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Interview with Edmund S. Phelps

Howard R. Vane and Chris Mulhearn

Edmund S. Phelps has been McVickar Professor of Political Economy at Columbia University in New York City, New York, since 1982 and director of the Center on Capitalism and Society at Columbia University's Earth Institute since 2001. In 2006, he was awarded the Nobel Memorial Prize in Economic Science "for his analysis of intertemporal tradeoffs in macroeconomic policy." We interviewed Professor Phelps at his hotel in San Francisco, on January 3, 2009, while attending the annual meeting of the Allied Social Science Associations.

Background

VANE and MULHEARN: As an undergraduate you entered Amherst College without having chosen a predetermined course of study. What led you to major in economics at Amherst?

PHELPS: I was winding up my freshman year, and I was kind of entranced by the philosophy course I'd taken in the second semester and a related course that I'd also had to take. So I was getting interested in philosophy when my father asked me whether I would do just one favor for him, and that was to take a course in economics. I wasn't terrifically enthused about the idea, but I took the course in my second year, and within two weeks I could see that I had a real talent for it. I found that economics was just as interesting as philosophy but that it also had this practical side. I didn't fully understand the subject, so I decided to study it further. I kept failing to understand the relationship of microeconomics to macroeconomics [*laughter*]. I like to joke that I went to graduate school to get to the bottom of this relationship. However, after four or five years of graduate school, I realised that I still didn't have the answer, and so I decided: very well, I'm going to have to try to solve this problem myself.

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VANE and MULHEARN: Having graduated from Amherst College with a BA in 1955, you undertook postgraduate studies at Yale University, from where you were awarded your doctorate in 1959. Which faculty members at Yale stood out as being particularly influential or inspirational?

PHELPS: I'm not sure how exactly I would rank them, but probably William Fellner and James Tobin were two of the most influential, but I was also extremely impressed by Thomas Schelling, from whom I took a course and who I got to know a bit outside the classroom. Tjalling Koopmans, too. There were others with whom I had interactions: Henry Wallace and Robert Triffin, who in his own way was rather impressive. That was an incredible period at Yale; it was really Yale's golden age in economics. And I'm not even counting Gerard Debreu, who was there for a couple of years, and Jacob Marshak, who left after my first year. It was an incredible period.

Growth Theory

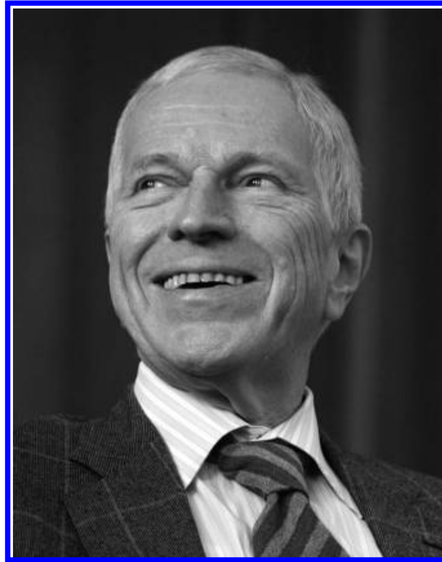
VANE and MULHEARN: After working for a year as an economist at the RAND Corporation in Santa Monica you returned to Yale in 1960 where you remained—with the exception of a year at MIT (1962–63)—until 1966. In this early phase of your academic career, the main focus of your research concerned growth theory. What attracted you to this field of study? To what extent were you inspired by the seminal contributions of Robert Solow (1956, 1957) and Trevor Swan (1956)?[1]

PHELPS: Without doubt, I was impressed by the papers by Solow and Swan, and I was fortunate enough to get to know Robert Solow in the year I spent at MIT. When I got back to Yale, there was Trevor Swan hanging around for a year. So I became fascinated by that growth model: exploring it, extending it, maybe even modifying it. But also it was at a time in the history of the U.S. when there was a concern about the adequacy of the growth rate. It seemed to us that the growth of potential output was declining slowly over the 1950s and early '60s. I've no idea what the revised data would show now, but that was our impression at the time. And so, just as a substantive matter, even if there hadn't been a Solow or a Swan, I might very well have gotten interested in the contributions of tangible capital investment, education, and research and development to economic growth.

VANE and MULHEARN: Your best-known paper on growth theory, "The Golden Rule of Accumulation: A Fable for Growthmen," was published in the *American Economic Review* in 1961 (see also Phelps, 1965). Were you surprised that you were able to make such an important contribution to the growth literature so early on in your academic career?

