The Rate of Return and the Rate of Interest

by

Robert M. Solow

The Industrial Institute for Economic and Social Research

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Address

Industriens Utredningsinstitut
Box 5501, S-114 85 Stockholm, Sweden
Tel. 08-783 80 00

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Preface

Professor Robert M. Solow received the Prize in Economic Science in Memory of Alfred Nobel in 1987. As part of the established tradition IUI and the Federation of Swedish Industries invited him back to Sweden to give a lecture on a topic of his choice.

We were very happy that Robert Solow could present his lecture on the occasion of the 50th Anniversary Symposium of IUI, November 15–17, 1989 on Research in Industrial Economics. Few topics could be more appropriate under that heading than the topic chosen by Robert Solow for his lecture on November 17, *The Rate of Return and the Rate of Interest.* It is the concern of policy makers of most industrial nations today.

Stockholm in January 1991

Gunnar Eliasson

The Rate of Return and the Rate of Interest

The general subject of this talk – the relation between the rate of return on investment and the market rate of interest – was suggested to me many months ago by Gunnar Eliasson. I accepted the suggestion willingly, for several reasons. The most elementary is that I am lecturing under the auspices of IUI, and the Institute's director is better able than I to guess what will interest this audience, and what will be relevant to the research program pursued by the Institute and its associates.

Gunnar Eliasson was not making his suggestion in a vacuum, however. The definition and measurement and significance of the social rate of return on investment is a subject to which I once devoted some serious effort. (See *Capital Theory and the Rate of Return,* 1963). So it was reasonable to suppose that I might like to revisit those questions and reconsider them from a contemporary point of view. The important point, in fact, is that new developments in the theory of capital and growth actually do have implications for the way one should think about the return on investment; and that will be my main focus in this lecture.

Having said that, I want to start all over and approach my subject from another direction, to connect it up with much broader concerns. Just recently I have spent some 2.5 years as a member of a faculty committee at my university; the committee had been asked to think about the development of industrial productivity in the United States, especially its relative decline compared with other industrial countries, and to recommend any changes in the design of engineering and management education at a leading technological university like M.I.T. that might be a proper response to those developments, and might even help to halt or reverse them. I am not about to tell you what we found. The report of the committee was published as a book earlier this year (*Made in America,*

1989). It has been well received and has sold many times more copies than any other publication to which I ever made a substantial contribution. (It is a bit frightening. I know there are other economists who habitually write on large subjects for large audiences. I wonder where they get their self-confidence.)

But I want to use my experience on that committee in another way. This was a group of some sixteen members of the M.I.T. faculty. The members included engineers, scientists, some with extensive experience in industry or as consultants to industrial firms, professors of industrial relations and political science, but no psychologist (for an accidental reason). These were able and articulate people. From our early discussions, in which there was a lot of what you might call "station identification", staking out of attitudes and positions, it emerged that the group had a fairly homogeneous view of what "economists" typically think and how "economists" typically react to questions like the ones we were supposed to study. According to that consensus, "economists" think that the organizational and behavioral habits of industry are more or less irrelevant to perceived failures in the field of productivity or international competitiveness. "Economists" think that all such questions have purely macroeconomic solutions. Get interest rates and exchange rates right and there will be nothing left to discuss. I need hardly tell you that along with this attribution of opinion went a certain amount of derision.

Were they right about economists? I have to say that I think my colleagues from other disciplines certainly underestimate the power and importance of things like interest rates and exchange rates in determining what they see around them. They are too tempted to take one micro-problem at a time, without realizing that they are interrelated through the general functioning of the economy. So it is all the more important for economists to keep emphasizing the importance of such economy-wide forces for microeconomic events. But I also argued that their perception of "economists" was largely incorrect. I believe, and I suppose that most of the people in

this room believe, that macroeconomic variables will inevitably leave a lot unexplained. They will not tell us why one industry or group of firms in Sweden or the U.S. will succeed in capturing and holding customers around the world by pushing costs down and designing a stream of innovative and marketable products, while other industries or groups of firms will not, or why some industries in some places stay at the technological forefront and prosper while others fall behind and fade away. I really think most economists understand that very little is achieved by labelling such differences as differences in "comparative advantage" without analyzing what actually constitutes comparative advantage and how it is developed and maintained. At least that is what I told my colleagues on the committee.

I do have to admit that, when our report was published, and did try to analyze in concrete detail how industrial practice affects productivity and competitiveness, some of my old friends in economics did ask me whether I had been "converted" to this alien belief and really intended to abandon my normal focus on interest rates, exchange rates and other sensible economy-wide quantities. It was quite worrying. But I think I have a more comforting interpretation of what they really meant. Maybe it is that we (economists) have learned how to think analytically and clearly about prices and quantities, even if we often make mistakes; but if we are tempted to think about industrial practice, we will descend into vagueness and hand-waving. That is not a foolish point, and I will return to it occasionally later on in this lecture. It has something to do with the main point I want to make about the rate of return on investment.

