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The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 2006 'for his analysis of intertemporal tradeoffs in macroeconomic policy'.

INTRODUCTION

It's a beautiful warm summer day, and it's definitely nicer outside than inside the 'Center on Capitalism and Society', a number of rather charm-free office cubicles at The Earth Institute in Columbia's Hogan Hall, which Edmund S. Phelps presents with an ironic grin: 'Well, this is it.'[1] The Center has been created in 2004 with money from the Kauffman foundation.[2] Its purpose is

> to go beyond the mainstream models of markets to a serious study of capitalism, to the questions about its dynamism and its stability and how capitalism compares in these respects with its rivals, corporatism and market socialism. Such a study is imperative because a country needs to have a more thorough and reliable understanding . . . to make institutional choices. . .[3]

Interestingly, the Center has a rather 'Austrian' research agenda, and Phelps himself dryly affirms: 'Capitalism is Hayek country.'[4] But most of the Center's members can be said to be eclectic neo-Keynesians. Phelps himself is the embodiment of such a mixture, fully appreciating the self-regulating and knowledge-generating forces of market interaction on the one hand while not having excessive qualms about asking for some kind of state intervention on the other hand. Members of the Center include Amar Bhidé; Glenn Hubbard, formerly Chairman of the Council of Economic Advisers; Roman Frydman; Janusz Ordover, formerly chief economist for anti-trust in the Department of Justice; Jeffrey Sachs, Director of The Earth Institute; Robert Shiller; Joseph Stiglitz, Nobel Laureate of 2001; and Phelps himself as Director. The Center issues its own journal, by the name of *Capitalism and Society*.[5] The door to Phelps's office is always open; the noise level is not really zero, but moderate. Neon lamps artificially brighten up the place with their cold, no-nonsense light. A couple of bookshelves along the wall bear witness of the owner's own work – and of his various sources of inspiration.

But Phelps, right now, isn't inspired, he isn't even motivated, he is just tired. He remains charming as usual, but frankly, our conversation drags on. But it's not that he doesn't like to tell his story. It's also not that he needs time to warm up to his interviewer; we first met six years ago and have been friendly ever since. The reason is rather that he would like to take a nap and get over jet lag. Ever since his 'Nobelity', speaking engagements and interview requests have multiplied, and Ned Phelps is constantly on the road. Not that he can't stand traveling. He actually likes it very much – but in style. And that's not always guaranteed, even though, only ten months after the award, he already has managed to earn himself the reputation of being one of the most 'expensive' and 'difficult' Nobel Laureates. These absences take their toll on his research. 'Ever since I've received the Nobel Prize, I don't have so much time. I'm now really drawing on what I've done before. I don't like that', he complains. And then, after all, research in economics is not all that life is about. Phelps loves New York City life, the opera, concerts, theater. He is a trumpet player and also a gifted singer himself – and he often proves his talent at home, when he gives receptions and dinners at his apartment on the Upper East Side together with his Argentinean wife Viviana Montdor. He loves good food, he adores champagne and good wine: over the years, he has become an accomplished epicurean. He does and wants to enjoy life. Locking himself up in a sort of monk's cell atmosphere in order to be productive in research is something he did earlier in life but just wouldn't do any more. 'It's too big a price to pay', he affirms, serious for once. It is true that Phelps is proud of his own achievements, eager to preserve a good public image, and receptive to applause. For example, just two weeks before his prize was announced, he posted a new, extensive CV on his website, with helpful links and all relevant telephone numbers clearly marked. Maybe he just had an excellent intuition. Notwithstanding this, Ned Phelps shows an impressive degree of self-irony and laughs a lot. As a consequence, our conversation is not only instructive, but also tremendously fun to read.

Edmund Strother Phelps was born in Evanston, Illinois, in 1933 – at the height of the Great Depression. The family background was 'bourgeois', as he calls it. His father was in advertising, and his mother a nutritionist, and both had benefited from some economics education. He calls it a 'very practical background of economics', though. The financial and economic news was ever-present in dinner-time conversation. Both parents lost their jobs and got by only with help from their parents, until his father found a new job in New York. Young Edmund S. Phelps was more interested in music than anything else, playing the lead trumpet in various bands. Instead of pursuing a music career, he ended up going to Amherst College to get his undergraduate education. At first, he flirted with philosophy, but upon his father's request, he also took a course in economics – and that decided him to go for economics all the way. 'This was an extremely good fit for me', he says. 'It was a godsend.' And yet, philosophy had left its enduring trace in the encompassing depth of his thought. Reaching the end of his college years, Phelps went on to Yale for graduate studies. At Yale, very much a stronghold of Keynesianism, he was influenced by distinguished scholars such as William Fellner, Jacob Marschak, Tjalling Koopmans, Gérard Debreu, Robert Triffin, Henry Wallich, James Tobin and Thomas Schelling. James Tobin, his dissertation adviser, gave him the idea to write his thesis on the effect of demand and cost shocks on the correlation between changes in prices and output. It was a mathematically demanding paper, but conceptually, it was 'awfully problematic', as Phelps recalls[6] – which is probably the reason why no direct reference to it can be found anywhere. He received his Ph.D. from Yale in 1959 and then took off to the RAND Corporation in Santa Monica. After a year at the RAND Corporation, Phelps returned to Yale on behalf of the Cowles Foundation as an assistant and then associate professor, eventually, however, moving on to the MIT. In 1966, he spent a sabbatical at the London School of Economics (LSE) and at Cambridge. From 1966 to 1971, Phelps then assumed a professorship at the University of Pennsylvania. In 1969/70, he spent a year at the Center for Advanced Study in the Behavioral Sciences (CASBS) at Stanford University and got in touch with John Rawls. In 1971, for both professional and private reasons, he left Penn and moved back to New York, where he had found employment at Columbia

University. New York, to him, is 'the most exciting city in the US', and it is also close to the suburb of Hastings where he had grown up. 'So it seemed very natural to want to be back to the number one city', he says in our interview. So Columbia is where he's stayed until today, interrupted only by a short intra-city stint to New York University in 1978/79 – and of course by regular trips to various destinations in Europe. Ever since the 1980s, Phelps has increased his overseas collaboration with European universities and institutions, ranging from the Observatoire Français des Conjonctures Economiques (OFCE), the Banca d'Italia and the European Bank for Reconstruction and Development (EBRD), to the University of Rome Tor Vergata.

