Finn E. Kydland

The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 2004

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Autobiography

In the winter of 1968, Sten Thore, who was then a professor of economics at the Norwegian School of Economics and Business Administration (abbreviated *NHH* in Norwegian), where I was finishing my three undergraduate years, made me an offer that would change dramatically the path I was to take for the rest of my life. I had gone to the business school with the expectation that I would eventually become a business manager. But when Sten asked if I would like to be his research assistant (vitenskapelig assistent), I agreed without thinking about what would turn out to be one of the most crucial decisions of my life.

It is interesting that I would be employed by the Department of Economics (to which Sten belonged as a faculty member), as I had not shown any more interest in the economics classes than in business. Admittedly, as business schools go, the curriculum at NHH included a substantial focus on economics. The department housed several economists who were highly visible internationally.

During my studies, I had made a couple of wise (or perhaps just lucky!) decisions. The curriculum did not permit any flexibility, except in two ways. One was to choose two elective areas of concentration, which students were to pursue by taking one course for each area per semester during the first two years. These elective tracks could be selected among four foreign languages, economic geography, economic history, law, and mathematics. I chose mathematics as one of the two (German being the other). I even took two math courses beyond the four-course sequence required for the elective.

Another source of flexibility was that the curriculum called for three relatively advanced courses, to be selected from an extensive list. My second wise decision was to take, as one of the three, the one offered by Sten Thore. In this course, we read from Howard's book on *Dynamic Programming and Markov Processes* and several rather mathematical articles from journals such as *Operations Research* and *Management Science*. I wrote my first computer program (in FORTRAN) doing dynamic programming, a tool I've used repeatedly ever since. After I had finished the course, Sten recommended to me a summer job at the local shipbuilding company to work on a computer program designed to determine a reasonable ship size for any particular route, given the available data on tonnage to be shipped and the per-unit time it took to load and unload it. Mathematically, it was an application of fractional programming - linear constraints and an objective function consisting of the ratio of two linear expressions.

As I said, Sten encouraged me to become a research assistant as I was approaching the end of my studies rather than take a job in industry. (He saved me from a boring life!) But then, after I had worked a few months as his research assistant, Sten informed me that he would be going on leave for a year to Carnegie-Mellon University, starting in January, and would I like to do my research-assistant duties there, and I agreed again. I postponed that move, however, until the summer, as Liv Kjellevold and I had married in August 1968, and Liv was finishing her last year as a nursing student. The second half of 1969 was when my fate was sealed - I would be an academic, and economics would be my field.

The early years

There had been virtually no indication earlier in my life that such an outcome might even be a possibility. I grew up in Søyland (although born on the farm of my mother's parents in the neighboring township of Bjerkreim), a small area of the township Gjesdal, about 40 km south of Stavanger. There were seven farms and us. One of the neighboring farms was my grandfather's. My father, Martin, was the eldest son and therefore in line to take over. He decided, however, to buy a truck - the first in the area to do so. He would base his living largely on a milk route between Søyland and the nearest diary, in Ålgård, 15 km away, and also on hauling other goods (and, in the spring, sheep to better pastures, returning them in the autumn) for the farmers. Eventually, he expanded to two trucks. My mother, Johanna, worked at home until all the kids (six of us, of whom I'm the eldest) were grown. I've been told that my father did well in school, although neither of my parents tried to influence their children in terms of career paths. In fact, it came as a surprise to both when I ended up as an academic.

The elementary school, in which all the pupils, except for my siblings and me, were farmers' kids, did seven years divided in three classes. We met twice a week in the first three years and three times a week during the remaining ones. The education wasn't especially "active." I was the only one in my class to go beyond elementary school. As a 15-year old, I went off to Bryne to attend Rogaland offentlege landsgymnas, the nearest high school. The need to rent a room was obvious, and probably half the pupils did so, as the distance to home made it infeasible for any of us otherwise to get to school in the morning in time for classes. Most pupils came from rural areas, as the cities typically had at least one high school.

This particular high school required an entrance exam, which I passed easily. As with all high schools in those days, one had to choose a concentration. This high school offered two, one emphasizing math and physics, and one placing a greater weight on foreign languages (although we all had English, French, and German for at least three years). Some high schools (but not this one) also had a concentration in topics oriented towards business and economics.