So now I come back to the rate of return itself. In my earlier work, this concept played a very particular role. For both intrinsic and historical reasons, much of economic analysis centers about the relations between prices and quantities; how to use those connections to interpret what is going on in the economy, and thus in the end how to use them to affect what is going on in the economy. When it comes to capital theory –

by which I mean only those questions that are closely bound up with the fact that production of final output involves the productive use of stocks of goods – one of the relevant prices is the rate of interest, or more generally the (marginal or average) cost of capital to firms engaged in investment. I am not going to discuss financial structure at all, so I will usually just speak of "the" rate of interest to stand for this more complex concept. If the rate of interest is a price, what is the corresponding quantity? As you know, habit suggests that it is "the marginal product of capital" in much the same way as with other productive inputs. For many purposes, I think that is a reasonable way to proceed, without claiming that it is more than a first approximation. But that usage is also vulnerable to criticism. It depends, perhaps too much, on taking chances with aggregation, on making unrealistic assumptions about the smooth variability of the proportions in which stock and flow inputs can be combined in production, and on casually ignoring the tremendous variety of forms that capital-using production can take, from aging wine, to building power plants, to engaging in research. (It is sometimes argued that there are somehow "deeper" difficulties with this simple way of thinking. I am unable to make sense of those arguments; but this is not the place to go into them.)

It seemed to me that one could avoid a lot of diversionary and unproductive debate by more or less eliminating the concept of "capital" from capital theory, by focussing instead on feasible variations in the flow of investment and the consequent variations in the flow of final consumption. From the point of view of the investing firm or from the point of view of the saving and investing society, the interesting and important questions are usually of that form; if goods are diverted from investment to consumption or from consumption to investment, how will that alter the feasible future flows of final consumption? If funds are borrowed or diverted from profit and used in acts of investment, how will the feasible future time paths of profit be changed? Thinking in that way has the virtue of corresponding to the sorts of decisions that firms and

societies actually have to make. And it leads quite naturally to concepts like sequences of one-period rates of return, or stationary rates of return, or intermediate notions. (In particularly simple cases it reproduces elementary notions like "the marginal productivity of capital" but without so much extra freight.)

The main outcome of this investigation, generally speaking, was fairly bland. Under ideal conditions – divisibility, constant returns to scale, competitive markets – market equilibrium requires equality of the private rate of return on investment and the appropriate market rate of interest. If, in addition, there are no significant externalities, the market interest rate is equal to, i.e. measures, the social rate of return on investment. The standard assumptions give the standard results. Today I want to emphasize the likelihood that the standard assumptions will fail in certain important and interesting cases, and then the standard results have to be considerably modified.

As soon as one asks questions like this, it is natural – at least for an economist like me – to embed them in a model of economic growth. It is characteristic of economic theory that every price measures two things in a well-functioning market. Those things are usually a marginal cost and a marginal benefit; it is by comparing them and equating them that competitive markets are supposed to do the magic they are supposed to do. In a growing and investing economy, the rate of interest is supposed to measure the rate of return on investment; that is the marginal benefit. It is also supposed to measure the value to consumers of the current consumption that has to be foregone in order to make room for an incremental flow of investment; that is the marginal cost.

In the standard sort of ("neo-classical") model of a growing economy, given all the nice convenient assumptions I mentioned a moment ago, the standard propositions continue to hold. The interest rate can guide the paths of investment and saving in much the same way as the price of bread guides the supply of and demand for bread. That is so for private transac-

tions and it is also true for the economy as a whole, always provided the assumptions are met and there are no externalities. (At this level of abstraction the sorts of coordination failures that seem to underlie business-cycle fluctuations can mostly be labelled as externalities without too much strain, so they are ruled out of court in order to focus on trends.)

In the simplest models of economic growth, technological progress is occurring, but it is purely "exogenous". That is to say: technological progress occurs without any of the participants in the economy having to make any decision or incur any costs. It just happens that, silently, production from given inputs of capital and labor gets more efficient as time passes. But technological progress affects the return on investment. It must; a given flow of investment will generate a larger subsequent flow of consumption if technology is improving than if it is not. So exogenous technological progress will get built into the interest rate as well. On the other side, in a growing economy that has settled down into its steady state, the rate of interest that guides investment will have to include an allowance for the fact that growing per capita consumption – fuelled by technological progress – will be satisfying less and less urgent consumption needs as one moves further into the future. Other things equal, faster technological change implies a higher return on investment and a higher rate of interest.