Edmund S. Phelps's first years in active research were the direct outcome of his training; they were dedicated to growth theory. One important paper, written in 1961 at the Cowles Foundation, adopting the intertemporal viewpoint, provided 'The golden rule of accumulation'.[7] He also published a book on economic growth.[8] Beyond that, a 1966 paper highlighted the role of managers in the diffusion of new technologies over the economy,[9] and another one, also from 1966, dealt with research and development and their relation to economic growth.[10]

Perhaps the most crucial period, however, began when Phelps left for the London School of Economics in 1966. This was the beginning of his famous 'micro–macro' years. Actually, most of Phelps's work ever since has been motivated by a profound uneasiness with the traditional 'schism' in economics, dividing the field up into microeconomics and macroeconomics. As he says in our interview, 'those Keynesian models where you turn the crank and you get what the GDP is going to be may be fascinating and they may even have some practical value, but I never felt very comfortable with them. I didn't think they had much foundation.' Ned Phelps set out to integrate the two by providing a micro-foundation of macroeconomics. This, however, meant not just that a simple bridge needed to be built. Since there were outright incompatibilities between the two fields, he had to reformulate a lot of microeconomic theory so that it could become a foundation for macroeconomics. The critical element that he introduced into microeconomics was uncertainty, imperfect information and imperfect knowledge.[11] The most famous result of this work, focusing generally on the link between employment, wage setting and inflation, was Phelps's challenging the traditional Phillips curve which claims that there is a trade-off between inflation and unemployment.[12] In the framework of his new micro-foundation of macroeconomics, Phelps now argued that inflation also depends on the expectations of firms and employees about price and wage increases – to the effect that, since there can be no permanent discrepancy between actual and expected inflation, there is also no long-run trade-off between inflation and unemployment.[13] This implies that Keynesian demand management can only have limited, mostly transitory effects. This was a result that Milton Friedman also reached simultaneously, but without providing a micro-foundation.[14] Phelps's model, presented in his famous (1968) paper on 'Money-wage dynamics and labor-market equilibrium', became known as the (vertical) 'expectations-augmented Phillips curve'. This was also the time of the first efficiency wage theories, of search unemployment, and so on, all of which were part of Phelps's important paradigm shift: while Neoclassic and Keynesian theory had so far dealt with unemployment as being a disequilibrium, the difference just being that Neoclassics were more optimistic about a return to equilibrium, Phelps showed that there can be unemployment in equilibrium. Phelps also introduced the notion of a 'natural rate of unemployment', again alongside Friedman. The Keynesian school viewed all this as a terrific blow – which it was – and reacted rather unpleasantly, as Phelps recalls.

But then, in a typical dialectic move of science, a new front opened as Robert Lucas entered the stage with his rational expectations approach. Phelps preferred to think of expectations as being adaptive. But Lucas proved Phelps's model along his own lines and on his own terms.[15] This directed the attention away from Phelps – which the latter regrets: 'It was only a simplified version of what I was doing. . . But it prolonged the debate, because it afforded the Keynesians with the illusion of a victory.' The rational expectations approach went too far for him as well, simply because people aren't fully rational – but nevertheless, his subsequent research aimed at strengthening the neo-Keynesian approach again while taking the rational expectations hypothesis into account. Together with John Taylor and Guillermo Calvo, Phelps thus started a research program building on his 1968 paper, now employing sticky wages and prices. In such a setting, monetary policy can be effective.[16] Finally, in the 1980s, Phelps began to question the explanatory value of all the three existing monetary theories of employment, that is according to the New Classical, the New Keynesian, and the Neo-Keynesian approaches. This started his 'structuralism' years, a phase of non-monetary modeling of employment determination and the underlying time patterns, beginning with the Fitoussi–Phelps book in 1988[17] and ultimately resulting in the book *Structural Slumps* in 1994.[18] The next topic, mainly in the next decade, was labor market participation and inclusion.[19]

Ever since the fall of the Iron Curtain, however, Phelps has directed much of his attention towards the more fundamental issue of capitalism. The question concerning what it is that allows for and fosters entrepreneurial activity, how the necessary institutions come into being, how knowledge is passed on – all these topics are close to his heart. And that's not surprising: they are the logical continuation of his endeavor to provide the micro-foundations, that is to dig deeper, to get at the core of things, to really understand the world. Even if it comes at the price of what may look like eclecticism.

As for his major sources of inspiration, Phelps is pretty outspoken: it was his 'bourgeois' background, with economics being very much a topic at home; it was his father's direct influence, urging him to take at least one class in economics at college; it was a good teacher (James Nelson) and an accessible, excellent textbook (Paul Samuelson's *Economics*); it was a brilliant environment at the Ivy-League university that he went to (Yale), with the philosophical undercurrent that William Fellner provided, as well as the role model and the latitude that his doctoral adviser James Tobin gave him; and it was the era, of course: a time where the Keynesian paradigm in economics, while still fresh and flourishing, had already showed its imperfections, and therefore there was much room for improvement. Topics were abundant. In that sense, Phelps has also been a puzzle-solver, in Robert M. Solow's wording[20] – but his curiosity and his ambitions have always been those of a system-builder. Summing up, it is fair to say that all three fundamental lines of inspiration were present in Ned Phelps's case, interacting systematically and with pretty equal weight. Sometimes just everything comes together to provide an interesting playground for an active, gifted mind.

INTERVIEW

Ned, you once said that you always had sort of a predisposition for becoming an economist. What makes you say that?

I may have had in mind that I grew up in a somewhat bourgeois setting, one oriented toward business. My father's father was a shoe manufacturer and had several shoe retail outlets in the Midwest. He was successful and made a fair amount of money. My mother came from a family with a good-size farm in southern Illinois. She went to a college in the region and was a trained nutritionist. My father's profession was advertising. He had majored in economics at the University of Illinois between 1920 and 1924. He must have been a pretty good student. He remembered so many things about economics that I had never heard of. He was always enthusiastic about that subject. So I had this very practical background in economics; it wasn't a background in science or in the arts.

But does that mean that you were discussing the economic situation and economic policy at home, at the dinner table? It must have been, given that you were born at the bottom of the Great Depression, and I understand the economic turmoil of these years had its effect on your parents' household, with both your mother and your father losing their jobs. When your father finally found a new job in 1939, your family settled in a New York suburb.
Oh yes, we sure we talked about the economy and Washington. The financial and economic news was ever-present in dinner-time conversation. Also, like many middle class people in those days, my parents were very concerned about people who didn't have much opportunity, and I kind of absorbed that.