The education was exceptional. I've sometimes claimed that I knew more math at the end of high school than a typical American business or economics major, even at a university as highly ranked as Carnegie Mellon, know at the end of college. (As a consequence of my personal experience through my first 12 years of schooling, my bias has been to pooh-pooh the need for intensive education in elementary school, believing it is better to allow the pupils more time for play while they're kids, and instead to emphasize the importance of great high-school education.) Whether because the student body was more select than at most other high schools or because the teaching was first rate (or both), this high school always ranked highly in terms of number of *preseterister* - those students who had the grade of *very good* or *excellent* (the latter almost impossible to achieve) in absolutely all subjects. Indeed, to achieve the status of *preseterist* was regarded as important enough all over Norway to be worthy of photos in the local newspaper. In my case, I missed that distinction because of my grade in one subject: Norwegian composition. But my point score was still high enough to offer me the choice of just about all the university majors (the only exception being, I thought at the time, theoretical physics).

My initial inclination was to apply to the university for engineering studies, not out of a deep interest in engineering, but more for the simple reason that I had had an easy time with math and thought that's where that skill would be rewarded handily. Still, I had a nagging doubt about that decision. So to give me extra time to think about it, I applied for a one-year teaching position at the elementary school in Oltedal, the second-largest town in Gjesdal, and got it. In those days, the shortage of elementary-school teachers meant that such temporary hires straight out of high school were not uncommon. Thus, I spent a year teaching fifth and sixth graders in all subjects on Mondays through Fridays and second and third graders in Norwegian on Saturdays.

The year at Oltedal elementary school turned out to be important for my future. One of the other three teachers I'd see during lunch and other breaks was Harald Aarrestad who taught a junior-high-school class in the same school building. This program was nonaccredited, meaning that the students, at the end of the two years, would have to take their final exams in all fields at an accredited junior high school in a different town or city, but while they were in school, they could live at home. Aarrestad had taken the initiative to start this program and taught all the subjects. Further evidence of his energy and imagination was that he had started and was running two small businesses. I found him to be an extremely interesting person. Because his accountant was making lots of mistakes, he encouraged me to take a correspondence course in accounting and then promptly hired me as his accountant, a job I could easily do in my spare time. This experience gave me insight also in what it meant to run a business, a subject about which I had hitherto known nothing. Business hadn't been among the fields I had even considered for study. But by the end of the year, I decided to apply to NHH.

I had thought that, with my high-school grades, theoretical physics was the only field unavailable to me. I was wrong - NHH rejected me! Business education in Norway was a relatively young field - NHH had been started in 1936. The class size was only 60 students, and these students came from all over Norway. I learnt later that NHH gave preference to students graduating from the business concentration (which wasn't even offered at my high school). With that concentration, one could be admitted with a considerably lower high-school GPA than mine. Evidently, in those days, there was no appreciation for the notion that, in business and economics, mathematical ability could more than make up for lack of background in business subjects!

An option open to me was to study for a supplementary exam (in economics, law, business correspondence in English, German, and French, even in typewriting, which has served me well ever since!) to make my high-school education equivalent to that in the business orientation. This could be done through correspondence courses, which is what I decided to do. In the meantime, Aarrestad encouraged me to stay for another year in Oltedal, teach two subjects in his junior-high program, continue to do the accounting for him, and be fully in charge of running one of his two businesses (which imported tropical fish from Holland and distributed them to retail stores all over Norway) on a profit-sharing basis. As a result of being busier than expected, I was far from being finished with my correspondence courses at the end of the year, so I decided I might as well get my one-year mandatory military service out of the way and continue my correspondence courses while in the army. The following May, in 1965, I took the exams in Sortland, the location of the nearest high school with business concentration, during a two-week leave from the army and did well enough to be admitted to NHH starting that August. Four years later, I was off to the United States.

Doctoral student

Liv and I arrived in the United States in July 1969, and I still remember vividly when Sten Thore first took me to the Graduate School of Industrial Administration (GSIA) at Carnegie-Mellon University. We entered the building through the back entrance and immediately, on the back steps, met two professors to whom Sten introduced me. One was Herb Simon.