If I may revert just for a moment to technical jargon, once a growing economy of this particularly friendly kind has got anywhere near its steady state, the rate of interest will simultaneoulsy measure two quantities. One is the rate of return on investment over a period corresponding to the term of the interest rate. The rate of return has two components, one coming from ongoing technological progress, the other from increasing capital intensity. The other quantity is the marginal cost of inducing savers to part with resources for investment. That has two components as well, the first is pure time preference, or impatience. The second reflects the fact that in a growing economy, consumption deferred from now until later is consumption taken away when the standard of living is lower

and added back when the standard of living is higher. With diminishing marginal utility, this transfer is a loss even apart from the passage of time. Savers will require remuneration for accepting that loss. Since the rate of interest equals both the rate of return on investment and the required rate of remuneration for saving, the two are equal to each other. Comforting conclusions follow from this result.

There are two important ways in which this chain of reasoning might fail. The first is recession; if there is unemployed labor and unused capacity, then there is no need to sacrifice consumption in order to increase investment or to sacrifice investment in order to increase consumption. The (social) rate of return on investment may exceed the required rate of return on saving. Beneficial investment is prevented from happening by a market failure. The second broad source of problems is the presence of more conventional externalities. That is the case I want to emphasize here.

Just recently there has been a revival of interest in the theory of economic growth, stimulated by the work of Paul Romer and Robert Lucas of the University of Chicago, now followed by many others. Romer and Lucas proceed in slightly different ways, but they have similar goals and share the same general approach. If it were at all reasonable, even as a first approximation, to model technological progress as exogenous, then all countries of the world would have access to a common technology. Well, perhaps not *all* countries; but textbooks circulate freely, proprietary technology can be licensed, and it is hard to deny that India and Pakistan must in principle be able to know the same technology that is in use in Sweden. In that case, or even if they lag behind by a fixed number of years, they ought in the long run to grow at the same rate (in *per capita* terms). One can perhaps maintain that there is some sort of convergence of growth rates among the already successfully industrialized countries of the world, but surely not when the poor countries are included. So, according to this line of reasoning, there must be another important source of productivity growth besides exogenous technological progress,

something that can differ enduringly between countries. The likeliest candidate for this factor is investment in human capital. And that is an endogenous factor, determined by centralized or decentralized human decision based on a calculation of benefits and costs.

Before going on, I had better say that I do not find this reasoning altogether convincing. It is always possible to *define* abstract technology as something that is universally available. But I am not sure it makes good sense to presume that all countries and cultures have access to it on equal terms. Endemic political and social instability might be an effective barrier for some countries, widespread private and public corruption for others. It is not hard to imagine tax laws, kinship structures, religious beliefs and social practices that would interfere with the full use of modern industrial technology. I think Lucas and Romer would say in reply that those barriers themselves just reflect underinvestment in human capital. The capacity to make productive use of industrial technology has to be learned, and learning it is a form of investment. That may be so, but it only raises the question whether, in some societies, the activities normally modelled as human-capital investment will have the kinds of effects postulated in a theory that draws its inspiration from already modernized societies. A school for commissars or ayatollahs may not produce engineers.

The idea of endogenously determined growth, induced by deliberate investment in human capital, or in research and development directly, is interesting and plausible in any case. Romer and Lucas both argue explicitly in terms of human capital. Mere inclusion of human capital in a growth model will not change the outcome very much; it just introduces another asset that can be accumulated by diversion of current output from current consumption. What really makes a difference, and what is important for my purposes here, is a further assumption. Accumulation of human capital adds to the productivity of the person in whom it is embodied, and that is why individuals invest in it. But there is also an additional external effect. The general level of productivity rises by more

than can be accounted for or captured by the person or firm that makes each particular investment. It is easy to think of reasons why such an externality might exist. The thought is hardly a new one; but Romer and Lucas deserve credit for taking this thought out of the realm of vague general commentary and building it into complete models of economic growth where one is forced to be precise about the way human capital affects productivity and the particular consequences that follow.

One of the things we learned from the "old" growth models is that it is not easy to increase the rate of growth permanently. The "new" growth models have to be very generous to achieve that result. I mean they have to incorporate human capital in ways that give it a lot of leverage over productivity. Romer and Lucas both make the assumption that there are increasing returns to scale in the aggregate when full account is taken of both the internal and external effects of the stock of human capital. That is to say: if output depends on the inputs of tangible capital, raw labor and human capital, then increasing all three inputs in the same proportion will cause aggregate output to increase more than proportionally. But that is not all; mere increasing returns to scale will not by itself make the long-run growth rate endogenous. Some more leverage is needed. Romer provides it by supposing that there are increasing marginal returns to human capital by itself. Human capital is produced by spending resources under diminishing returns; but once an increment to the stock is generated, it adds more to aggregate output than the preceding increment did. That is certainly "generous". We have very little intuition to go on here, and even less evidence, but that does not strike me as inherently plausible.

Lucas takes a slightly different tack in his paper. He can get along with diminishing returns to the stock of human capital; but he supposes that there are increasing returns in the production of human capital. To be precise, if you imagine increments to human capital to be produced by the existing stock of human capital and by the current allocation of labor-

time to training and similar activities, then in Lucas's model the production function for human capital is homogeneous of degree two. If society starts with twice as much human capital (per capita) and devotes twice as much time (per capita) to training, the "output" of additional human capital will be multiplied by four. That too strikes me as stacking the deck in favor of an endogenous growth rate. It is a very valuable exercise to see how these strong assumptions create the link between thrift and growth that the older theories could not. But a reasonably skeptical person would not regard the case as solidly made.