PHELPS: I was far from being the most precocious or the most talented person in my generation. Robert Mundell was writing far more profound papers in 1960

[1] Robert Solow (1956, 1957) and Trevor Swan (1956) independently developed the neoclassical growth model, which fundamentally changed the way economists approach the study of economic growth. The Solow–Swan model is built around the neoclassical aggregate production function—which is assumed to exhibit constant returns to scale, diminishing returns to factor inputs, and smooth elasticity of substitution between factor inputs—and focuses on the proximate causes of economic growth.



Edmund S. Phelps

and '61, and Amartya Sen also wrote a wonderful paper in the *Economic Journal* that was a favourite of mine ("On Optimising the Rate of Saving," 1961). I was working hard to make a mark but I don't think my papers on the whole in the early years were as deep and original as those by Mundell and Sen. I guess I'll let it go at that. Maybe I could add Marc Nerlove and perhaps others. But let me just elaborate: I was very conscious that to some extent I was just improving my skills and building my set of tools. There were a lot of things that I hadn't studied at Yale; it all went by rather quickly—I'd taken a rather eccentric set of courses, and there were things I didn't know much about. During my first year at the RAND Corporation, I spent an ungodly amount of time after lunch in the library looking at management science and operational research journals, trying to get a better sense of how those mathematical models worked. So I think this period was to some extent an apprenticeship—an investment in building my toolbox of concepts and methods.

VANE and MULHEARN: In addition to the golden rule of accumulation, which Peter Howitt (2007) has suggested "remains the most basic proposition of optimal growth theory,"[2] you also have made a number of other important contributions to the study of economic growth. In particular, we have in mind here four very influential papers.

PHELPS: Four! Maybe I could get to three on a good day [*laughter*].

VANE and MULHEARN: Well, in chronological order of publication there is, first, your 1966 *Review of Economic Studies* paper in which you derived a "golden rule

[2] As Howitt (2007) described Phelps (1961), the "golden rule of accumulation . . . requires that the country's saving rate equal the fraction of national income accruing to owners of capital. This same paper showed that under ideal conditions the golden rule is mathematically equivalent to the condition that the rate of return to investing in capital be equal to the rate of economic growth."

of research," which finds that social welfare is maximized by "equating the rate of return from research to the growth rate," and which in turn suggests that a very high share of workers should be involved in research activities (Phelps, 1966).

PHELPS: That's a largely forgotten one; I like that paper.

VANE and MULHEARN: Second, your 1966 *American Economic Review* paper, co-written with Richard Nelson, where you considered the role of education in the process of technological diffusion and economic growth, pointing out, for example, that "the rate of return to education is greater the more technologically progressive is the economy" (Nelson and Phelps, 1966). Third, your *Canadian Journal of Economics* paper where you argue: "One can hardly imagine, I think, how poor we would be today were it not for the rapid population growth of the past to which we owe the enormous number of technological advances enjoyed today" (Phelps, 1968a, p. 511). Fourth, there is your 1968 *Review of Economic Studies* paper, co-written with Robert Pollak, in which you "investigate the optimal saving policy of an 'imperfectly altruistic' present generation under various assumptions about future saving behaviour and its control" (Phelps and Pollak, 1968). Which of these papers are you particularly proud of?

PHELPS: Well, it's like your four children . . . it's hard. I think I'd say that I felt a rush of pleasure most over the population increase paper. I felt it was terribly original and I had a lot of fun doing it. I did write my second-best-known sentence in that paper: If I could rewrite the demographics of the world, halving the population size at each point in time, I wouldn't do it for fear of losing Mozart.[3] The paper had some real influence. The provocateur Julian Simon wrote at least one book inspired by it (for example, Simon, 1981, 1986). Unfortunately he died at quite a young age; an interesting person, quite a maverick.

I think the most influential paper was possibly Nelson and Phelps (1966), but I can't say I'm proudest of it because a big part of it was Dick Nelson. I came in halfway and tried to change some elements that I thought I could improve. It had a big impact on my thinking, and of the four papers you mention, I feel pretty sure that it's the one that will live longest.

Inflation, Unemployment, and Structural Slumps

VANE and MULHEARN: In September 1966, you moved to the University of Pennsylvania to take up a tenured post as Professor of Economics.[4] At Penn you refocused your research efforts to investigate the relationship between inflation

[3] The exact quotation: "If I could re-do the history of the world, halving population size each year from the beginning of time on some random basis, I would not do it for fear of losing Mozart in the process" (Phelps, 1968a, p. 512).