My formal status at GSIA was visiting student. Although I was there to work half time for Sten, I still signed up for three core economics courses: macroeconomics by Martin Bronfenbrenner, econometrics by Marty Geisel, and general equilibrium theory by John Ledyard (intended for second-year students, but my mathematics elective at NHH, along with subsequent math courses there, made it eminently manageable for me). Moreover, I decided, probably with the encouragement of Sten, to take linear programming from Egon Balas. About a month into the semester (while still doing a moderate amount of work for Sten), I came to the realization that to make it in research I needed a doctoral degree. I applied, and was promptly accepted in the doctoral program.

In December, Sten and his family returned to Norway and we were on our own, although I was still supported financially by NHH. Nonlinear programming by Egon Balas was a useful course for me. So were statistical decision theory taught by Morris DeGroot and microeconomics by Mort Kamien. But the most unusual course that first spring was economic fluctuations by Bob Lucas. He started out with basic mathematics, such as Kuhn-Tucker theory and functional equations, interspersed with economic applications. One day, sometime after the midpoint, Bob started setting up a model. In the following class he told us to scrap everything he had said the last time. He started over again, making a simplifying assumption or two, and then, over the course of the next couple of lectures, took the analysis to its conclusion. Later, we realized we had seen his paper "Expectations and the Neutrality of Money," for which he was later to get the Nobel Prize, being developed right there in front of our eyes.

GSIA was (and is) unusual in at least two ways. One was the small class size, which promoted a co-operative environment among the students. Also, there was relatively little course work. Most of the material taught was foundational, with emphasis on tools to put the student right on the research frontier. Important requirements were the first- and second-year summer research papers. My first-year paper was entitled "Duality in Fractional Programming," a topic that came to me partly because of the project for the shipbuilder in Bergen, partly because of all the mathematical programming I was taking. The paper ended up being my first serious publication, in *Naval Research Logistics Quarterly*, which my professors told me was ranked third in the operations research field. My main advisor was Bob Kaplan, later to become the dean of GSIA, whose specialty was dynamic programming (but who years later moved into accounting).

My second-year summer paper also involved dual prices, this time in hierarchical linear programs, and it was published in *Management Science*. By that time, I had become interested in an economic topic that went under the name of "the assignment problem." It had generated a substantial literature. The idea was, within the system-of-equations framework dominating macroeconomics at the time, that fiscal policy was more effective at achieving certain goals and monetary policy was effective at others. If the right instruments were assigned to the right targets, the economy would function quite well, while if the wrong assignment was chosen, the economy would function poorly and could, in the worst case, even be unstable. My idea was to think of the monetary and fiscal policymakers as separate decision makers with different goals, such that all the target variables would enter each objective function, but with different relative weights. Thus, a dynamic game resulted. Moreover, because of the differences in policymaking process for the alternative instruments, it made sense to me to think of the fiscal policy maker as dominant in the sense that he went first in every period, with the monetary policy maker a follower. This set-up represented an alternative to the symmetric noncooperative (Nash) solution.

In August 1971, I happened to run into a new professor who temporarily had been placed in a windowless office (presumably due to his arrival well before the start of classes), which later served for years as the mail room. He introduced himself to me as Ed Prescott and asked what I was working on. Evidently, he liked what I told him, and showed me some game-theoretic research he had done in an oligopoly context.

By the following spring, I had taken a course from Dave Cass who, like Ed Prescott, had arrived in GSIA in time for the 1971-72 academic year. I had had some conversations with him, and among other things told him about one of my findings, that even in the symmetric noncooperative case, the outcome was different when the solution was regarded as a policy rule in a recursive way as opposed to a *sequence* of decisions for the entire future. Dave asked me to prove it, as this finding went counter to the well-known property in single-player control theory that the solutions in what can be called policy space, on the one hand, and sequence space, on the other, give identical outcomes. Of course, once Dave saw the proof, he bought it right away. Moreover, I argued that the solution in policy space represented a more reasonable equilibrium from an economic standpoint.