(I want to digress for just a moment. It is interesting that both Romer and Lucas argue throughout in terms of human capital although the equations they write make the "third factor" look more like accumulated knowledge. Both have to assume that human capital is passed on intact from generation to generation; nothing is lost with the retirement or death of those in whom the training was originally conferred. This seems implausible to me for most learned skills; why would firms regret the loss of skilled employees? This assumption does make sense for technological knowledge of the kind that can be recorded in manuals and textbooks. Most of the equations in the Romer and Lucas papers, perhaps all of them, could be carried over with this alternative interpretation. But even so, the strong assumptions needed to endogenize the growth rate still seem less than convincing.)

I want to go on to a different sort of model that strikes me as a better vehicle for the discussion of endogenous technological progress; but first I make one obvious remark about the Romer–Lucas models. As soon as the stock of human capital (or the stock of accumulated research results) has external effects on production as well as internal effects, the social value of investment in training (or research) exceeds the private value. Those who invest resources in these externality-generating activities can profit from only part of the productivity benefits they create. In equilibrium, the cost of capital for this sort of investment will measure only the private

rate of return; it will be less than the social rate of return. So there will be underinvestment in human capital or in research and development.

As they specify the problem, ordinary tangible capital confers no externality at all. The market will bring the cost of capital into equality with the private and social rate of return to investment in capital goods. If investment in human capital (or R&D) can be financed directly or indirectly through the capital market, the same (or a risk-adjusted) rate of interest will be brought into equality with the private rate of return to investment in human or intellectual capital. But both will be less than the social rate of return. There is a case for extra incentives to generate the socially profitable human-capital investment that the market will pass by.

Subsidies to lower the private cost of capital would not be the best choice here because not all uses of capital carry the externality. Something more capable of being directed at the appropriate target is called for. Here it is a good idea not to take the model too literally. The model distinguishes just two kinds of investment and supposes one of them to have favorable external effects but not the other. In reality, not all human-capital investments or R&D expenditures will carry significant externalities; and some tangible investment may do so, as when transportation and information channels generate network externalities. The goal would presumably be to design incentives that discriminate reasonably between externality-carrying investments and others, without being unreasonably detailed and complex.

All this is elementary reasoning, and it does not take a complicated model to verify it. But Romer's and Lucas's models make this thought precise. On their own assumptions, they specify the parameters on which the size of the investment shortfall depends. But of course those specifications carry no more conviction than the assumptions themselves. Actually, the lessons to be drawn from a literal reading of the model are reasonable, easy to understand, and perhaps not even so hard to apply. The shortfall in human-capital investment will be

larger the larger is the externality-generating effect, the more productive the "technology" for converting labor-time into human or intellectual capital, the more patient the society (i.e., the lower the rate of pure time preference) and the faster the rate of population growth. There are no surprises in that list, but there are no real insights either.

Having said all this in praise of some genuinely interesting new work, I will venture the guess that many of you have the feeling that this line of thought somehow misses the point. If you try to put your finger on what it is that bothers you, I will guess further that many of you will say that this description of the accumulation of human capital is not "Schumpeterian" enough. This is a very common complaint, as you know. It is almost a slogan; growth theorists – like me, as well as Romer and Lucas – talk about technological progress, exogenous or endogenous, in a sort of aggregative mechanical way, and those who like to look at the same problems from the point of view of the firm say that the theorists are simply missing the Schumpeterian character of the process of innovation. Well, I want to agree with you, but with one or two qualifications.

No one who ever modelled technological progress as exogenous – here I speak with experience – ever believed that it was exogenous, except for a residue of chance discoveries and other windfalls. That device was just a way of postponing the need to think about a difficult subject while one got on with understanding the consequences of technological progress. Similarly, when Romer, Lucas, Hahn and others get around to modelling endogenous technological progress, they do not really believe that the process of innovation can be described in the simple mechanical way that they choose to represent it in their models. Highly sophisticated economists do not lull themselves into believing that intellectual capital can be described as a one-dimensional stock, or that generalized productivity increase can be described as a homogeneous function of any degree of the size of that stock and the amount of current resources devoted to investment in human capital or in research and development. They are simply making the

minimal assumptions they need to get on with the next phase of the analysis.

The progress of economic analysis is not much helped just by insisting on the chancy and irregular character of invention and innovation, or even just by noticing the problematical and temporary character of the profits from innovation, as Schumpeter did. Those things need to be said once. Thereafter, progress in understanding depends more on finding ways to make the Schumpeterian vision more precise, to reduce it somehow to a workable model. This process will undoubtedly reintroduce mechanical elements and bleach away some of the realistic quirks and fuzzy colors that a less formal description can communicate. Nevertheless I think the model-building exercise is absolutely essential if we are ever to use the insights provided by the loosely Schumpeterian view.