I suppose this probably triggered a rising political awareness in you.
I remember that my parents always voted for the left, for the Democratic Party. They had some Republican friends, too, and I remember they had exchanges. So I saw that there was a right wing also, and I had a bit of a sense of what the differences were between them. Some who voted right wing didn't like high taxes and didn't like big government. I assumed that it was a superficial, self-interested thing; that there was nothing intellectual about it. The two sides were not talking macroeconomics. But I must have developed some curiosity about whether there were deeper differences between the left wing and the right wing.

What kind of job did you dream of?
At high school, I thought maybe I would go into business, to be the manager or the president of a company. I felt that I could run an organization. It's not that I was tremendously gregarious, or that I could instantly command a room or anything like that. But I did notice that in our musical groups, I tended to be the person that it revolved around. I was usually the lead trumpet. I was the guy who tended to make it work. Also, when I was in grades 10, 11 or 12, I didn't have much sense of what else I could do in my career. I didn't have any sense of economics. Remember, in the US in those times, it would have been unheard of to have a course of economics in high school.

In Germany, that still is more or less the case.
In France, they do have such classes. With the effect that students coming out of high school seem to think they know all about Adam Smith and Alfred Marshall. The Germans, I notice, seem to be just about burned out by the time they get to university. It's all about getting a degree and moving on. In my case, it was an intellectual adventure when I went to college.

You started out at Amherst College. Did you then know where you were going – intellectually speaking, or in terms of a career?
No. At first, I got seduced by Plato and Hume and the humanists, such as Montaigne, which we were required to read. I was stimulated and was fully expecting to become a philosophy major. My father may have seen this coming. In any case, he asked me a favor: to take one course in economics. I did. And I realized within a week or two that this was an extremely good fit for me. I couldn't explain why, and maybe to this day, I don't know why. But it was clear to me that in a philosophy course, out of 50 students, I could be among the three or four best. In economics, however, it was easy for me to be the best, certainly number one or two. I realized that I had an unusual talent for it. That counted for something. I mean, you like to be able to succeed when you study. And I also had a sense that with economics, I'd be able to get to the bottom of some of these political questions about left and right.

The fact that your father kind of pushed you in that direction, didn't that turn you away? Didn't you react in a rebellious way?
My father had hardly asked anything of me before so I didn't feel very pushed. Besides, the subject was just as interesting to me as philosophy had been. It came as a revelation to me that economics in the hands of Paul Samuelson, the author of the new textbook I was reading, could be so stimulating.[21] The lectures by James Nelson were also brilliant. Both of them were quite fun, too, and they were great comedians when they wanted to be. I concluded that this wasn't a second rate subject. This was probably as good as it gets. And there was a lot of stuff there to be thought about and to be made sense of. So no, I didn't have any resentment at all. It was a godsend.

And when you started out, did you find economics easy?
It was easy, from day one. But you asked about rebellion against one's parents. Remember, my mother was a nutritionist and my father was working in a bank in those days. I thought that I was now going to be on a higher plane. I was going to be the philosopher economist who would settle the issues of left versus right. So in a way, I did put some distance between myself and my parents.

When did you start thinking that economics could be a career for you?
It was not before I was well into my third year at college that I began to think about the relation of all this to a career, wondering whether it would be possible to make a living with this stuff. I think it was probably at the very end of the third year or perhaps at the beginning of my fourth year that Paul Samuelson was prevailed upon to come to Amherst and give a lecture and

meet some of the more promising seniors who were majoring in economics. He talked to me for about 20 minutes or so. He talked as if it was a given that I would be going to graduate school and that I'd find it interesting to go on to become a professional economist. So I thought, well, let's give this a try, I'll go to graduate school and I'll take each day as it comes and see whether I can find interesting challenges and some fun in the process.

I guess that if someone whom you admire treats you that way, expecting you to go on, it must be a tremendous motivation.
Yes, it was. I felt some pressure to live up to expectations.

So what made you go further was, for a good part, a top education and motivational support. What about the internal motivations, motivations that came out of the subject itself? You once joked that you went on and on with economics because you hoped that if you took just one more course you would finally understand it.[22]
It's true that I was trying to get to the bottom of the subject. But there was also my interest in left versus right – in the good economy, though that was somewhat unconscious.

You then went on to Yale on a fellowship. Who were your teachers? And how did they teach? What did your classes look like in those days? Was it very mathematical?
No, there wasn't a lot of math. I had one professor by the name of William Fellner. He came from Hungary. He had come from a business background. His father was the owner of a large brewery in Budapest. So Fellner was not only a highly intellectual person, smart, serious, cultivated; he also knew more than a little about business. He could actually talk about things from a realistic point of view that I had never heard before in a classroom.

Who else was there to impress and inspire you?
Another very influential teacher was James Tobin.[23] He taught me a ton of macroeconomics. I thought that the material was really clever and important. But I also thought it was maybe not fundamental enough. I appreciated the story, apocryphal I am sure, told in class by the instructor who was teaching the first year statistics course to graduate students, Robert Summers, father of Larry Summers, brother of Paul Samuelson and husband of Kenneth Arrow's sister Anita.[24] The story goes that a lecturer expounded the workings of a mathematical economic model, as if the economy was like a mechanical toy, upon which a student raised his hand and asked: 'But sir, where are the people in that model?' That student was so right. The Keynesian models where you turn the crank and

you get what the GDP is going to be may be fascinating and they may even have some practical value, but I never felt very comfortable with them. I didn't think they had much foundation. What I liked about Fellner was that he was very concerned with the foundations. You could see that he thought in terms of people and what their situations were, their beliefs, and their expectations. I soaked that up. Another very important influence was Thomas Schelling.[25] He was sixteen years younger than Willy Fellner. He had already had a practical career in Europe with the Marshall Plan, and in Washington, and now there he was, a still very young full professor at Yale. I just saw him recreate himself, in front of my very eyes. I saw him turn to issues of bargaining and transactions typically among small numbers of persons – what we now think of as game theory. But he didn't think of this as game theory yet. He was building this stuff from the ground up. I saw him create these little models of reciprocal fear of surprise attack.[26] It was bold, and I wondered whether I would ever be able to do something like that. Later, maybe two or three times in my career I did something as stimulating as that. But that doesn't matter. At any rate, Tom Schelling was a living example of what I thought it might be possible for me to do.