That spring, I presented my thesis proposal to the faculty. The main element, motivated primarily by my version of the assignment problem, represented an application of dynamic game theory. Immediately after the faculty's deliberations, Bob Kaplan, who had been my main advisor up to that point, came to my PhD carrel with the outcome. In addition to informing me that I had passed, he told me that Ed Prescott had insisted on becoming the chairman of my committee. All along I had thought it would be Dave Cass (who still was to be a member of the committee, as was Kaplan). Thus started in earnest years of productive and much-appreciated interaction with Prescott.

The 1977 paper ...

After four years of PhD work, I defended my dissertation in time to graduate in May 1973. It was time to return to Bergen. While Cass and Prescott had both suggested I would do well on the U.S. job market, I felt obliged to take a position offered me by NHH. They had provided full financial aid for my studies. But one thing remained to be done. In April, around the time the final draft of my thesis was turned in, Ed had shown me a new paper by Bob Lucas. Its title (at least in its final draft) was "Econometric Policy Evaluation: A Critique." Ed had given it to me to consider whether one of the models Bob had used as examples of his critique could be banged into my dominant player framework, with a government objective added to Bob's general set-up. In their *Econometrica* article on "Investment under Uncertainty" two years earlier, Bob and Ed had shown, using a dynamic industry model with capital accumulation, that the competitive equilibrium could be obtained quite simply by solving a particular stand-in consumer-surplus problem. So we thought why not do something similar, but then add that the industry was affected by cyclical investment-tax-credit policy? We submitted an abstract to the June Stochastic Economics and Control Conference, an annual conference that had recently started up at the initiative, I believe, of David Kendrick and Gregory Chow, and then we worked like crazy to get it in shape.

So while Liv and our one-year-old son Martin left for Norway at the end of May as planned, I stayed behind for an extra month to work on the paper and to attend the conference. I was pleased (as was Ed, I'm sure) with the attention the paper got.

The first year at NHH was frustrating as it was clear that modern macroeconomics was not the school's strength. Soon I came up with the idea of inviting Ed to spend a year at NHH. He seemed interested, and I set the wheels in motion to drum up financial support for his stay. I succeeded, and Ed and his family showed up in time for the 1974-75 academic year. By that time, two significant things had happened. In my thesis, although describing three different solutions to the dominant-player game - the recursive without commitment, the commitment solution in policy space, and the commitment solution in sequence space - I had calculated examples of only the first, in part because that's the one I argued was *the* candidate for an equilibrium. Also, the other two were much harder to calculate. During my first semester of teaching, I had identified an exceptional undergraduate, Nina Bjerkedal, whom I encouraged to become my research assistant. I gave her the task of writing a FORTRAN program to calculate the profits of a dominant firm on the assumption that it could commit to its optimal policy. It turned out these profits beat those of the otherwise time-consistent outcome by an astounding margin. Moreover, when Ed arrived, he had clearly warmed to the idea that the focus of our paper had to be a comparison with and without commitment. We expected the result that the time-consistent solution could represent quite an undesirable outcome.

Our progress initially was slow because Ed was busy with other matters and I was in the middle of getting out three papers based on my dissertation. One contained a description of the solutions that I argued, from an economic standpoint, represented dynamic equilibrium outcomes in symmetric noncooperative games and in dominant-player games (*International Economic Review* 1975), one was on the assignment problem (*Annals of Economic and Social Measurement* 1976), and one focused on dynamic dominant-player games (*Journal of Economic Theory* 1977).

When Ed and I finally got going on our paper in the spring, we first worried about two key issues. Our intuition was that the difference between the two solutions was greater when a lot of inherent dynamics was present in the model. Sticking to our stochastic model of capital accumulation in the face of government stabilization policy using the investment tax credit, we introduced "time-to-build" into the model *in combination with* the standard cost of adjustment. Secondly, we had realized, after we wrote our 1973 paper, that we needed to make sure the rest of the economy was treated explicitly as being inhabited by atomistic agents (not treated as one player). Ultimately, the appendix dealt with how to solve that issue (the "big K-little k" problem).