Fortunately, or maybe one should say inevitably, that is exactly what is now beginning to happen. It may even have begun to happen some years ago, with the publication of the book by Richard Nelson and Sidney Winter. But the more recent developments are neater and more likely to fit in with the further development of the theory of economic growth. I am going to discuss only one of those efforts, so far just a single paper by my M.I.T. colleague Paul Krugman. I choose it because it is the simplest to describe and because it is most adaptable to my purposes here today. For the record I should mention some other research in this direction that has come to my attention. It includes an interesting but difficult paper by my colleague Philippe Aghion and Peter Howitt that tries to model precisely Schumpeter's notion of "creative destruction", i.e. that succeeding innovations erode the profits of their predecessors. There is also a series of papers by Gene Grossman and Elhanan Helpman in the context of product cycles in international trade. Finally, there is a group at Stanford that is especially interested in path-dependent processes which allow the local accidents of the timing of innovation to have permanent effects on subsequent history. I think that this line of thought is not only important for understanding

economic growth, but also fascinating as an intellectual exercise.

I will not try to describe the details of the Krugman model, but I must give you a fair picture of the outline. The story lasts for exactly three periods, though it can undoubtedly be expanded into a succession of overlapping three-period episodes. Three periods are needed because entrepreneurs are assumed to incur costs to create innovations in the first period; these innovations confer monopoly power in the second period, during which successful entrepreneurs recoup their costs and make some profit; but in the third period an innovation becomes public knowledge and competition erodes any rents there might otherwise be. Around this elementary story a lot of mechanical detail has to be inserted so that a precise analysis can be made.

Now let us look a little closer. The Krugman–Schumpeter economy has a given number of industries, each producing a single commodity and competitive enough so that firms in each industry earn only normal profit. The commodities are symmetrically imperfect substitutes for each other. In the first period a certain number of firms in each industry pay a cost and become innovators. There is nothing in the model to determine which firms do this, but the number of innovating firms is endogenous and has an equilibrium value; if too many firms innovate, they will not be able to earn enough to induce consumers to release the needed resources for investment in innovation. Although Krugman does not put it this way, the rate of interest would exceed the private rate of return on R&D investment. If too few firms innovate, the inequality would go the other way.

In the second period, innovating firms have lower costs than the non-innovators. (These are process innovations, not product innovations, though I suspect it would not be hard to model product innovations analogously.) They can shelter behind the break-even prices charged by the non-innovators and thus each innovating entrepreneur collects rents equal to the cost advantage over non-innovators. Without these monopoly

profits, as Schumpeter insisted, there would be no motive to innovate. It turns out, in the particular model analyzed by Krugman, that the return to innovation is actually larger the more firms choose to innovate. It must be that the additional competition is outweighed by the higher income that accompanies more intense innovation. The interest rate has to increase still faster to generate a stable equilibrium. This is an interesting characteristic because it opens up the possibility of multiple equilibria. (This is one form of path-dependence.) If there is little innovation, then the return is low and there is no stimulus to further innovation; but if there were a lot of innovation, the return would be higher and possibly high enough to justify a high-innovation equilibrium.

In the third period, Schumpeter's imitators clog each innovating industry so the price falls back to average cost (including normal profit) and the episode is over. (The reduction in real cost is permanent, of course.) Needless to say, it would certainly be possible to improve on this model. It could be made sequential. It could be made stochastic. It could be made more complex. But I like it in this simple form because it is so transparent.

Krugman uses this model to make a Schumpeterian point. Innovators charge a monopoly price for their goods in period 2. It is a slightly peculiar monopoly price because their demand curves turn horizontal at the price charged by non-innovating firms. But it is a price higher than marginal cost; it is precisely out of the gap between price and marginal (=average) cost that innovators collect their rents. So the outcome in period 2 is inefficient. If innovators could be forced or induced to behave like competitors, there is a lump-sum redistribution that would make everyone better off in period 2. But that policy would effectively eliminate the incentive to innovate. Krugman is able to show quite directly that the economy is better off in the three-period Schumpeterian equilibrium than it would be if there were no innovation at all. The productivity gains from innovation are more than enough to cover both the specific costs of innovation and the inefficiency that results

from the monopolization of new productive processes in period 2.

It is possible to take this analysis a bit further than Krugman does. The equilibrium condition can be interpreted as the equality of the rate of interest and the private return to investment in innovation. The monopolistic distortions that accompany innovation certainly suggest the possibility that the social return to innovation exceeds the private return, not because of any public-good aspects but just because of the market distortions that are needed to cultivate the innovations themselves. So this may be one of those cases where the social return to innovation exceeds the rate of interest. That turns out to be the case. It is possible to show that the Krugman–Schumpeter equilibrium entails underinvestment in innovation; the representative household would be better off if the number of innovating firms had been larger.

