in Willamette as the beneficiary of a full tuition scholarship, majored in economics and mathematics, and ended up as one of two of the senior assistants to Professor Richard Gillis in my senior year. After benefiting from the solid liberal arts tradition of the oldest university west of the Mississippi, I went on to do graduate work at what was then the Carnegie Institute of Technology. In addition to the fact that I was offered a more generous fellowship than either Stanford or Harvard, it was an innovative program that emphasized an analytic as well as interdisciplinary approach to the study of economics and management. I must admit, I was also attracted by the fact that it was located in Pittsburgh, which had played central role in the industrialization of America.

As it turned out, Pittsburgh was already on it way to becoming the beauti-

ful city that it is today when I arrived in 1961, not the smoky and dirty place it was still reputed to be. Although Herbert Simon had turned his attention to the application of the computer to artificial intelligence by the time I had arrived in 1961, he still taught those of us in the Ph.D. program how to think about social phenomena and how to represent those thoughts in a mathematical model. We were also introduced to the wonders (and frustrations in these days of punch cards and slow turnaround) of the digital computer and taught how to apply it to real computational problems that arose in management using the recently developed methods of linear, non-linear, and dynamic programming. We were instructed in the new behavioral approach to the theory of organizations in general by Jim March and Herb Simon and the application of that theory to the analysis of the firm in particular by Richard Cyert and Jim March. Although these ideas interested me, eventually I turned to more traditional economics and began a thesis in growth theory directed by Michael Lovell, who by chance happened to be the son of my favorite history professor at Willamette. Along the way, I also found the love of my life, Beverly Patton. We were married in 1963 and soon became the proud parents of three children, Karl, Lia and Julie, between the time I started and completed my thesis.

Carnegie Tech was an amazing place at the time. New ideas of all kinds were in the air. They were not always consistent with each other, as in the case of the conflict between John Muth's suggestion about how to model expectations as "rational" and Simon's notion of "bounded rationality." We students benefited from the lively debates among the faculty. We were also encouraged by the faculty to get involved in the research process even before we had mastered the details of the literature. In my last two years, I took courses from Robert Lucas and Oliver Williamson with fellow student Ed Prescott, all of whom are now Laureates. In retrospect, it was obviously a very special educational experience.

I became a member of the faculty at Northwestern University in 1965 but did not complete my thesis until two years later at a graduate ceremony at which Carnegie Institute of Technology became Carnegie-Mellon University. At Northwestern I was mentored by the "three Bobs," Robert Eisner, Robert Strotz and Robert Clower. In my first couple of years, I finally learned the foundations of economic analysis by teaching Hicks and Samuelson to first year Ph.D. students. Those first three years were a struggle trying to develop teaching skills, finish a thesis that was taking too long, and helping care for three babies. Without the patience and perseverance of a loving wife, I would not have succeeded.

My interests in economics changed in this new environment. Two new and important colleagues arrived, Frank Brechling and Art Treadway. Frank's research was focused on the dynamics of the labor market and Art, a student from the University of Chicago, was developing costs of adjustment models and applying them to investment theory. Together with colleague Bernt Stigum who was doing work on both time series analysis and dynamic general equilibrium, we taught ourselves dynamic control theory from the pages of

Pontriagin's book. Finally, I was fully proficient in control theory as well as dynamic programming, the tools that dominate macroeconomic theory to this day.

In the mid 1960s a debate raged over the Phillips curve and its implications for economic policy. Some interpreted the curve, a negative statistical association between the inflation rate and the unemployment rate, as an unpleasant trade-off that policy makers had to face. According to this view, one could use monetary policy to lower unemployment but only at the expense of a higher permanent rate of inflation. However, the theory behind the curve was suspect. Milton Friedman in his 1968 AEA Presidential Address declared that the economy tends toward a 'natural' rate of unemployment determined in market equilibrium which is invariant to the inflation rate, at least in the long run. Monetary policy can reduce the unemployment rate over the relative short run by lowering real wages. However, any attempt to maintain the level below its natural or equilibrium rate though monetary policy will be frustrated by ever increasing inflation as workers and employers realize what is happening.