[4] Those who have not yet won their own Nobel Prize may take some solace in the fact that Phelps's early academic career path was not completely smooth. With the exception of a year at MIT in 1962–63 as a visiting Associate Professor of Economics, from 1960 to 1966 he held research positions at Yale's Cowles Foundation and, in consequence, had a reduced teaching load first as an Assistant Professor of Economics (1960–62) and then as an Associate Professor of Economics (1963–66) at Yale University. Yale, however, failed to reward Phelps with promotion to tenure as a full professor, and following tenure

and unemployment, and in two seminal contributions (Phelps, 1967, 1968b), you challenged the conventional wisdom of a stable, negatively-sloped long-run Phillips curve relationship between inflation and unemployment.[5] Your analysis, together with that independently undertaken by Milton Friedman (1968), led to the expectations-augmented Phillips curve and the Phelps–Friedman natural rate hypothesis. Is it fair to say that the main difference between your and Friedman's analyses is that, unlike Friedman, you offered a theory of the natural rate of unemployment that was based on microeconomic foundations and which revolved around quit rates specified as a decreasing function of a firm's relative wage?

PHELPS: Well, that's probably the single most important difference. I'm sure that there's at least one more, and if I had a little time to think about it, I might be able to come up with some others. Of course one distinction arises from my emphasis on quitting, which is a concern for the firm because it costs money to prepare a new recruit for work and because there are hiring costs. It's also reasonable to assume that there are increasing marginal hiring core costs—that is, the faster you're hiring, the greater the cost of hiring one more. This means that when you increase your workforce, you don't do it in one gulp; you build it up gradually—which generates an equilibrium path of the unemployment rate. In this respect, it was a significantly more advanced thing than Milton Friedman was talking about, and naturally I felt good about the added feature. On the other hand, it made it harder to discuss my model. Everything went so simply for Friedman because he had such a straightforward framework, but I was stuck with one that was inherently a bit more complicated. Another difference, as you imply in your question, was that my model was not wage–price, it was wage–wage.[6] For simplicity I take the worker as supplying his or her labor perfectly inelastically, so the real wage is not an issue. The question is what sort of relative wage am I being offered here, could I do better by looking elsewhere? And that wasn't in Milton Friedman's thinking at all.

VANE and MULHEARN: Should this work, which brought expectations into the analysis of wage- and price-setting, be seen as part of a much broader research agenda in which you have sought to provide rigorous, choice-theoretic, microeconomic foundations for macroeconomics?

PHELPS: I didn't have any great foresight about where I was going from paper to paper. I had a succession of new ideas, and to some extent they built on one another. Originally, all my thinking was in terms of the labor market only, and

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offers at full rank in 1965 from both Northwestern University and the University of Pennsylvania, he moved to Penn in 1966.

[5] The original Phillips curve appeared to present the authorities with a menu for policy choice: specifically, policymakers could seemingly permanently reduce unemployment by expansionary aggregate demand policies but would have to accept a higher inflation rate; conversely, they could lower inflation by accepting a permanently higher unemployment rate.

[6] This terminology refers to a particular aspect of Phelps's model: that the quit rate depends on the relative wage and unemployment, not the real wage. If, for example, unemployment is driven below the equilibrium or natural rate, firms will face a big turnover problem. In this situation, every firm will raise its wages, trying to outpay all the other firms and increase its relative wage in an attempt to reduce the worker's quit rate. However, all firms will be disappointed as they fail to outpay the others.

essentially the supply of and demand for labor, to put it crudely. But then Sid Winter suggested we think about the corresponding issues in the product market: the problems of price setting and acquiring customers, which was analogous to the problem of wage setting and acquiring or retaining employees. That took me up to about 1969—the Phelps–Winter paper was written in 1968. We presented it at the Penn Conference in January 1969, and it was published in March 1970 (Phelps and Winter, 1970).

In 1969, I also talked about an economy consisting of islands in each of which there's an auction market for labor (Phelps, 1969). I never had in mind building a complete edifice or mansion that would constitute a wide-ranging microeconomic foundation for macroeconomics. But then later on when I was writing a book on inflation policy using some of the microeconomics that I'd developed in the 1960s—I'm referring to the 1972 book *Inflation Policy and Unemployment Theory*—I tried to widen my perspective a little. I tried to take into account labor unions and sociological aspects. But the next time I really worked on that was in the early 1980s when I was writing my textbook *Political Economy* (Phelps, 1985), and I realized that I was coming up to a chapter on capital markets and finance and I had absolutely nothing to say. This started me thinking about problems of imperfect information, and it occurred to me that, if a firm wanted to issue some shares, there might be no ready market: people might not want to buy because they might not have much of a clue as to what they would be getting an ownership claim to. I built up some material in that line. In the end, I was somewhat wide-ranging, but I never got into insurance markets or some of the things that people like Joe Stiglitz would work on.