Specifically because he was already working on some kind of microfoundations?
Yes. He was in fact interested in micro–micro. He was interested in thinking about the interactions of people at a very realistic level.

What was it that bothered you about macroeconomics the way it was?
There was no micro in it. There were no people, no beliefs, and no expectations. I saw Fellner and Schelling as having the people very much on their minds. So my research agenda was to get the people into growth economics. Growth economics was first. And then later I was concerned with getting the people into employment economics, along the lines that Fellner and Schelling would have said was about right.

Why was it so important to have micro within macro? I mean – microeconomic theory was out there, price theory had been fully established much earlier. It wasn't as if nobody had ever been thinking about human action and individual choices before in economics. Micro and macro, the two building blocks of economic theory, coexisted side by side, reflecting two different levels of abstraction. What was missing was a proper integration of the two. Why did you feel this was indispensable? Why is it important to understand the – non-mechanical, but interactive – process of aggregation by which you get from the micro level to the macro level?

It was not just about integrating two separate views that are like two ships in the night, not dependent on one another. The problem was that there actually was a real incompatibility between the two. And I wasn't the only person to see that there were contradictions. Standard microeconomics implies that there will be full employment, and that changes in that level of employment will depend upon things like changes in technology, changes in the capital stock and changes in the weather. But this is nothing at all of what Keynesian economics says determines employment. Keynesian economics delivers implications that are inconsistent with standard micro. It became clear that we cannot simply use the existing – neoclassical – microeconomics as the foundation of Keynesian macroeconomics. So – well, I was going to say that I had to invent a new microeconomics. But that's a little bit pretentious, and inaccurate. I had to make use of the little bit of knowledge that I had of microeconomics and start there and build up alternative microeconomic models that could easily be hooked up to macroeconomics. I'm not sure I can document it very well, but I have always felt that my year at the RAND Corporation[27] right after my dissertation in 1959/60 was helpful to me in this.

Why was that?
It was helpful in exposing me to a more realistic kind of macroeconomics than what I would otherwise have been aware of. RAND had a real-life mission of making sure that air force bases would always have spare parts on hand. In order to deal with that, I began to learn a little bit of probability theory and a little bit of modeling of stochastic processes. That was a leg up. And I also read my Tom Schelling. In the reciprocal fear of surprise attack, country A doesn't know what country B is up to, and country B can't observe what country A is up to. That kind of thing was very much on my mind. It connected with the fact that Fellner had communicated to me a deep sense of the radical uncertainty in which business people find themselves. They do not know what the probabilities are. So it's all nonsense to assume that we can predict anything. If at all, we have to use probabilities in a modified way. Summing up, it was thanks a bit to the RAND Corporation, a bit to Fellner, a bit to Schelling, that these things came together and helped me find my way.

In which year did you write your dissertation?
In 1959. In 1957/58, I was also at Yale, but I was getting nowhere. I was trying to find a good thesis topic, but I wasn't really successful at it.

Who was your adviser?
It was Tobin. But he had a sabbatical and spent a year in Switzerland. Which was good in a way, because I was on my own, shopping around for topics,

talking with Tom Schelling a little bit. And then I tried to work on one topic that Schelling had suggested but I didn't have the mathematical practice to know how I should have approached it. It wasn't mathematically difficult, but I just bit off more than I could chew. A year later, I had learned that lesson.

OK, back to your year at the RAND Corporation.
At RAND, I also learned a little bit of management science and operations research. It was fascinating, I was glad to have gone out there and met all sorts of interesting and stimulating and brilliant people. But after about six months, I knew that I had to get back into academia. Then, as luck would have it, I found myself right back at Yale. That was because the offer at Yale was a position at the Cowles Foundation,[28] which meant I'd have reduced teaching. So I took it, even though I wasn't very happy about being back in New Haven. It's a very boring place.

But intellectually, it wasn't boring.
Oh no. I was at Yale during its golden age.

And now you had a good degree of freedom there to think and not teach too much.
Yes, I did. No complaints.

How was interaction with your colleagues? Was that an environment that fostered your creativity? What directed the topics you were concentrating on?
In part because I was at the Cowles Foundation, it was terrifically easy – too easy – to interact with people. The consequence was that I wrote some co-authored papers that I shouldn't have been doing, but I did them. I probably had a tendency not to think too much about what my legacy was going to be in economics but rather about what I had to do to get from this page to the end of this paper. It was only after four years at Yale, one year of which was spent at MIT [1962/63], that I started to think more strategically. I had done very much in growth economics. I had written two papers that I think of as breakthroughs in growth economics, one on the role of managers in the diffusion of new technologies over the economy,[29] and another on research and development and their relation to economic growth.[30] As I was then moving toward my full professorship at the University of Pennsylvania,[31] for which I received the offer in 1965, I thought I could afford to have a shot at something more ambitious, and I began to work on the micro–macro thing. But it's very hard to get away from the papers that are already in the pipeline. It was only six months later when I had my own accumulated sabbatical in my last semester at Yale and I went overseas, to London, that I really got started.

Did you find a good work environment there?
Well, I went to see the Convenor of the London School of Economics. He asked me whether I was going to work a lot. I gave the wrong answer. I said: no. I should have said: of course I will be working my poor head off, that's what I do all the time. The effect was that he didn't give me an office. He gave me essentially a key to a locker in the library. This turned out to be great. If I had had an office, I would have talked with Richard Caves and Ronald Jones all day. And I would have gotten nothing done. With this locker, I was completely alone. Even if I wanted to talk I couldn't, because I was in room Q of the library and would have disturbed the others with my chit-chat. It was like being in a monk's cell.

How effective was that?
Very. I remember being at the opera one night to hear Wagner's *Parsifal*, with the great Jon Vickers in the title role. But there was a problem bothering me all day that I had not been able to solve and it was completely distracting me from the opera. There I was, in the front row of Covent Garden, the Royal Opera House, maybe 50 feet from Vickers, and I was not even listening, I was thinking only about that problem. But then, at the end of the first act, I realized what the solution was! I was able to concentrate on the music again after that. I'm just telling this story because it is an illustration that what I was doing was very consuming. I must have been almost impossible to live with.