Edmund Phelps (1968) argued along similar lines. Moreover, he complemented his assertion with a new view of how the labor market works. Specifically, he suggested that some positive unemployment level was a natural outcome of the matching process in the labor market. Any attempt to lower unemployment below that determined by the rational agent behavior of individual employers and workers by inflating the economy would simply induce adjustment back to its natural level at a higher rate of inflation. Underlying the arguments of both Friedman and Phelps was the long standing proposition that there is no money illusion. Hence, only real wages matter, at least in the "long run." Any attempt to artificially manipulate the price level through monetary policy would induce agents in the labor market to agree to offset the effect by revising the nominal wage.

At Northwestern we created an informal reading group focused on the macro economic implications of the new ideas about the labor market circulating in the profession. Included in the group were my colleagues Frank Brechling and Art Treadway as well as a visitor to the department, the British economist Chris Archibald. Both Frank and Chris had known Phillips at the London School of Economics, knew his work well, and had done research that was stimulated by it. With their help, I began to think about how to capture the essential features of a decentralized market with search friction in a formal but simple economic model. Using the labor market as my focus, I came up with the idea of modeling the consequence of search and matching friction as the outcome of a sequence of random meetings between potentially interested parties.

After I had written a very long working paper designed to formulate and work out some of these ideas, Edmund Phelps came to Northwestern to give a seminar in the fall of 1968. During his stay, he informed me that he had read my paper with considerable interest. Furthermore, my ideas were closely related to those in a working paper of his co-authored with Sid Winter as well as several other working papers by others that tackled the issues that arose in decentralized exchange among rational agents that possessed only imperfect information about trading opportunities. He then revealed that he was planning a conference on the topic at the

University of Pennsylvania, his academic home at the time, in which these papers would be presented and discussed. I don't recall whether he invited me to participate in the conference on the spot or whether that invitation came later. In any case, I was fortunate to have my first major paper, "A Theory of Wage and Employment Dynamics" published in the collection of papers presented at his conference that became universally known as the "Phelps volume" .

The Phelps volume published in 1970 bore a rather pretentious title: *Microeconomic Foundations of Employment and Inflation Theory*. It became a classic anyway. The basic message of the collection was that one could and should consider the dual macroeconomic problems of employment and inflation as the outcomes of market behavior of individual agents who act in their own interests as best they can in a market environment characterized by uncertainties and incomplete information. As a corollary, the papers argued that macroeconomics should be founded on microeconomic principles. Included among the authors who published in the volume was Robert Lucas. Although the three of us, now all Nobel laureates, have not always agreed on the details and taken different direction in the pursuit of the goal, we have shared a common view that macroeconomics needs a foundation in equilibrium market analysis based on the principle that agents in such markets act in their own self interest. In the intervening forty years this view has come to dominate macro-economics.

My companion paper, "Job Search, the Duration of Unemployment and the Phillips Curve," published in the *American Economic Review* in the same year, was an attempt to use my ideas about decentralized exchange in the labor market to provide an interpretation of the Phillips curve. I refer to it here, not because I accomplished that goal. In fact, I regard the paper as a failure in that dimension for several reasons. One of these was the inability to close the model with a convincing theory of agent expectations. Arguably this could have been done by invoking "rational expectations," a concept that had already been introduced by one of my then Carnegie Tech professors, John Muth (1961). Unfortunately, I did not see its relevance for my work at the time even though I had participated in a seminar on the subject briefly as a student. The issue was subsequently resolved by later adopting this approach in spite of its drawbacks.

The actual contribution of the AER paper, along with those of John McCall (1970), published at about the same time, was the formal model of sequential wage search that it embodied. In the model, an unemployed worker samples sequentially from a known distribution of wage offers until one is found that exceeded an optimally chosen reservation wage. The optimal reservation wage is simply that which compensated the worker for forgoing the option of continuing to search for an even better wage. Although this formulation was simply an application of the well-known optimal stopping problem in statistical decision theory, it was a new dynamic approach to the understanding of unemployment that incorporated the need of workers to gather information. Since this need suggested that the time spent searching was productive, the model offered an entirely new view of unemployment.