VANE and MULHEARN: This research programme helped change the methodology of modern macroeconomics. Particularly influential in this respect is the volume on the *Microeconomic Foundations of Employment and Inflation Theory* (Phelps et al., 1970) that resulted from the conference you organized at Penn in January 1969. Can you explain the background to this conference and how you decided upon the 12 papers that make up the volume?

PHELPS: There wasn't much of a selection process, but let's start from the beginning. I'd noticed a paper from Rochester, maybe one from Carnegie-Mellon; perhaps I'd seen the Lucas–Rapping paper in draft somewhere. And then I heard that Dale Mortensen was doing some work. I began to catch wind of these developments at a conference at Montauk Point in August 1967 at which one of my commentators was Axel Leijonhufvud—Harry Johnson was also there—and it offered an opportunity to exchange information. It occurred to me that with the three or four papers I'd heard about or seen that it would make sense to bring these together in a conference at Penn.

Now, you asked how the papers were chosen—I chose them. There wasn't much of a selection process though. I included just about every paper that followed the kind of work we were interested in—imperfect information, the forming of expectations, and so forth. But my role was a little bit more than acting just as a mailbox because I actually precipitated some of the papers by inviting prospective authors to contribute and with Sid Winter I developed a model of customer markets (Phelps and Winter, 1970). I also contributed one myself; a revised version of my 1968 *Journal of Political Economy* paper.

VANE and MULHEARN: One indicator of the importance of the conference

is that your "island" parable, introduced in your 1969 *American Economic Review* paper (Phelps, 1969) and elaborated upon in the introduction you wrote to the *Microeconomic Foundations* volume (Phelps et al., 1970), was used by Robert Lucas in his seminal 1972 *Journal of Economic Theory* paper "Expectations and the Neutrality of Money" (Lucas, 1972). Following the conference did you correspond or talk with Lucas about the ideas that appeared in his paper?

PHELPS: I remember I was teaching an advanced seminar at Penn in 1970–71, probably in the spring semester, and I wanted Bob to give this paper at the seminar. I never really discussed the paper with him much. I think I needed some time to think about it. Even when it came out, it took me some time to read it carefully.

VANE and MULHEARN: In 1971, you moved to Columbia University, where you began to investigate a number of new issues. One of these involved a much-cited paper you co-wrote with John Taylor where you demonstrated the "Stabilising Powers of Monetary Policy under Rational Expectations" (Phelps and Taylor, 1977). Looking back, given the impact that new classical macroeconomics had made by this time, how significant was this paper in helping to reconstruct the Keynesian paradigm?

PHELPS: I think John went on to develop a model that was better suited to that purpose (see, for example, Taylor, 1979).[7] I also worked on a model of optimal disinflation conducted by the monetary authorities that had the same overlapping, nonsynchronous, staggered wage-setting feature. But the paper that John and I wrote was a kind of a learning exercise—we thought, let's just get our feet wet and see what's involved here. The model wasn't very close to anything I'd done before, although I had written a paper on inventories that was somewhat close to it. John probably had other ambitions, but my only aim in that paper was to try to convince the Chicago crowd that if some wages or prices are going to last not just for the current period but for a second period or more, then that makes a difference. It changes the dynamic response curves to shocks and slows down or gradualizes the return back to some medium-term unemployment rate. And these things are true even if you use rational expectations. Later I regretted writing that paper a little bit because it seemed to put me in the camp with the Chicago school. It might have been interpreted as showing that I had gone over to the Chicago school with its rational expectations. In fact, the rational expectations approach was not at all congenial to my way of thinking. It's clear from the *Microeconomic Foundations* volume (Phelps et al., 1970) and from my 1968 paper on money–wage dynamics that I'm not what Bob Mundell would call an equilibrium theorist. By that time, I was intrigued by expectational disequilibrium; I didn't intend at all to endorse rational expectations (see, for example, Frydman and Phelps, 1983).

VANE and MULHEARN: In your 1972 book, *Inflation Policy and Unemployment Theory,* you introduced the notion of hysteresis in the rate of unemployment, where the equilibrium or natural rate depends on past rates of unemployment. Do you consider this to be one of your most important and lasting contributions?