The implication is that even in an economy in which innovators collect monopoly rents, there may well be a case for further encouragement of technological innovation through public policy. But I think this situation is more complex than the conventional case for subsidizing activities that generate positive externalities, like reforestation. If one conceptualizes the situation as one in which the social return exceeds the market rate of return in equilibrium, and therefore exceeds the market rate of interest, an easy conclusion might be the desirability of subsidizing the cost of capital used for investment in research and development. It would probably have the desired effect; but it would also involve a large undesired transfer to those who would be innovating anyway. That is very often the case in such situations; it is very hard to focus a tax or subsidy exactly on the relevant margin of decision. The extra difference here is only that infra-marginal subsidies go directly into monopoly rents. For this reason I was rather attracted by the suggestion made in one paper at this conference that public encouragement of innovation might take the form of accepting stock options in innovating firms.

I think this is an interesting example of a way in which the

industrial economics of IUI can be reconciled with or connected to the work of macroeconomic model-builders. It shows that questions of industrial structure can have macroeconomic implications and it at least suggests how the insights that arise from studies of industrial structure might be translated into the language of macroeconomic model-building. There is no reason for these two traditional approaches to the understanding of economic growth to be thought of as mutually exclusive alternatives.

There may be other methodological lessons to be learned. Much of the most interesting recent work in macroeconomics, and in the related study of labor markets, has come from giving up simple assumption about market structure and allowing instead for imperfect competition, for strategic behavior, for the adoption of robust rules of thumb, and other such departures from simple price-taking. Nevertheless I think it has been tacitly assumed that long-run analysis could safely dispense with these ideas. There is a tendency to believe that in the long run competition rules and markets clear in the conventional way. I have been meaning to cast doubt on that belief.

Suppose the goal is to construct a model of growth with endogenous technological progress. The work of Romer and Lucas provides excellent examples of how such a model might look if it stays as close as possible to the traditional rules of the model-building game. It is interesting, even exciting theory, and it has excited many economists. But it seems to me, and probably to most of you, that a model built on assumptions a little closer to the reality of industry would be even more exciting, and more convincing. Having said that, I want to insist that hand-waving is not enough. The trick is to find ways to embody "the reality of industry" in workable formulations that can serve as the building-blocks of precise (and therefore "unrealistic") models. It is more than a trick; it is what economic theory actually is.

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DISCUSSION

Gunnar Eliasson: We have heard a very interesting presentation of one of the most important problems we have in economics. The nature of the rate of interest and the rate of return has been a problem in economics for centuries. It integrates the intricate economic problems of distribution, balance and growth. So, even though we won't expect to come out with *the answer* today, there must be many questions.

Lars Werin (University of Stockholm): You mentioned externalities very often in your lecture. You said, for instance, that interest rates could coordinate savings and investments. Monitoring and information problems create a wedge between the rate of interest and the rate of return, and savings and investments are not coordinated. This is an externality. Is there any research going on that will make these externalities more tangible? Research that would bring your analysis closer to the practice of industries and banks. Maybe markets do not function because of policies. Could you say something more to describe the reason for the externalities?

Robert Solow: I am trying to think what is in Lars' mind so I can get it more closely, but let me just try. Obviously in a fifty minute talk I could not describe in detail how one might try to model the precise way externalities occur and the way policies might get at them. But let me reply to a few of the specific questions which were implicit in what Lars Werin was saying. What happens when the capital market fails to coordinate the savings and investment activities and the business cycle? I classify that as an externality, half as a joke and half not. As it happens, *much of the most interesting macroeconomic theory being done today* is an attempt to investigate coordination failures in the economy. They are very much like external effects, or what in the U.S. academic circles are called thin market externalities. I simply did not want to discuss short-run business cycle problems. The issue for my lecture was the long-run

question. *Does the modern economy underinvest in the accumulation of knowledge?* Is there a gap, which the market cannot eliminate, between the social return to investment in knowledge and the cost of capital? I was assured last night at the dinner by doctor Wallenberg, that the cost of capital is not a factor of importance in investment decisions. But I think doctor Wallenberg limited himself to consider obviously profitable investment decisions. If there is a wedge between the extent to which private investment can profit from the accumulation of knowledge and the extent to what the society at large profits by the accumulation of knowledge, then a public policy should be designed to eliminate that gap. The policy worker would have to know exactly the economic mechanisms contributing to that end. Otherwise much of what he does will be dissipated in rewarding actions which would have taken place even without the award. I do not myself know very much about how to model the market mechanisms that generate that kind of wedge. I would have thought that it is the task of an industrial research institute, like IUI, to look at the concrete details of innovation and to ask how it is, or where it is, that private investment in knowledge is not fully appropriate. I would give you a simple example that was in the press in the U.S. last week. The Electrical Power Research Institute, which is a semipublic research organization, sponsored by the electric power industry, has just unveiled an electric car, a delivery van, which apparently runs effectively on batteries. It has a range well over 100 miles between charging, and has an operating speed as high as 70 miles an hour. Careful calculations have been made on the environmental effects of one of these vans, including the marginal emissions from the power generation that is required to charge the batteries. There is an allocation of private funds to R&D that certainly will generate a social externality. That is a simple way in which the wedge arises. I do not think it is the most interesting way in which the wedge arises. More interesting is the questions raised in some of the papers of the IUI 50 years conference yesterday: Why is it that Sweden has a comparative advantage in the production of