You were probably told so.
No comment (*laughter*). But that was a difficult time. And then, that summer, I went on to Cambridge, England. I'd sit there in solitary confinement in an office provided to me by Richard Stone, head of the department of applied economics, later to win the Nobel Prize.[32] I'd sit there all day looking at the wall, trying to get through the next step in my work. The only bright moment would come when a young woman, I think her name was Rosie, would come in and give me my mail – if I had any. That was the high point of the day.

Sounds terrible. Did you need that monk's cell atmosphere in order to be productive?
Yes, I needed it. Now I am not sure that I would do something like that again. It's too big a price to pay. It's probably for this very reason more than any other that breakthroughs tend to be produced by young people. Now it would be very difficult for me to detach myself to that point, and to shut the world out and just do that. There were days back then when it was a bit difficult not to go crazy, you know. But I didn't.

In your memoir,[33] *you show that you had sort of three different phases in your work, and in each one you had one major idea. Interestingly, you point out that new ideas came up every time you moved. Do you think there is a correlation?*
Frankly, I can't tell whether I had new ideas because I changed places or whether the two just coincidentally happened at the same time. The causation is not clear. It is true that I always came to a new start after I moved. But I might have had a new idea anyway. You see, you work on something for six or seven years, and then you move on to a new topic – and I also was restless geographically, and after I had been in one place for a number of years, I moved on. At Columbia, I didn't work on just one thing, just because I didn't have the good fortune of moving any more. I did change (*laughter*).

Let's concentrate on a line of research that you started in Cambridge and completed at Penn. It dealt with the Phillips curve.[34] *In those papers, you were challenging the alleged relationship between inflation and unemployment, arguing that inflation also depends on the expectations of firms and employees about price and wage increases.*[35] *Without coming up with as highly a formalized model, and particularly without integrating micro and macro the way you did, Milton Friedman*[36] *had a similar point.*[37] *How popular was it in those days to challenge the Phillips curve? Was the Phillips curve controversial at all? Was there a wave of rebellion against it? Or did you face a lot of criticism as you dethroned it?*
Well, there is no question about it that while criticizing the Phillips curve I also had to take into account the grain of truth I thought it had. I couldn't really escape dealing with it in more detail. It wasn't controversial in academic circles, it was rather quickly accepted. I was one of the few who thought that it wasn't good enough and that we shouldn't let this stand as it was.

Did you have any interaction with Friedman on that issue?
No, none whatsoever. I thought that Milton Friedman's piece was a quick and dirty pass at the problem, one that really didn't deserve the vastness of attention that it got. What I tried to do was model the relation of unemployment to inflation, while at bottom, Friedman was more talking about labor force participation in relation to inflation, bringing in misperception about the real wage and consequent impact on the labor supply, or rather the labor supply curve. I took the labor supply curve as given throughout, however. I was talking about the relationship of the unemployment rate to these monetary disturbances.

What was the reaction in academia?
Yes, this was a big deal in the profession, a tremendous brouhaha. It sure was. When my two decisive papers came out in 1967 and 1968,[38] both of

them essentially in the summer, I was 34 and 35 years of age, and I was being attacked by the most senior and most admired figures in the profession. I was made fun of. Some of them still remained my friends at some gut level, but it was a very serious competition for academic status, and for the truth. It was a rivalry about who was to be regarded as the one who had understood this right. It was a huge topic in academia.

Who were the people you had the biggest fights with?
Jim Tobin and Bob Solow. They were dug in deeply in the Keynesian perspective, and so for them, it was a battle that they didn't want to lose. Paul Samuelson was more like a neutral bystander, a referee. He could go either way pretty happily.

How did this go on? It seems to me that your insights are so plausible and so hard to refute that people like Bob Solow might have given in at some point.
Well, there is a tendency of people to grow silent after they have made their case.[39] Right through the seventies, there was this bone of contention. And then, the entry of Robert Lucas[40] into the situation further complicated things. I have never been a rational expectations advocate, but like almost every economic theorist, I have dabbled in it from time to time. To some extent, the battle then became to be one between the Keynesians versus Lucas, and I was actually bypassed. I felt, however, that the battle should have been between me and the Keynesians! I didn't think that the Lucas variant was the important thing to focus on. Nor did I think that it had a great deal of utility. There is something there that was undeniably interesting, but it was only a simplified version of what I was doing. It was one of many possible simplifications. It was kind of inevitable because people do like simplicity. But it prolonged the debate, because it afforded the Keynesians with the illusion of a victory. They found Lucas' version absurd because expectations are in fact not rational, and so they thought they had won after all. But what about me? To get to the truth, you sometimes have got to be complicated.

Right in the middle of your work on the Phillips curve, in 1969, you went to the Center for the Advanced Study in the Behavioral Sciences (CASBS) at Stanford for a year. You met John Rawls[41] there. Did that encounter give you an impulse for something new?
I had met Amartya Sen[42] in New York when he was working at the United Nations. When I told him I was going out to the CASBS at Stanford, he said that John Rawls was going to be out there, too, and that I absolutely had to meet him. At that time, I wasn't quite aware of who John Rawls was. When I was a graduate student, I still had tried to pick up a little bit of philosophy. I used to go to the reading room of Sterling memorial

library in the morning. I would start with the letter A, and look at all the journals, and then go to the letter B, etc. It would take about three months until I reached the letter Z, and then I would start all over again. This means that I did know a little bit of philosophy in those days, but I wasn't reading systematically. And so ten years later, even though Rawls had in the meantime become a very important figure, I didn't know who he was. I glanced at one or two of his papers and realized this probably was something I should pay attention to. So I made a point of arriving at the Center a couple of days earlier than I might otherwise have done. I very quickly went to the Center and picked up my room ahead of most others. I saw that Rawls was there in one flight of offices. At the end, next to Rawls, there was a vacancy. I chose that office, next to him. We hit it off very well. We became good friends. He was significantly older than I was. I was 36, he was 48. Rawls had a very important influence on me.