PHELPS: I thought it was a neat idea, an attractive idea, and I'm grateful that

[7] In Taylor's (1979) model, wage and price dynamics are influenced by staggered commitments as well as by expectations.

in the end it became more associated with me that with anyone else. However, you can't say that it's done all that well in the competition of the academic marketplace. There were some statistical tests which suggested at least to the people doing the tests that there wasn't anything that we could call hysteresis. Sure, if there's a massive slump of some length, then even if the economy gets to be humming again, you have to put the pieces back together, and it might take quite a long time to return to the same unemployment rate as before. But it was very hard, I think, to establish the unemployment rate that the economy tends to approach—there's going to be a different medium-term unemployment rate than what would have prevailed had the slump not occurred. It was very difficult to extract and confirm that proposition from the data. Also, people came along and said if you have a depression, people go to college and they wouldn't have gone without the depression, and they wouldn't have gone to graduate school. You have all these brilliant economists in the U.S., like Kenneth Arrow, who wouldn't be with us if it hadn't have been for the Great Depression—they probably would have been doing something else, but they couldn't get a job. I think a couple of people at MIT, Ricardo Caballero and Mohamed Hammour, wrote about the idea that recessions have a cleansing effect; everyone takes a breath and investment activity happens that there wasn't time for when unemployment was at a normal level (Caballero and Hammour, 1994). So it's an important subject, but I don't think that we can say that hysteresis has become an established truth.

VANE and MULHEARN: Many of your ideas have been central to the development of the new Keynesian school. They include the consequences of introducing incentive wage considerations, unsynchronized wage-setting, customer markets, and hysteresis effects into macro models. Which of these ideas are you most proud of?

PHELPS: I would have to say that the incentive wage idea is the most important. The reason is that this laid the foundation for thinking about unemployment as an involuntary phenomenon, rather than just the result of having to be on the boat between one island and another before you could get into the labor market in that new place. I thought that was a very important breakthrough, and it became even more important when Steven Salop, who'd been an undergraduate student of mine at Penn, pointed out that I could have rewritten that paper without money. He proceeded to set out a nonmonetary theory of the incentive wage and involuntary unemployment (Salop, 1979). This was followed by the paper by Shapiro and Stiglitz (1984) which was a shirking model of a nonmonetary type to get a nonmonetary theory of involuntary unemployment.

VANE and MULHEARN: In the 1980s, in collaboration with Jean-Paul Fitoussi, you developed what you have called a structuralist theory of unemployment to explain the slump in Europe (Fitoussi and Phelps, 1986, 1988). Building on this analysis in your 1994 book *Structural Slumps* (written in collaboration with Hoon, Kanaginis, and Zoega), you developed a theory of unemployment in which the equilibrium or natural rate of unemployment is endogenously determined by a variety of nonmonetary forces including the real rate of interest. Can you outline the main thrust of this analysis?

PHELPS: Sometime after Jean-Paul Fitoussi and I finished our book, I'd had a chance to reflect on the material quite a bit, and I came to be curious as to whether I could rewrite it in such a way that the models are nonmonetary—as if IBM shares were used as the medium of exchange or some very low-priced share like Citigroup. But when I started, I didn't have any vision of a 400-page book. I struggled with at least one paper, maybe two, on a two-sector model that was in the spirit of what Fitoussi and I did in one of our chapters—a model in which I left money out yet still had involuntary unemployment. I used the shirking model because that was more convenient. And then I thought why not do a nonmonetary version of the macro model that had customer markets in it? And a simple version of that came out in the *Quarterly Journal of Economics* (Phelps, 1992). That left the third model of the Fitoussi–Phelps book (Fitoussi and Phelps, 1988). The key feature here was the turnover training treatment of the labor market, which made it very much like my paper "Money–Wage Dynamics and Labour Market Equilibrium" (Phelps, 1968b). But I realized that this was going to be a more complicated business. I had a very good student in the doctoral programme at Columbia—Hian Teck Hoon—and he was looking around for dissertation topics, and I sent him off to think about this problem. I probably suggested that he wanted to set up a Hamiltonian maximization problem with shadow prices and whatnot. A few weeks later, he came back with a model, and we worked on it for quite a long time; it took a while to understand it all and to fashion arguments for all the steps so that it would actually be persuasive. It was published in the *American Economic Review* (Hoon and Phelps, 1992). And then it became easy to bundle these papers together, and I was able to write *Structural Slumps* (1994) in a year or a year and a half, even though it was 400 pages, because I had so much material already prepared.

I would have felt rewarded with this enterprise if the models ended up having the same implications as the Keynesian model. And I would still have shown something that at least interested me (I don't know whether it would have interested anyone else) that I could have a Keynesian model without bringing in money. Keynes had left us with the idea that money was at the very essence of his arguments. One of my teachers at Yale, James Tobin, was always insisting that an economy with money is fundamentally different to one without money. He thought that I was committing a terrible sin every time I took a nonmonetary model. That would have been reward enough. Some people thought that was precisely what happened—that all I'd done was to dress up the Keynesian argument in terms of a nonmonetary argument so to speak. But actually the reason I cared enough about that book to write 400 pages was because there were a lot of new implications about the way the economy worked and new things to think about with regard to fiscal policy.

VANE and MULHEARN: Does your analysis have implication for the present situation we find ourselves in, where a number of countries are planning to engage in fiscal stimulus financed by increases in public debt?