research? Why do multinational enterprises do research in Sweden, and produce their goods elsewhere? I do not know the answer to questions like that. I think that is your business to find out.

Gunnar Eliasson: May I relate what you just said to the results of John Bishop of Cornell University? John finds that the private return to education is lower than the social return. It pays for society to invest in education, but not for the student. Maybe this is a case of what you just suggested? The labor market for educated people does not seem to be very efficient.

Robert Solow: Yes, but Bishop's results generate another question. John studies the effect of schooling. One would like a very careful study of the effects of training conducted by firms. There is reason to believe that in the U.S., an economy with quite a lot of mobility of labor, the wedge between the private and the social return of training is low. A firm that engages in training programs can capture the benefits itself and the full effect of training may be larger than in an economy in which the mobility of labor is not so great.

Tore Browaldh (Svenska Handelsbanken): My question may be outside the topic, because you said specifically that the financial markets were not to be touched on in your lecture. I wonder whether any of the models you refer to incorporates a particular problem that we worry about here in Sweden. The problem is that financial investments for many years have been larger than investments in machines and buildings. Is this at all a problem when seen from your point of view? Do any of your models capture this?

Robert Solow: Do you have in mind what in America is called financial engineering? I know of no one who has tried to model, measure or capture this phenomenon. I am convinced, but not on grounds that I can defend very well, that there is overinvestment in financial engineering, and underinvestment

in real engineering. But I do not know how to capture that. It is possible that the resources that are devoted to financial engineering are highly specialized and have no other social value. Maybe all those MBAs are not suited for any other activity than buying and selling pieces of paper. But it is a fact that careful research on the productivity of financial engineering, for example the effectiveness of *takeovers* and of *leveraged buy outs,* has only come out with very inconclusive results. I share your instinct but this is a primitive instinct. It needs more analysis than it has had.

Hans Werthén (Electrolux): If you go to the United States and look at Silicon Valley, you will notice enormous innovative progress generated by competition and venture capital. These innovations have had a lot of consequences for instance for communication industry. I would like to ask you about how much is distorted because of the enormous military research, for example the star wars and the satellites. I understand that a significant part of U.S. investments in R&D and in innovations comes from military sources.

Robert Solow: Yesterday and this morning at the IUI conference, I was listening to papers on research and development of the U.S. economy. It seems to me that both military and civilian R&D expenditure were simply added together. It is not clear to me that this is appropriate. I cannot give you a direct answer about the communications industry, but I will mention an important example which illustrates the kind of diversion that can take place when research and development respond primarily to military needs. The numerically controlled machine tool was invented not only in the U.S. but at M.I.T., my university. The defense department immediately saw the value of this technology and financed the development of numerically controlled machine tools. The military needs extremely fine tolerances of missiles and missile engines. The metal part of such things must be shaped far more exactly than ordinary civilian use requires. Under the stimulus of military

financing the American machine tool industry concentrated on the building of very large, very expensive, dedicated, numerically controlled machines that could do only one thing, but could do it superbly well. The Japanese industry focused instead on the production of small, more flexible, less expensive, more easily reprogrammable, numerically controlled machines and they naturally came to dominate the world industry. This is a very important effect of the military sourcing of R&D. On the other hand, the most successful and powerful industrial policy that the United States has ever had, concerns its civilian aircraft industry, which also has benefitted significantly from the fact that the same aircraft and engines have military applications. So there can be pluses and minuses. Realistic measures indicate a 25–35 percent decrease in real military expenditure in the U.S. over the next few years if political changes in Eastern Europe will be permanent. Some of these technological resources could perhaps be devoted to civilian purposes. We may have, not exactly a laboratory experiment, but an experiment to see whether this diversion or rediversion of resources back to civilian use will have any effect on American industry.

Gunnar Eliasson: In your introduction you mentioned that there was no psychologist on the M.I.T. project. Somebody whispered in my ear: I wonder if there was a sociologist. We have one on our staff.

Robert Solow: I said that the reason we did not have any psychologist was that M.I.T. had only one and she was on leave. But that is one more than the number of sociologists.