Did he make you read parts or drafts of his Theory of Justice?
He didn't at first. But there was no doubt I was going to read some of it sooner or later. It was just that my own book was taking much, much, much longer than I had thought it would. I was kind of depressed about that. The book I was writing was later called *Inflation Policy and Unemployment Theory*.[43] It was a take-off on a whole series of books by Alvin Hansen. I had gotten a grant from the Brookings Institution to do it. I had been supposed to do it the previous year. I had gotten the money, but I had no book. When I got to CASBS, I had to start on that book even though that was not what I wanted to do with my year there. After ten or eleven months, I was still not quite done with it. The year had gone by, and I hadn't really taken as much advantage of the place as I had wanted and hoped to do. But I had a number of conversations with Rawls, and I listened to his talk very carefully. He listened to my talk, too. He liked me a lot. He told others in his field that 'Ned Phelps is different than the other economists'. I don't know what exactly he meant by that, but it was positive. At first, I didn't understand his book correctly. I even thought I had hit upon a better way of doing it. But then I realized that he wanted nothing to do with that way of doing it, and that the point of the whole book was to do it a different way. I didn't really master what he was doing until the book finally came out in January 1971.

Those papers that you later wrote about the structure of tax rates in a market economy, finding that the marginal tax rate on the top income from labor must be zero, were inspired by Rawls, weren't they?
Yes, absolutely. Those papers were an exploration of the implications of the Rawlsian maximin criterion. When I came back from the CASBS, I

cleaned up a few things around my desk, and I said to myself I was going to have a Rawlsian period now. The paper that you are referring to came out in 1973, and I wrote it in 1971/72.[44] It was one of the first papers I wrote of that kind. After my year in Stanford, I came back to Penn, but I was getting divorced and wanted to go and live in New York. I met Kelvin Lancaster on the train. He was the chairman of Columbia. That's how I came to Columbia in 1971. The deal was done very quickly.

Why New York?
Well, it's the most exciting city in the US. I had spent four years in the countryside in Massachusetts; I had spent four years in Connecticut, and then again another four and a half years after that; a year in Los Angeles; a year in Boston, visiting at MIT; a year in Palo Alto. . . And I had grown up right outside of New York. So it seemed very natural to want to be back to the number one city.

But it's a noisy city, full of distractions. And you said earlier that you needed a monkish atmosphere in order to be productive.
It's true. There were lots of nights when my wife, Viviana, and I would be up till midnight watching the last act of the opera at the Metropolitan, then, when we finally got home, I had to walk the dog, getting to sleep at one o'clock. There were days where I thought this was a hardship post, this New York City. Poor me (*laughter*). On the other hand, it's nice to have this excitement. I don't know that it's actually bad for my work. And it was good to get back to the financial capital of the country, and to see a lot of economists of various types.

Those were the seventies. What was your focus in the eighties? You've described those years as 'a period of synthesis'. The second part of that decade then was the beginning of your 'European years', with many trips to various places on the European continent.[45]
My non-monetary modeling of employment determination and the underlying time patterns, beginning with the Fitoussi–Phelps book which germinated in 1986[46] and which ultimately resulted in my book *Structural Slumps* in 1994,[47] was for me a very satisfying period. I also liked very much what came out. There are parts of the book that are a mess which could be cleaned up now, but I'm not sure I'll ever get around to doing it. I understand now why it wasn't terribly well received by large elements of the economics profession. And that's because it is somewhat strange in that I suppose that some shock comes out of the blue, and nobody has ever conceived of that shock before, and now that the shock has arrived, it is permanent; nobody ever can imagine that a future will be any different.

And then later, some other damn thing comes along. I have expectations being correct after the shock, but each shock always comes completely out of the blue. It may be a little bit of a strain, but for practical purposes, it's useful, because that is probably the way that most policy economists actually think. I'm very happy about that book because I feel there are a lot of things I understand about the world economy that others won't understand until they get their heads into that book.

What exactly was it that people didn't like?
Some of the rational expectations people would probably say that it's true that each shock may have a different name on it, but then a good theory should be one that takes into account the stochastic properties of that process of shocks. I just don't have any interest in that point of view. That makes me an outsider to this little industry that wants to model everything as a known stochastic process. This is part of a huge war that has been going on between me, and on my side Roman Frydman, and some others from time to time, not a large group – and on the other side Robert Lucas, Robert Barro, Thomas Sargent, and a huge crowd of people. These fights have been a very big part of my career. I don't want to dramatize 'my struggle', but first there was the battle with the Keynesians, then the battle with the rational expectations crowd. It would be melodramatic, though, to say that this has been a huge drain on me. Also, I haven't been overworked by the university system. There were times where I had a pretty large teaching load, but it's never been bone-crushing. There were many, many years where I had basically light teaching and a lot of support from the National Science Foundation. Right now, I have some very generous support by the Kauffmann Foundation. But still, these professional battles have taken a big chunk out of my time.

Were those fights inspiring or frustrating?
These two struggles with the Keynesians and rational expectationalists just consumed a lot of my research time over the years. The Frydman–Phelps conference volume attacking rational expectations,[48] for example, was to some extent an attempt to defend my earlier work in the late 1960s against later criticism by the rational expectations people that I was being pre-scientific.

That must have triggered something like a desire to move on again.
Oh, it did. It's been a nice thing to have this new phase that I've been in ever since my time at the EBRD in 1992/93, since the fall of the Berlin Wall and all that, taking up the subject of capitalism. The first thing that I worked on was inclusion, about how to integrate the disadvantaged into

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the system. And then, as I was winding up my book *Rewarding Work*,[49] I realized that I was saying a lot of things about capitalism that nobody had said before: that it was actually fun to work in a capitalist economy. I'm not sure that one could have said this in the nineteenth century, but times have changed to some extent. After the book was out, I began to think more and more about this issue. Basically, what I am saying about capitalism is that it has dynamism. What I mean is not just that the GDP grows rapidly. GDP also grew rapidly under Lenin and Stalin. What I mean by dynamism is innovating in commercially successful directions. Discovering this field has been a kind of liberation for me. Speaking of meeting people on a train, I once was traveling back from California on a plane from John Wayne airport. The seat next to me was empty, and I was very relieved about it. But just before the door closed, somebody showed up, and that was Bob Shiller from Yale. He was pumping me about my work, and I distilled the whole thing, beginning with Aristotle. I just hoped he wouldn't steal it all, only one third of it. At one point he said: 'Gee, you're the only guy in the whole profession who is working on the core part of our subject'.

He was right!
It's on capitalism, dynamism, innovation, growth, and on what a good economy is, what the good life is. And it has been a tremendous liberation because I don't have to battle with Keynesians and the rational expectationalists any more. I'm just all by myself.