PHELPS: If one country were to increase the public debt even by a huge amount, then after the dust has settled, it may not have a big effect on the country's natural unemployment rate and likewise the labor force supply in the country. I

could see in my small open-economy model that could be the case. But if the whole world were to run up public debt, then that could have big effects in raising real-world interest rates and cause asset prices to plunge, which in turn would have effects on investment activity and then finally the natural rate of unemployment. I thought this was really important and worth bringing out in the book.

But there were also other public expenditure issues—I'm thinking of the two-sector model about which there was quite a lot in *Structural Slumps* (Phelps, 1994). There's a necessary distinction between public expenditures on the output of the consumer goods industries and public expenditures on the output of the capital goods sector. I saw that I was able to show easily that public expenditure on capital goods would pull up the prices of the consumer goods, causing suppliers to ramp up their output, and that would increase aggregate employment and reduce the natural rate of unemployment.

Without laboring the point, I thought there was a whole lot of stuff there that was quite novel, and I didn't think the book got nearly the attention it deserved. It was pretty nicely treated by the reviewers, including a nice review by Clive Crook in the *Economist* (Crook, 1994), but it wasn't taken seriously enough, and it took me a while to figure out why that was the case. I think, now, that I understand why it didn't go over very well at the time. The book is full of these thought experiments in which a parameter of the model changes: households decide that they're going to be less thrifty, or the population growth rate increases, or the government decides to get bigger. The rational expectationists would argue that these things are happening over all human history and what we need in order to be able to handle this is the notion of a regime. There are big government spending regimes and there are small government spending regimes, and there's the probability of the one and the probability of the other. It appears that the dice is thrown, and it may happen that the current regime continues for the coming period or a number of periods, but eventually there's a transition to a different regime, and that's the only satisfactory way of treating these changing structures of the economy. That kind of paper actually got written by one or two people in the present decade. My attitude, for what it's worth, is just that seems like an awful lot of paraphernalia in return for not gaining very much. It's still going to be the case that if you jump to a regime with more government spending, you're going to get the kind of effects I was talking about; you're going to have more equations and more complications to get there, but you're still going to get the same results, so why not be more pragmatic? Besides, we don't live in a stationary universe in which we're just bouncing from regime to regime; most of the unsettling things that happen have a feeling of novelty about them, they catch people completely by surprise. Yes, I think that book was unjustly understudied.

Fairness and Equality

VANE and MULHEARN: While you are best known for your pioneering contributions to the theory of economic growth, and employment and inflation

theory, you have also made important contributions to a number of other fields including labor economics (for example, "The Statistical Theory of Racism and Sexism," Phelps, 1972b) and public finance (for example, "Taxation of Wage Income for Economic Justice," Phelps, 1973). What sparked your interest in these areas?

PHELPS: There's no hidden agenda, no grand design; I'm just an opportunist [*laughter*]. I had what I thought was a pretty good idea about statistical discrimination. I tried to straighten it out in my mind and then write about it clearly for a page or two in my book, *Inflation Policy and Unemployment Theory* (1972a). Then I became aware that—I was at the Center for Advanced Study in the Behavioral Sciences, just up the hill from Stanford University—Kenneth Arrow was reported to have written something similar. I quickly prepared a paper for the *American Economic Review* (Phelps, 1972b) so that I wouldn't be scooped by Arrow. I think this was one of the few times I actually managed to out-maneuver somebody else [*laughter*]. Usually, I was always being out-maneuvered.

It's a hard paper for me to talk about. I thought it was interesting and of some social importance because its theme, which was somewhat tacit, was that discrimination is much harder to get rid of than we might think because there's a big rational component in it. A person is to some extent judged on the basis of the ethnic or other group to which he or she belongs, rather than on merit. And we don't have all the information we'd like, so we use information about membership of the group to fill in to some extent. If someone has grown up in a slum, we assume this person has acquired the bad habits that many people acquire when they grow up in a slum.

Social justice was a huge area of interest of mine, which was somewhat unfortunate because the papers were always really hard to write. They made a lot of technical demands. I didn't really have enough mathematics to do them with complete assurance that I was doing the right thing, although I think that on the whole I got the right answers, which was all I really cared about. But I think I'd like to develop this part of my answer at two levels.