At the time, Keynesian thought dominated the profession's view of unemployment. Viewed though the lens of classical supply and demand analysis,

Keynesians argued that unemployment arose because the real wage was too high. Unemployment in this view was totally involuntary; unemployed workers were simply those that could not find a job at the prevailing wage. The idea that workers might rationally choose to be unemployed was beyond their imagination. Indeed, when I presented a working paper version of what became my AER article at the annual meeting of the American Economic Association, some who attended walked out in protest. Nevertheless, my colleagues at Northwestern chose to promote me to Associate Professor with tenure in the fall of 1970 with only two forthcoming papers to my credit.

My family and I spent the academic year 1970–71 on sabbatical visiting at the University of Essex in England with my colleague Frank Brechling. At the time Frank and I were working together on a project designed to formulate and estimate a model of employment dynamics based on the idea that the process of recruiting and hiring workers was a time and resource consuming process. As part of that project, I wrote a draft of my theory paper "Generalized Costs of Adjustment and Dynamic Factor Demand Theory" (Mortensen, 1973) which provided a mathematical foundation for investment in an arbitrary number of capital goods couched in terms of mathematical control theory.

Several of the Essex faculty had visited Northwestern previously, so we had a congenial group of colleagues that also included John Kennan, a Ph.D. student at the time, who served as our research assistant. Christopher Pissarides was a student at Essex finishing his undergraduate course that year. He tells me we discussed his plan to pursue search theory as a graduate student at the London School of Economics although I don't recall the meeting. During the year I was invited to Oxford, London School of Economics, and Durham University to present my current work on search in the labor market. These were opportunities to meet English economists, opportunities that were rare in the expensive early days of the jet age.

The 1970s was the decade of developments in the new area of information economics. Search theory, which emphasized the need to gather information, was joined by models that featured asymmetric information, the case in which information differed across individual agents. Signaling, screening, moral hazard and adverse selection, terms never mentioned when I was a graduate student, became the new vocabulary of the analysis of market performance during these years. Search theory, particularly as it related to the phenomena of unemployment, continues to develop as well.

The debate over whether unemployment was "voluntary" or "involuntary," "natural" or not, reflected "disequilibrium" or was an "equilibrium" phenomenon continued. Although thankfully this vocabulary has disappeared for the most part, the basic issue has not. Are workers unemployed because the real wage fails to clear the labor market or does unemployment reflect the trade-off between the value of time spent searching and its value in production given the search frictions identified in the new approach to unemployment and inflation expressed in the Phelps volume? James Tobin (1972) in his AEA presidential address, "Inflation and

Unemployment," was perhaps the first to ask the question in precisely this form.

Tobin was particularly critical of the prevailing job search model which assumed, as many still do, that only unemployed workers search and that workers might quit to unemployment in order to find better jobs. He cited sketchy evidence for the hypothesis that large numbers of workers move from one job to another without an intervening period of unemployment, a hypothesis which has since been verified. We now know that about half of hires in the U.S. and an even larger fraction in other countries consist of workers who already have jobs. To the extent that search-on-the job was feasible, productive search unemployment as an alternative explanation for unemployment was questionable.

Stimulated by Tobin's critique, my student Ken Burdett developed the first formal model of search on-the-job in his Ph.D. thesis, which was published as Burdett (1978). Although the analysis clearly illustrated the fact that the worker criteria for an acceptable job are less stringent when there when the option to search while employed is available, the idea that the unemployed were engaged in job search, as standard measures require, was fully captured by the theory. Of course, this fact did not rule out the possibility that the real wage may be too high as well, a point, which seemed to escape some Keynsian critics of the new theory.

Challenges to the idea that workers search because wages differ across employment opportunities came from the other contributors to the theory. Peter Diamond (1971), in his attempt to understand how prices might be set in a world of search friction, found that wage-setting employers would offer a single wage when workers located employment opportunities sequentially. Surprisingly, that wage was the monopsony wage, one which lies below the value of a worker's marginal product. This result led Rothschild (1973) to ask, "What is the source of wage dispersion that is supposed to motivate search unemployment?" Later Burdett and Judd (1983) answered the question with a paper that provided that foundation for the modern theory of wage dispersion.