Won't there be new battles?
Of course, I will provoke people and they will attack me. So in the end, I will have to defend myself once more. But at least there won't be huge armies any more. Well, you never know. Maybe that's the way it will be.

This is also the core of what you're concentrating on at the 'Center on Capitalism and Society' that you're directing here at Columbia ever since 2001.
Right. We're still pretty small, though. It's very important that we have our annual conference, where we can show our face to the world, talk to each other and present our views. It's a very important operation, and there is nothing else out there. So we have to exist, it's very important.

Why is it so important? What do we need to find out about capitalism? What is it that we are failing to realize?
At bottom, all the discussions of political economy and economic policy have been conditioned by neoclassical economic theory. There are people

on the right who advocate a minimalist government and minimalist regulation; the *Wall Street Journal* would write about wealth accumulation of the economy as the be-all and end-all of the economy; others would talk about the GDP in worshipful terms. For me, all of this lacks appeal. This is not a very helpful way to look at what the economy actually is. And it is also not helpful in telling us the most important things we have to do in order to make the economy better. But the left wing also, for the most part, only looks at the economy in a very neoclassical way. People on the left are concerned with inequality, they are worried about unemployment. But the only role they see in jobs is just that it is cheaper for society to provide people with consumer goods in return for working than attempting to do that without any work at all. The left celebrates employment, but there is mainly nothing philosophical about it, it is simply about getting everybody working in order to grind out more consumer goods. And that gives the government more wherewithals with which to throw goods at disadvantaged people, that is, the elderly, the sick and the tired. I found that was a pretty uninspiring view of the economy, too. The typical left-wing piece would be John Maynard Keynes' famous essay 'Economic possibilities for our grandchildren'[50] in which he says that there will come a time when the horrors of the workplace will be a thing of the past because we won't need to work any more to have the incomes that will permit us to pursue the arts and all that . . . So we must do all we can to speed up economic growth so that we get a little quicker to this bliss level in which the commercial economy and the profit motive and all that fully serve their purpose. There is all sorts of absolutely crazy stuff out there that provides the core of conventional thinking in the area of political economy. I decided I had nothing to lose by attacking all that.

And how did you do that?
I developed the concept of what I called economic dynamism to understand its social benefits. This began in the years immediately after completing *Rewarding Work* – with interruptions. First, in the end of 1996, I got a phone call from Luigi Paganetto, the architect of the economics department in the new branch of the University of Rome, called Tor Vergata. He told me that he could get me appointed as the senior scientific adviser to a project on 'Italy in Europe'. I said I would do this, but only if I could write predominantly about dynamism. It was okay with him. So I had a little project going there at the University of Rome, with six or seven young people who would meet with me two or three times a year. I had to write a chapter on Italy every six months for three years. During this period, I began to form my thoughts about what was wrong with the Italian economy. I could see the lethargy, I could feel the stultification of

the workplace, I could see the emptiness in the faces of the young people as they wait to be age 35 to get married and to begin counting the days until they would reach retirement age 55. So I began to criticize Italy as suffering from a dearth of dynamism, and I tried to understand the origins of that. This of course also led me to think about France. At that time, Germany wasn't yet sputtering down the way it did later, and I wasn't so much aware of the problems in Germany at that time. I became well aware of it later, especially in October 2003, when I gave my first big blast of a paper on Europe at a CESifo[51] conference.

What about Germany?
Germany is actually my paradigm case of tragedy. Germany is a country in which things were going on in an unspectacular way until about 1860. And then, the land we now call Germany took off. Productivity began rising rapidly. There were all kinds of innovations going on, some of them very famous ones. It was an incredible golden age for Germany, and it was to some extent emblematic of what was happening over all of continental Europe and the UK, too. It was very much tied up with the rise of finance capitalism. This was a period in which Europe made the transition from a traditional economy with known stochastic processes to a system that is evolving and always transforming itself according to unknown processes in unpredictable ways. By the way, I made this the topic of my Nobel Prize Lecture.[52] Progress in economic theory in the twentieth century has consisted of small steps towards getting away from the economics of the traditional economy to the economics of a modern economy.

How comparable did you find Italy, France and Germany?
Well, as I tried to get at the roots of the problem, of course I found that the task wasn't easy because each one has its distinctive features. Generally speaking, I attacked the view that it's simply the welfare state and high tax rates. I took advantage of the fact that people said that we are making a theoretical error if we're holding constant private wealth when discussing the consequences of a decline of the after-tax wage owing to the tax burden of paying for the welfare state. I argued that after the fall of the after-tax wage caused by the higher tax rates to pay for the welfare state, private saving will fall in response, not just private consumption. So private wealth will go on falling and falling until finally it has come back to the same ratio of after-tax wage as the one it had before. At that point, however, you can't say that people aren't working because the after-tax wage is low. It's still higher than it was in the eighteenth century. So that doesn't make any sense. The only thing you can say is that the benefits themselves of the welfare state erode incentives to work and to be a good employee. But

then, how important is that? There are some countries such as Denmark and Sweden where they have pretty good sized welfare states, and they don't have high unemployment rates. So I felt fortified that I could make a case that the problem was not primarily the social model. What was more important was the economic model. I argued that what was wrong with the economic models on the continent was that the economies were not structured in such a way as to generate as much dynamism as some other economies possessed, such as the US and Canada, especially. That's where I am now.

What are you working on right now?
I'm seriously thinking about writing another book on the subject, even though I swore not to. It's just too draining, and there is too little reward for it. As I said, *Rewarding Work* wasn't very well received, and it didn't even get a review in the *New York Times*. It didn't sell very well either. And then there was a conference volume coming out of the 1998 conference at the Russell Sage Foundation.[53] I don't remember what the sales figures are exactly, but it's nothing. And that was hard work! We slaved over this for months. Then I put together my six reports for the Italian Science Foundation. I didn't want it to be a big deal.[54] It's almost hidden from view. Actually, I never had a bestseller. To be true, I never even tried. Actually, I had thought that my textbook *Political Economy*[55] might catch up. But that didn't happen.