I had submitted my assignment-problem paper to a stochastic control conference to take place in Boston in May. At some point early in the conference, Gregory Chow announced a session for work in progress. I signed up to talk about Ed's and my paper, and was told I could go first. All hell broke loose. Everyone was trying to locate the error. Admittedly, we had chosen a rather provocative title for our first draft: "On The Inapplicability of Optimal Control for Policy Making." I was certain nothing was wrong. With all my experience in dynamic dominant-player games, I knew time inconsistency had to be an issue. I suppose at that point, after what happened at that presentation, I realized our findings could generate considerable attention. Moreover, as a consequence of the difficulty people had in understanding the time inconsistency, we decided to add, for expository reasons, a Phillips-curve example to our investment-tax-credit example before we resubmitted our revised version of the paper to the *Journal of Political Economy*. As I recall, it was motivated by a model in a recent paper by Phelps and Taylor. Of course, that example has turned out to be used a lot by subsequent writers.

With the rules-vs-discretion paper pretty much done, I dabbled in industrial organization for a while, especially pushing further the dominant-firm model from my dissertation. I had gone on leave for the academic year 1976-77 to the University of Minnesota (never to return, as it turned out, to Norway for any permanent position). While in Minneapolis, I was invited to visit Carnegie-Mellon University the following year. During that year, I was offered an associate-professor position, which I accepted.

... and the 1982 paper

By that time, Prescott and I had started to work on business cycles. Some of the computer programs used in our 1977 article could be adapted quite easily to calculating dynamic equilibriums of business-cycle models. In the beginning, we considered models in which we made the technology linear and the representative household's utility function quadratic. We included technology shocks, but at first, in large part because of Lucas's 1972 article, we didn't think we could do without monetary shocks. For an NBER conference in 1978, we wrote a paper that was somewhat schizophrenic. It contained a business cycle model, but also evaluated stabilization policy. The main idea behind the latter was that changes in taxes were costly as a way to balance the government budget over the cycle. Instead the "slack" should be picked up by fluctuations in government debt. In the end, we were asked to reduce the length of the paper for the resulting conference volume published by the NBER in 1980, and we had to leave out much of that material.

Instead, we wrote another paper on policy - a standard growth model with labor and capital taxes financing government purchases - and made the point that capital taxes should be low to maximize the representative consumer's welfare. At the same time, lack of commitment was likely to lead to capital taxes that were much higher than optimal. That paper, published in the 1980 *Journal of Economic Dynamics and Control*, also dispelled a common misunderstanding at the time, namely that time inconsistency originates from the policy maker's objective being different from those of the people. Our model economy is inhabited by millions of people who are all alike, and the government is assumed to have preferences that coincide with those of the representative household. Finally, the paper presents a way of determining the optimal policy (with commitment) through the use of a particular shadow price as a state variable. Variants of this basic method are still being used in many contexts.

A key breakthrough in Ed's and my continuing business cycle research was the realization that we could start with the basic growth model, with both production and utility functions in exponential forms and, to the extent necessary, approximate in order to make the computations tractable. Suddenly, the number of parameters was much smaller and their calibration transparent. For example, first-order conditions for the steady state implied relations between the parameters and steady-state values of the model aggregates, the latter with counterparts in corresponding average values in the data. With those averages quantified from the data, one could then map, through those first-order conditions, to the parameter values that would reproduce the steady-state values corresponding to the data. This procedure is called calibration.

The paper published in the 1980 NBER conference volume (titled *Rational Expectations and Economic Policy* and edited by Stanley Fischer) was our first real-business-cycle paper. In the summer of 1979, we had written the first draft of another paper, later to be published by *Econometrica* in 1982, in which we put our vastly improved calibration procedure to use. My first presentation of it took place at Cornell University in the early autumn of 1980 (in a job talk; evidently, the paper did not impress them, as I was told later that no offer would be forthcoming).