Partly in response to these developments, the theory of unemployment shifted its emphasis from the problem of a worker seeking a high paying job to the formation of good job-worker matches. The matching problem, whether found in the labor, housing, or the marriage market, is one of forming complementary pairs in a world in which individual workers and jobs are heterogenous. It takes time and resources to accomplish this task, and the duration of unemployment experienced by individual workers as well as the length of time that an existing job is vacant reflect this fact. Empirical labor economists found this idea persuasive and used the models developed in the 70s, such as that presented in Mortensen (1976) and Burdett and Mortensen (1978) together with the statistical tools of duration analysis to interpret both unemployment and job spell data.

In 1980, Ken Burdett and I taught an intensive two-week short course on search and matching theory in Oslo. Our students were young Ph.D.

candidates from all of the Scandinavian countries. At the time, Ken and I together with George Neumann and Nicholas Kiefer were estimating three state models of worker flows using longitudinal panel date generated as part of the Seattle/Denver income maintenance experiment (Burdett *et al*, 1984). One of our students, Lars Muus, informed us that a group of economists at Aarhus University were creating employment and earning history spell data from Danish administrative records. These data had the advantage that they were collected continuously, covered the entire population, and could be linked to detailed information about each individual's education, age, and family demographics. In August 1982 all four of us were invited to attend the first conference featuring these data held at Sandbjerg Manor, a convention center in the south of Jutland, owned by Aarhus University and built around an 18th century manor house. My visit to Denmark with my wife and father, who had been born in Jutland and emigrated to the U.S. at age 10, initiated a fruitful association with Aarhus University as well as a rewarding interaction with my Danish cousins.

In the early 80s, Peter Diamond (1982a, 1982b) and I (1982a, 1982b) were both creating models of two sided search and matching equilibrium. These papers incorporated the concept of a matching function and assumed that wages were determined through bilateral bargaining. They were two sided in the sense that agents on both sides of the market made search investments in the process of creating a match.

The matching function is a postulated relationship between search and recruiting effort and the aggregate rate at which workers and job meet. Once a particular pair meets and finds that the match offered a surplus relative to continued search by both, it is formed. The division of the surplus, which determined the expected future wages and profits that employer and employee enjoy once matched, is regarded as the outcome of a bilateral bargain. Shortly after the appearance of these papers, Christopher Pissarides (1985) completed what became known at the Diamond-Mortensen-Pissarides or DMP model of the labor market by adding a job creation condition which determines the number of vacant jobs as that which equates the cost of creating a job to the expected present value of profit from an acceptable match.

The DMP model is an equilibrium theory of unemployment determination. It has important implications for unemployment dynamics and for the effects of government policies on labor market outcomes. For example, the model implies that procyclical movements in vacancies will induce counter-cyclical movements in unemployment as Beveridge (1944) documented for the U.K. in the early 20th century and has been established for every other developed economy with data on job openings. However, it also provides insights into the effects of unemployment insurance and taxes as well as active labor market policies. As more generous unemployment insurance adds to the option value of continued search as an unemployed worker, one expects higher wages and longer unemployment spells in countries with high benefits and long unemployment benefit periods. The theory also implies that job search training will lower the time required to find acceptable

employment. Generally empirical studies have confirmed these predictions as well as the existence of a relatively stable matching function.

In the original version of the DMP model, job separations were regarded as exogenous. The research of Davis and Haltiwanger demonstrated that layoffs were an important determinant of movements in unemployment (Davis *et al.,* 1998). In a review of the first edition of Pissarides' book, Equilibrium Unemployment, published in 1990, I suggested that this fact should be incorporated into the model. Later, Chris and I collaborated on the task in Mortensen and Pissarides (1994) and Chris incorporated the extended model into the second edition of his book in 2000.