One is that I came under the influence of John Rawls (1971). I was quite taken with his idea of maximin: that economic justice involves making the portion going to the person earning the least as high as it can be. I was just curious to see how that would play out in a mathematical model of taxation. At another level, I saw that Rawls was being portrayed in a distorted manner by people who wanted to use him for their causes. Some people wanted to use him as grist for their platform of egalitarianism, while I thought that what was interesting and especially distinctive about Rawls was that, yes he was in a sense an egalitarian, but he was paradoxically an egalitarian who was willing to tolerate a lot of inequality for the sake of those at the bottom of the heap. He was interested in the *absolute* rewards to the people at the bottom, not their relative rewards. I was fascinated when I stumbled on the result that the marginal tax rate on the last dollar of income of the highest earner should be zero because to leave it at some number above zero would mean foregoing the opportunity to cut a deal with that earner to work a little more in return for a cut in the marginal tax rate on the last dollar. In a way, I was showing people: don't think that with Rawls you're getting egalitarianism. If you've read his book, he's really saying that he wants to deploy incentives to increase the amount of tax revenue in order to have the largest amount

of funds possible for subsidies to lift up the contributors to the economy at the bottom of the heap. I thought I was serving to clarify things.

VANE and MULHEARN: In your 1997 book, *Rewarding Work,* you examined the issue of how to reduce joblessness and low pay among disadvantaged workers. In doing so you put forward the case for wage subsidies to employers of low-wage workers as a way to raise their pay and employment. Should your research in this area be seen as part of a broader interest in economic and social justice?

PHELPS: For sure I always had an interest in justice and fairness, even as a high school student and certainly in college. But Rawls (1971) was almost the only piece of work that was a systematic examination of what we might mean by economic justice. It was this that got me going. Until then I was interested in the subject but my interest had been confined to providing people with social entitlements to provide for basic needs if they couldn't provide for them themselves. (By the way, I was one of a small group of economists advising Robert Kennedy in 1968 prior to his expected campaign for the presidency; of course, he was shot before he got the nomination.) What was fascinating about Rawls was that he wasn't talking about some sort of broader justice on, say, the division of land or schooling. He was talking about dividing the social surplus that comes about when people cooperate in production. Although he didn't use the term "economic justice," this was really what it was all about. I thought that his work was such a breath of fresh air—we don't have to be talking about everything at once any more, we can talk about this for a while and get it straight, and then we can think about entitlements for people who don't work and who are not part of the economy.

Then a funny thing happened. When I was writing my textbook *Political Economy* (Phelps, 1985), I knew I had this chapter coming up on economic justice in connection with taxation and whatnot. At the same time, I knew that professors in law schools had Rawls standing for something quite different—for some unspecified version of social justice and extreme egalitarianism. So I wrote Rawls a long letter from Amsterdam in 1981 or '82, where I was one summer, asking him to confirm that my interpretation of him was right: that he's not talking about the distribution of wealth, he's essentially talking about the principles for a just pattern of wages to apportion the surplus that gets created when people of diverse talents and backgrounds cooperate with one another. He didn't answer the letter. Finally I finished my textbook and sent him a copy, whereupon he wrote back a lovely comment saying that I had accurately represented his position.

But people *continued* to misrepresent him, and in 1991 we had lunch in New York together, and I was complaining to him about this and his apparent reluctance to say anything in print to disavow any of these other guys with their rival interpretations. There was one guy—a philosopher of sorts—who interviewed him in a book–length way towards the end of his career, and he asked Rawls this question: what's this economic justice about, who's it for, what sort of conditions do you have to meet to get this payment or whatever it is? And Rawls just comes out and says in a perfectly blunt way: "Look, it's not for beach bums" [*laughter*]. And I felt so relieved and gratified that my little campaign to set people right on Rawls had been vindicated. But twice I had to come to his rescue in the *Wall Street Journal* (Phelps, 2002, 2007). Twice they smeared

him with the most naive thinking—as a rank egalitarian. I replied once in what I thought was a very eloquent letter setting them straight, but then they did it again about three years ago, calling him a socialist, and I again had to pick up my pen.

Then a few months later, I got an email from Rawls's eldest son who wrote how disturbed he was about how his father had been portrayed. So I wrote back (and here I was sticking my neck out a little bit) that I knew him, that I'd been around at Stanford when he was writing his book, and that I thought he was motivated by the huge social problems in the U.S. associated with the difficulties young blacks were having connecting with the American economy. And I said I thought that Rawls was doing a tremendous service to an American capitalism jeopardized by the social problems that were becoming so inflamed at that time. His son wrote back that I was right on the mark.

Current Work: A Justification for Capitalism

VANE and MULHEARN: What issues or areas are you currently working on?

PHELPS: Since I won the Nobel Prize, there are days when I find it hard to remember what I'm working on. It's devastating what numerous trips to Beijing, Delhi, and Rio can do to you. You feel great while you're there. You're having fun and sometimes it's lucrative too. Sometimes I come home and my wife tells me I look happier and better than when I left, but then the next day I'm dead and I lose momentum with whatever I've been doing and it's very difficult.