Loose ends

In our first two business-cycle papers Ed and I were surprised to find that including a role for money made very little difference to the model outcomes. Virtually all the "action" came from technology shocks and, moreover, they accounted surprisingly well for the data. In 1980, I wrote a first draft of a paper in which I focused on the role of money for the business cycle. I used two separate propagation mechanisms, one based on imperfect information in the spirit of Lucas's paper on "Expectations and the Neutrality of Money," and one in which there was a household trade-off between time and holdings of money. In the latter, the cyclical movement of the price level was in the right direction and of considerable magnitude, even when the central bank let the money stock grow steadily without fluctuations. The most interesting finding of the first part of the paper was that, while price shocks had the effect, as in Lucas 1972, of making people act in part as if these nominal shocks represented changes in real prices (because of the imperfect information), they also led people to react less to everything, including the real shocks, and the latter effect turned out to dominate, so that, for the calibrated economy, the larger is the variance of the price shocks resulting from central-bank behavior, the lower the business-cycle volatility. (Of course, this reduction in volatility is by no means welfare improving!)

While I gave this paper several times, and it was once on the program of the winter meetings of the Econometric Society, it continued to remain on the back burner in the sense that there always seemed to be projects with higher priority. In the end, I got the paper in shape to submit it to the *Journal of Monetary Economics* and got a favorable revise and resubmit. But then, in response to an invitation to a conference on the 100th anniversary of Ragnar Frisch's birth (whom I admire greatly and regard as having been well ahead of his time in the 1930s), I decided the paper fit well for that purpose and elected to use it there. The proceedings were to be published in a special volume of the *Econometric Society*. Unfortunately, the referee criticized the paper on issues related to the by then standard approximations (in my opinion a completely peripheral and inconsequential issue relative to the question addressed), and the editor, along with the conference organizer at the University of Oslo went along with the referee. At that point, three or four years after the *JME* referee report, I figured it was too late to resubmit it to them, so the paper remains unpublished (but often cited).

Much of my research immediately following upon the 1982 *Econometrica* article revolved around the labor market. The main anomaly relative to Ed's and my business-cycle model was the high cyclical hours-of-work volatility in U.S. data. We were convinced that a large part of the discrepancy was attributable to the simplicity of the abstraction. All model people were assumed to be alike. Literally speaking, all of the model's labor-input variation is in hours per worker, while, empirically, much more of it is in the form of changes in the number of workers (employment). This issue was dealt with beautifully by Gary Hansen in his 1985 *JPE* paper.

Another aspect of reality, not shared by our model, is the workforce's great variety of skills for market production. When Allan Meltzer called me sometime in the autumn of 1982 (while I was visiting the Hoover Institution for a year as a National Fellow) and asked me to write a paper for the 10th anniversary Carnegie-Rochester Conference to be held a year later, suggesting that the organizers would like a paper on the importance of contract theory for the business cycle, I was so convinced of the much greater importance of heterogeneity of workers' skills that I decided, without consulting Allan, that that was the labor-market topic I wanted to write about. So in the paper, entitled "Labor-Force Heterogeneity and the Business Cycle," I document with the help of data from the Panel Study of Income Dynamics (PSID) the vast differences in cyclical hours behavior depending on skills, and then construct a model with two skills categories, calibrated to what was known about the workforce if it is divided in two, according to skills. One implication of the model indeed turns out to be substantially greater work-hour variability than in the basic equal-skills model.

Part of the problem was that, given the form of the 1982 model, comparing its labor-input implications with aggregate hours of work could be quite misleading. A better measure would quality-adjust the hours of each worker before adding them up. That's what Ed Prescott and I did in a study a few years later. Based on data from the PSID, we found, for that sample, that the volatility of unweighted total hours was 40 percent greater than that of a weighted measure. This paper was published in 1993 in the Federal Reserve Bank of Cleveland's *Economic Review*.

Finally, as it was clear while working on our 1982 paper that greater intertemporal substitution of labor was a way to produce more labor-input volatility in the model, as an alternative to our standard utility function we had also introduced one that was nonseparable over time in leisure. As later shown in my above-mentioned labor-force heterogeneity paper, this utility function can be regarded as a stand-in for a particular household-production formulation. Still, we were somewhat in the dark as to what were reasonable values of the two additional parameters characterizing the nonseparability. In a subsequent study with Joe Hotz and Guilherme Sedlacek, published in *Econometrica* 1988, those parameters were estimated based on data from the PSID.