The extended version of the DMP model supposes that the productivity of a job is subject to an idiosyncratic shock process as well as an aggregate shock. In this setting, a job is destroyed when its productivity falls below the value to both the employer and employee of seeking an alternative match. This formulation explained the basic asymmetry which characterized the typical effects of a business cycle. Namely, layoffs which occur early in the downturn are typically large and take place over a relatively short period of time, while recovery in employment once started is spread over a much longer period of time.

In a subsequent series of co-authored papers, Chris and I also investigated the implications of the model for a wide variety of government policies including employment protection. We found that our model did a good job of capturing the principal effects of firing restrictions, a reduction in both the typical flow of workers from employment to unemployment and from unemployment to employment, that characterize economies with strong employment protection. According to the model, economies with labor market inflexibilities of this kind have problems adjusting to the changes in technology experienced in the last thirty years. This argument has induced reforms in a number of countries, which take the form of allowing fixed contracts with no layoff penalty. Most of these findings are summarized in Mortensen and Pissarides (1998a, 1998b).

In another line of research, Ken Burdett and I pursued the implications of Diamond's original model of dynamic monopoly in the late 80s and 90s. As noted earlier, Diamond's analysis when applied in a labor market context implied that wages are set so that employers capture the entire surplus of a match if employers have the power to set the wages of their employees and workers search for jobs sequentially. Although the assumption that employers can post wages seemed consistent with how wages are determined in many labor markets, Diamond's conclusion that all employers set the same wage in an environment where many employers compete seemed inconsistent with empirical observation.

Ken Burdett, together with his then student Ken Judd, generalized the Diamond result by showing that equilibria exist characterized by price dispersion, different prices offered for the same good, if some buyers have the option of choosing between two offers while others must accept or reject offers sequentially. Shortly after the publication of their paper, Burdett and

Judd (1983), Burdett invited me to spend a quarter visiting at Cornell, his employer at the time. One day, I pointed out to Ken that conditions that he and Judd had derived for the existence of price dispersion were automatically satisfied in a labor market model in which unemployed workers search sequentially but employed workers also search and can choose between continued employment and any alternative wage offer generated. In 1990, we both published papers that worked out implications of this idea and submitted a jointly authored paper that provided the basic logic of our model to a number of top rated journals. Apparently referees and editors were not ready for our idea. After a series of rejections, each following a long review period, our paper entitled "Wage Differentials, Employer Size and Unemployment" finally appeared in the *International Economic Review* in 1998. By this publication date, the paper was already well known and had become the theoretical foundation for a new empirical literature on wage dispersion, which I reviewed in my Zeuthen lectures published in 2003 under the title "Wage Dispersion: Why Are Similar Worker Paid Differently?" The paper remains the classic reference in the new labor literature on monopsony. (See Ashenfelter *et al.,* 2010 for a review of recent work.)

I was again attracted in 1998 to Aarhus, where I spent 6 months at the Center for Labor Studies associated with Aarhus University learning about a new data source. My friends and colleagues there had integrated their longitudinal data on individual labor market histories with data that included firm identifiers and some information about firm characteristics. Later these data were augmented with accounting information from income statements and balance sheets. Along with the well-known French data set, it has become a major source of information about job-worker matches. Although there is now a similar U.S. data set created by combining business census data on firms with employment spell histories of worker qualified for unemployment insurance, the Danish data dominate the other two in the sense that it includes more detailed information about individual workers.

Detailed longitudinal data about firms had recently been exploited for labor market studies by Davis and Haltiwanger for their purpose of creating so-called job-creation and job-destruction time series, work, which is summarized in their book with Scott Schuh, *Job Creation and Destruction*, published in 1998. Their evidence clearly reflected the heterogeneity of firm growth rates in employment at the firm level. Subsequent research based on these data illustrated several interesting patterns relating firm employment, wages, and productivity. Namely, larger firms pay more and are more productive. Trade theorists added the fact those firms that engage in international trade are larger, more productive, and pay wage premiums. To my colleagues and me in both Denmark and the U.S., these facts suggested that workers and other resources should be moving from smaller less productive and lower paying firms to larger more productive firm and that this reallocation process might be an important source of economic growth as well as an explanation for why wages differed so much across firms.