Since 2002, I've been trying to develop a new justification for capitalism, at least I think it's new, in which I say that if we're going to have any possibility of intellectual development we're going to have to have jobs offering stimulating and challenging opportunities for problem solving, discovery, exploration, and so on. And capitalism, like it or not, has so far been an extraordinary engine for generating creative workplaces in which that sort of personal growth and personal development is possible; perhaps not for everybody but for an appreciable number of people, so if you think that it's a human right to have that kind of a life, then you have on the face of it a justification for capitalism. There has to be something pretty powerful to overturn or override that.

I've also done some work on attitudes to work across different countries—what people look for in their jobs—using the World Value Surveys carried out at the University of Michigan. I started to try to write a book this past summer incorporating all this material, but I see that it has to be a very well-written book—well-organized and thoughtful. And now the financial crisis has come along, so there's a whole other dimension I have to take into account. This means that instead of having essentially two parts, one about the rise of capitalism and another about the twentieth-century struggles with communism and market socialism, I've got to write a third part about the new crises. I think I'll argue that I'm still right, that justice requires that we have a well-functioning capitalism, though one with low-wage employment subsidies and so forth. But now you can't just assume a well-functioning capitalism, like the old joke about assuming a can opener, so how do we

underwrite this? It requires more thinking, and the book is going to be more interesting than I thought and much more challenging and difficult too.

Personal Reflections

VANE and MULHEARN: To the casual observer it appears that each time you have moved to take up a new post at another university—notably from Yale to Penn, and from Penn to Columbia—it has acted as a catalyst for you to refocus your research in a different direction. Has that been the case and, if so, why do you think that has happened?

PHELPS: Oh I don't know—it might just be a new stationary closet, new pens or paper; maybe I lost some books in the move [*laughter*]. Probably there's no causation. I most likely moved when I felt I was coming to the end of something, and so not only did I want to work on something new, I wanted to change my location too. But it is kind of stimulating to start in a new place.

VANE and MULHEARN: Testimony to your standing in the profession was reflected in an international *Festschrift* held in your honor at Columbia University in October 2001. Axel Leijonhufvud (2004, p. 811) has commented that the "assembly of participants who [came] to acknowledge their indebtedness [to you] was quite extraordinary" and that "it included almost everyone still living who has played a major role in macroeconomics over the last several decades." Most economists, especially macroeconomists, would agree that while the award of the Nobel Memorial Prize in Economics in 2006 was well deserved, it was long overdue. Did you ever think that your time had passed and that your work might not be acknowledged by the award of the prize?

PHELPS: I think one reason it appeared to come late is that I didn't want to get the Nobel Prize for my work on growth; I wanted it for my work on microeconomic foundations. And that work only really started to come out in 1967 and 1968; by then, I was 35 years of age. The lag between that work and 2006 was what, 39 years? Well, it's about what Mundell had to wait too. Some people did their seminal work quite early. I was fortunate that I was still alive and kicking, quite full of energy actually. I don't harbor any resentment over the wait. I wish I'd gotten it sooner, but mostly because I could have done more good with it.

VANE and MULHEARN: The prize rewards specific discoveries, achievements, or breakthroughs in economic science. Your pioneering contributions have opened up a rich seam of research for others to mine. Does academic knowledge largely progress through the lead taken by a small number of creative innovators?

PHELPS: That's such a good question. It resonates with a subject in the area of innovation theory. The old guys like Arthur Spiethoff [8] thought that progress was due to the great discoveries of the scientists and navigators. Schumpeter (1934)

[8] Arthur Spiethoff (1873–1957) was a leading member of the German Historical School whose work, in the first decade of the twentieth century, on the theory of cycles linked innovations to such forces as technological breakthroughs and the discovery of new overseas markets.

didn't depart altogether from that, he simply said, well, that's right but you've got to have some entrepreneur to actually implement it. But don't think there's much creativity there—everybody knows what's in the air. And it's very rare that anything new really gets created in the course of this development work. But now we don't think about innovation in that way so much. We recognize that once in a while there is a big leap which creates the ground for a surge of innovations to follow. Nowadays we realize that an awful lot of innovation just comes from business people operating at the grass roots having ideas on the basis of what they see around them. Nothing to do with science—it's just creative mankind chipping away at things.

I know that the Sens and the Mundells and the Lucases are towering figures, but they couldn't have become so if they hadn't read a lot of papers by, well, pretty average people who are just doing a good job of exploring a question and giving inspiration. I guess the towering figures are people with just a little more drive, a little more imagination, just a little cleverer in putting some things together. In other words, I don't know the answer to the question [*laughter*].

VANE and MULHEARN: Many thanks for a fascinating and enjoyable interview.

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