What will be the topic of your next book if you do write it? Dynamism?
The working title that is on my mind is 'The Good Economy'. 'Dynamism' would be not bad either. Maybe there is some way of using that in the subtitle. 'How the West Found Dynamism and Lost It'? No? Well, something like that. I always spend a lot of time thinking about titles and subtitles. The problem is that ever since I've received the Nobel Prize, I don't have so much time. I'm now really drawing on what I've done before. I don't like that.

Thank you, Ned. Thank you, Professor.

NOTES

1. The interview was held on 1 August, 2007.
2. The Ewing Marion Kauffman Foundation was established in 1966 to support education and entrepreneurship for young people, particularly in the founder's home city of Kansas City and the surrounding area (www.kauffman.org).
3. www.earthinstitute.columbia.edu/ccs/people.html.
4. Edmund S. Phelps (2006/2007a).

5. See www.bepress.com/cas.
6. Edmund S. Phelps (1995).
7. Edmund S. Phelps (1961).
8. Edmund S. Phelps (1966a).
9. Edmund S. Phelps and Richard R. Nelson (1966).
10. Edmund S. Phelps (1966b).
11. As a corroboration of this point, but also for a more detailed evaluation of Phelps's contributions in general, it is worthwhile to read Philippe Aghion et al. (2001). The generation and diffusion of knowledge is a topic that occupies Phelps's mind until this day, entering also his more recent interest in capitalism.
12. Alban W. Phillips (1958).
13. Edmund S. Phelps (1967 and 1968). A third paper was an extension of the latter: Edmund S. Phelps (1970).
14. Milton Friedman (1968).
15. Robert E. Lucas (1972).
16. Edmund S. Phelps and John B. Taylor (1977).
17. Edmund S. Phelps and Jean-Paul Fitoussi (1988).
18. Edmund S. Phelps (1994).
19. Edmund S. Phelps (1997 and 2003).
20. See the interview with Robert M. Solow in this volume.
21. Paul A. Samuelson (1948). Now in its eighteenth edition, the book has in recent years (after 1985) been co-authored by William Nordhaus. The book has in total sold over 4-million copies.
22. Edmund S. Phelps (1995).
23. James Tobin (1918–2002) was awarded the Nobel Prize in 1981 'for his analysis of financial markets and their relations to expenditure decisions, employment, production and prices'.
24. Lawrence (Larry) Summers, himself an economist, was the President of Harvard University. He resigned in 2006 and is now one of Harvard's select university Professors.
25. Thomas Schelling received the Nobel Prize together with Robert Aumann in 2005 'for having enhanced our understanding of conflict and cooperation through game-theory analysis'.
26. Thomas Schelling (1959).
27. The RAND Corporation is a research institution created originally in 1946 by the United States Army Air Forces, under contract to the Douglas Aircraft Company. In 1948, it became an independent non-profit organization, sponsored initially by the Ford Foundation, to 'further promote scientific, educational, and charitable purposes, all for the public welfare and security of the United States of America'.
28. The Cowles Foundation is an economics research institute, founded in 1932 by the businessman Alfred Cowles under the name 'Cowles Commission for Research in Economics'. The Cowles Commission moved to the University of Chicago in 1939 and later on to Yale University in 1995, where it was renamed the 'Cowles Foundation'. The motto of the Cowles Foundation is 'Science is Measurement', and thus it has been dedicated to linking economics, mathematics and statistics.
29. Edmund S. Phelps and Richard R. Nelson (1966).
30. Edmund S. Phelps (1966b).
31. The appointment at Penn began a year later, in 1966. Edmund Phelps stayed there until 1971, when he moved on to Columbia University.
32. Richard Stone (1913–91) was awarded the Nobel Prize in 1984 'for having made fundamental contributions to the development of systems of national accounts and hence greatly improved the basis for empirical economic analysis'.
33. Edmund S. Phelps (1995).
34. Alban W. Phillips (1958). The paper ultimately implies that there is a trade-off between unemployment and inflation that policy makers can take advantage of.

35. Edmund S. Phelps (1967 and 1968a). A third paper was an extension of the latter: Edmund S. Phelps (1970). This conference volume is actually popularly dubbed 'the Phelps volume'. Phelps's model became known as the 'expectations-augmented Phillips curve'. This model focuses on the wage-setting behavior of firms in a labor market in which matching the unemployed with vacant jobs is a time-consuming process. It says that for a given unemployment rate a one percentage point increase in expected inflation leads to a one percentage point increase in actual inflation. So in the end, there is something like a 'vertical' Phillips curve, suggesting that there is no long-run trade-off between inflation and unemployment, as there can be no permanent discrepancy between actual and expected inflation.
36. Milton Friedman (1912–2006) was awarded the Nobel Prize in 1976 'for his achievements in the fields of consumption analysis, monetary history and theory, and for his demonstration of the complexity of stabilization policy'.
37. Milton Friedman (1968).
38. See note 35.
39. Actually, Robert M. Solow never agreed. In an interview with *The Region*, recorded in September 2002, he says: 'What replaced the initial Phillips curve was the Friedman–Phelps natural rate of unemployment long-run vertical Phillips curve. And I have never, from the very first day, thought that that was other than a flimsy theory supported by flimsy empirical analysis.' Available at http://minneapolisfed.org/publs/region/02-09/solow.cfm.
40. Robert Lucas was awarded the Nobel Prize in 1995 'for having developed and applied the hypothesis of rational expectations, and thereby having transformed macroeconomic analysis and deepened our understanding of economic policy'.
41. John Rawls is famous for his 'Theory of Justice' which he worked on while he was at Stanford (John Rawls, 1971).
42. Amartya Sen was awarded the Nobel Prize in 1998 'for his contributions to welfare economics'.
43. Edmund S. Phelps (1972a).
44. Edmund S. Phelps (1973a) (reprinted in E.S. Phelps (ed.), 1974).
45. Edmund S. Phelps (1995, p. 104).
46. Edmund S. Phelps and Jean-Paul Fitoussi (1988).
47. Edmund S. Phelps (1994).
48. That conference was held in 1981. Roman Frydman and Edmund S. Phelps (eds) (1983).
49. Edmund S. Phelps (1997).
50. John Maynard Keynes (1930).
51. CESifo (Center for Economic Studies/Institut für Wirtschaftsforschung) is an economic research institute in Munich (www.ifo.de).
52. Edmund S. Phelps (2006/2007a).
53. Edmund S. Phelps (ed.) (2003).
54. Edmund S. Phelps (2002).
55. Edmund S. Phelps (1985).