A prevalent misunderstanding in the early 1980s was that Ed and I had put forward our 1982 model as a way to "fit" the data. As this misunderstanding was still evident from the general discussion following the presentation of my 1984 paper at the Carnegie-Rochester Conference session, I took the opportunity, in a rejoinder to the discussant's comments, to state as precisely as I could what the question had been: If technology shocks were the only source of impulse to post-war U.S. business cycles, what portion of the cycle would remain? Our answer, based on that model, was over 50 percent. Importantly, this measurement was based on a calibrated model that was explicit about the dynamic decision problems faced by the model's people and businesses.

My co-authors

Throughout my academic careeer, I've had the great fortune to work with so many eminent researchers. I've already mentioned Hotz and Sedlacek. Prescott and I continued to do joint work off and on over the next decade. One focus was on variable capital utilization in the form of variation in the hours the capital is used. This general theme is reflected in our 1988 *Journal of Monetary Economics* paper and in our 1991 *Economic Theory* paper. As our business-cycle methodology received greater acceptance in the profession, we were also asked for methodological or expository contributions, which resulted, in particular, in our papers in the *Scandinavian Journal of Economics* 1991 and *Journal of Economic Perspectives* 1996.

The area of international macroeconomics has been ripe with puzzles and anomalies. An example is the fact that, between pairs of major countries, the cyclical consumption correlations are about the same as the corresponding output correlations. Another seeming anomaly is that, cyclically, the trade balance is the worst (that is, more negative) when one's goods are the cheapest. Beginning with a one-year stay at the Federal Reserve Bank of Minneapolis in 1989, David Backus, Patrick Kehoe, and I wrote several papers on the general subject of international real business cycles. Indeed, our first paper, in the *Journal of Political Economy* 1992, is entitled exactly that. We find, among other things, that the "consumption anomaly" mentioned above is quite robust, even under serious impediments to trade. Another paper was published in the *American Economic Review* 1994. In this paper, we find that the seeming anomaly involving the trade balance and the terms of trade is precisely what a calibrated model says should happen.

In the early 1990s, Bill Gavin encouraged me to become a research associate at the Federal Reserve Bank of Cleveland, which entailed occasional visits there. Slowly but surely we started to work on questions having to do with the role of money for the business cycle. When, after some time, Bill moved to the Federal Reserve Bank of St. Louis, he talked me into continuing to visit him there. This working relationship has resulted in several papers. One in the *Review of Economic Dynamics* 1999, for example, documents differences in postwar co-movements of monetary aggregates with real GDP before and after 1980. The price level is still countercyclical in both subperiods, and the co-movements of various real aggregates with real GDP are very similar. Then we go on to show that a monetary model with fully flexible prices, but with differences in policy regimes, can account for the different episodes. It is well known in policy circles that an important change in monetary policy did indeed take place during and after the Volcker era at the central bank. The subsequent work with Bill has, to some extent, been joint with others at the St. Louis Fed, in particular Rob Dittmar and Mike Pakko.

After Gavin had left the Cleveland Fed, I was still encouraged to continue visiting there for a couple of weeks a year. Over time, I started to work with Peter Rupert, first on measuring quality-weighted labor input based on Current Population Survey data, which are monthly, more up-to-date, and cover many more workers than does the PSID. After a while, we, along with Paul Gomme, also became interested in a major anomaly in the literature on the interaction of household and business activity, a literature that had sprung up in the early 1990s. The anomaly had to do with the degree of contemporaneousness of cyclical movements in household and business investment, with the former leading the latter. Our most significant paper so far made headway on that anomaly and was published in the *Journal of Political Economy* 2001.

In the mid 1990s, I was hired by the University of Texas, supposedly to help them build up the macro group. Unfortunately, while everyone in my family enjoyed Austin very much, in the end the administration's concept of keeping a promise was quite disappointing, and we unhappily left. But that stay had two longer-run consequences. Scott Freeman and I had talked about writing a paper about the interaction of money and output over the business cycle, using a novel model with both inside and outside money, and we finally completed it after I had left Texas. The paper ended up in the *American Economic Review* 2000. Subsequently, Scott and I wrote a follow-up paper for a conference at the Federal Reserve Bank of Cleveland, this time jointly also with Espen Henriksen, then a GSIA PhD student.