In Lentz and Mortensen (2008), Rasmus Lentz and I developed and estimated a model in which productivity differences are the outcome of differences in the ability of firms to develop new products. These differences induce differential firm growth, which can explain the positive association found between firm productivity, average wage paid, and size in the data. Furthermore, an empirical version of the model estimated using the Danish data implies that over 50 percent of the growth rate in the value of goods and services of the private sector over the sample period studied can be attributed to this reallocation process. In our original model, the friction that sustains differences in productivity is a combination of the fact that every product is eventually displaced and that growth is costly. Although we abstracted from search and matching friction we are now working on estimating a model which also includes this possibility.

Obviously, the focus of this narrative is on my research and the development of some of its principal themes over the years. At the same time I was leading a life that was enriched by teaching and family relationships. I taught both undergraduates and graduate students while still at Carnegie Tech in the early 1960s. Although most of my teaching took place at Northwestern University, my home base since leaving graduate school in 1965, I have also taught at Essex University, Cornell University, the California Institute of Technology, New York University, and more recently at Aarhus University during visits of various length. Indeed, during the last five years I have split my academic year equally between Northwestern University as the Ida C. Cook Professor of Economics and Aarhus University as the Niels Bohr Visiting Professor of Economics.

With undergraduate class sizes that varied from 30 to 300, simple calculations suggest that there was a large number of students that listened to my voice if not to my wisdom. I have been fortunate to have excellent students and I hope that my influence on them has been for the good. Some of my most satisfying undergraduate teaching was in the program of Mathematical Methods for Social Sciences, an honors curriculum at Northwestern designed to train undergraduates in the analytic techniques and models in the social sciences that are usually taught only to graduate students. I also had the privilege of directing that program at two different intervals of time. The program continues to thrive as one of the most popular available at Northwestern.

My many Ph.D. students have been at least as important in disseminating my research ideas and results as my published papers. Many of them are professors themselves, some at top universities, but a number are also contributing their skill within the Federal Reserve Bank system and agencies of the federal government such as the Bureau of Labor Statistics. These individuals are not only former students but continue to be supportive friends that I value highly, along with colleagues at both Northwestern and Aarhus, as members of my close academic family. Finally, the continued support for my work by Northwestern University must be acknowledged. During my 45 years in the institution it has progressed from what was a

party school that catered to the children of the upper income professional classes of middle America to a first rate international center of learning and research. Although I may be the first Economics Prize Laureate on the faculty, I expect fellow company soon.

Let me finish with a brief tribute to my life partner. 'Behind every great man is a great women' is an old cliché that fits my case well, as any of our friends and colleagues will attest. As I mentioned in the beginning, Beverly and I started our lives together while I was still a student at Carnegie in 1963. Four years later, we had three children and my Ph.D. degree. That period established the relationship.

But as an undergraduate, Beverly trained as a musician and teacher, a profession that she practiced for the first year of our marriage. After five years of diapers, she ventured out into the world as a guitar player and folk singer. Later she directed the choir for 25 years at our local Catholic church and composed numerous hymns, psalms, and two oratorios for her favorite instrument, the choir. She then used her composing talents to collaborate with others in the writing several original musicals, all of which were performed by either high school or community groups. She also conducted a community chorus for many years that performed both pop music concerts and madrigal extravaganzas. Finally, as the three children were finishing their college studies, Beverly returned to school herself to complete a Ph.D. in Religious Studies. Since 1994 she has taught at Northwestern and was named last year by her students to the faculty honor roll. During these same years, she and I have managed to raise three children who have now provided us with eight grandchildren. The cousins, who range in age from 9 to 17, form a cohesive group that has enhanced the lives of their elders without measure. We see in them a bright future for our family.

During all this activity over these many years Beverly has been and continues to be my strongest supporter and best friend. My colleagues have long noted that my research on search and matching deals with the formation and dissolution of relationships. How is it, they ask, that you have been had the same job for 45 years and the same wife for 47? My response is simple: Both are excellent matches!

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