While in Austin, I was signed on as a research associate at the Federal Reserve Bank of Dallas, a relationship that has continued to this day. While there, I have done research with D'Ann Petersen, Mark Wynne, and Carlos Zarazaga. The work with D'Ann represented the beginnings of a labor-input measurement project. Mark and I (along with Alan Ahearne, a former student of mine at Carnegie Mellon, now at the Federal Reserve Board) decided to investigate the reasons for Ireland's spectacular growth after 1990. Carlos and I embarked on a study of a much less successful nation - Argentina - and wrote a paper on its great depression in the 1980s, published in the *Review of Economic Dynamics* 2002.

Subsequently, Carlos and I decided to plug into our model - a standard version of the neoclassical growth model - the numbers for the period after 1990. To our great surprise, the model predicted that, while output from 1990 to 1998 had grown at a rapid pace by most standards, GDP and especially the capital stock should have grown much faster in light of the productivity growth that had taken place. This is a curious finding. Why should it be so? Did Argentina suffer from the "time-inconsistency disease" to such an extent that, even with the currency board instituted by former President Menem in 1990, the nation's credibility among investors still was much too low? This research highlights the dire situation in which Argentina found itself after it experienced a second and much faster-progressing great depression after 1998. By 2003, measured capital per working-age person had fallen an astounding 20 percent relative to 1982, with pessimistic implications for the average real wage as well as for the income distribution.

In the early 1990s, Michael Bordo came to visit Carnegie Mellon for an extended period. Our interaction during his stay resulted in two papers in which we interpret the history of the gold standard in light of the time inconsistency of government policy and the need for a commitment mechanism to maintain credibility.

Dynamic macroeconomics, with the explicit analysis it entails of the decision problems of households and businesses, is not easy to teach and learn at the basic level. My impression over the past couple of decades is that the gulf between the research frontier and what's in undergraduate textbooks has grown wider and wider. So ten years ago, I started to use the computational experiment as a teaching tool in part of my semester-long course in intermediate macroeconomics. At first, the students would simply use the executable version of a FORTRAN program, with its rather crude input and output formats. A few years ago, during my annual summer visit to Bergen and NHH as part of my adjunct professor position, I was able to interest Solveig Bjørnestad of the Institute for Information Science at the University of Bergen in the idea that she and her students, in co-operation with me, could design and develop a learning environment that would be user friendly, perhaps even fun, for the students, with the goal that it would enhance dramatically their understanding of dynamic macro. This co-operative effort still continues. A detailed description of the approach and of the contents so far is written up in our joint 2004 paper.

During all of this, I have helped to raise four children: Marty, Eirik, Camilla, and Kari. At times I've wondered if academic life took away too much time, which I otherwise could have spent with them. Some years ago, Liv and I parted ways. Liv claims that the extent of my travels took its toll. She has since remarried.

Almost two years ago, I had the incredible luck to stumble upon Tonya Schooler (now Kydland), a wonderful person, and we have been inseparable ever since. The complications of three academic careers (including her ex-husband's) combined with co-raising her school-age children, Joel and Rachel, took us, in August 2004, to the beautiful city of Vancouver. I accepted the Jeffrey Henley Chair in Economics at the University of California at Santa Barbara. Admittedly, Santa Barbara is still a long commute from Vancouver, but one in which Tonya has been happy to share. In the spring of 2004, the University of British Columbia had made me a nice offer, but their rule of mandatory retirement at 65, from which their provost refused to make an exception for me, was inconsistent with my desire to remain an academic and continue my research until I drop. I believe my co-researchers are happy, I'm slowly but surely finding more time to get back into what I love to do. In my case, one of the benefits resulting from the Prize is the availability of funding to set up a major research institute at UCSB. Among other things, it will bring researchers from all over the world together for periods of time to brainstorm on important outstanding questions and anomalies in macroeconomics and finance.

From *Les Prix Nobel*. *The Nobel Prizes 2004*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 2005