Gunnar Eliasson: Across the river you have Ezra Vogel, a sociologist. He wrote a book called Japan Number One. In that book he suggested that one of the reasons why the United States is having problems with R&D efficiency, in comparison with Japan, is that a large part of the U.S. knowledge allocation, as mentioned by Hans Werthén, is run through a

non-market system, that is through defense and space. The Japanese allocation of R&D spending is run through a commercial filter, the market. The U.S. has allocated a large part of its R&D talents to space and defense, while Japan has their talented engineers in manufacturing serving international goods manufacturing. Of course, you will then have an export problem. Will you enforce or weaken that proposition?

Robert Solow: I will enforce only a fraction of it. It is now a great movement to try to imitate the Japanese. There is, for example, a great movement in favor of allowing, or even encouraging cooperative research amongst firms. The firm wishing to engage in cooperative research has been required to contribute a substantial fraction of the resources itself. I think this is important. My understanding of cooperative research in Japanese industry is that each of the firms which cooperates in research is also at the same time engaging intensely in its own research on the same general ideas. This implies that the cooperating group of firms will, at the end, be competing for market shares quite intensely among themselves. I hope that any American effort to encourage cooperative research, will insure that the firms will participate financially themselves. Important is also that the firms will be encouraged, and even required, to compete afterwards in the exploitation of the technology.

Hans Werthén: This is the old question about competition and regulation. You come from M.I.T. which started modern electronics because of the war needs. A very large part of American innovation today is developed because of military needs. This part of the economy can be called an allocation economy. If you look at the Russians they have a complete allocation economy. It has worked, unfortunately, very well on the military side, but badly on the civilian side. Japan has perhaps what you could call an economic dictatorship. When you discuss modern economics and innovation, have you studied how much is normal competition, and how much is really allocation

for various predetermined purposes?

Robert Solow: I am as illiterate as anyone else about the gross figures of R&D expenditure paid for by the military. My impression of this is that most of the industrially relevant R&D in the U.S. is market directed and not allocated in any way. I take this as a good thing. If anything is allocated, it is university research in the sense that everybody is interested in what the national science foundation is interested in this quarter. The Russian case is rather interesting, but I know very little about it. There is apparently a very large contrast between the military and space R&D output in the USSR and the output of R&D in industry. This suggests that the resources available are very scarce since not much was left after the allocation to military and space.

Pavel Pelikan (IUI): I want to reformulate Hans Werthén's question somewhat. It seems to me much more general than you want to make it. You mentioned the program explaining differential rates of growth in innovation in R&D in the different countries, and then you said that neoclassical growth theory cannot explain them. The problem is that neither can Schumpeter. In both theories an institutional vacuum prevails. None of the theories accounts for institutional intermediaries in the allocation processes of the economy. The institutions may be what really matters, not the amount of money you invest. We know that there are many variants of the market economy with different properties and rules of the game. Some perform very well, others are poor. The product market, the capital market, patents, legislation, and all other factors differ tremendously within market economies. This leads to the question: Do institutions matter economically? There is a third group of theories that attacks this problem, which you did not mention. What do you think of, for example Nathan Rosenberg, Douglas North and their attempts to explain the differences by way of differences in institutional rules?

Robert Solow: I can only reply as a casual reader of that literature. I am inclined to agree that the nature of economic institutions matter a lot to the direction of knowledge creating activities, research and development. The problem with studies on this subject is that there are no degrees of freedom statistically. There are as many parameters estimated as there are institutional structures observed. The art of looking at the broad scope of history, I am thinking of Douglas North now, is that it is a very limited form of story telling. It is hard to imagine how one tests hypotheses of this scope. That is why, strangely enough, I come to the conclusion that the study of institutional structures will have to be approached with at least as much theory as observation. The intellectual structure we have to model strategic behavior is, broadly speaking, game-theoretic. In addition you have to understand what motivates actors. I do not think there is very much to learn other than the historical details, which are very important as mental furniture.

Ishaq Nadiri (New York University): Bob, I have two questions. One is that you have been emphasizing the context of the new theory of growth, or the formulation of it. The impact of new technology is changing the characteristics of the market. In fact when you talked about Krugman's model, basically a three-period operation, the second period, a certain structure of the model will be endogenously determined. The market process itself becomes endogenous in this process. The other question I have is about aggregation in growth models. What is your opinion of what might happen in the new round of this discussion in terms of model building and the economic theory of the new model?

Robert Solow: Concerning the first question, why cannot the structure of markets get modified in the process of innovation? My response to that is yes, of course it can, and I wish that some younger person would try to investigate this problem. On the question of aggregation, I am a hard-liner. I do not think it is true that the less aggregation the better. I do not think it

is true that the more aggregation the better either. I think that the kind of methods that economists can use and the kinds of results that they can hope for will be different when work is directed to different levels of aggregation. One of the standard ploys in any discussion of analytical economics is "all you need to do is to disaggregate". Perhaps you do, perhaps you need to aggregate. In this case I think it is necessary to let at least a dozen of flowers bloom.

Gunnar Eliasson: In this context I have a question about the nature of capital. We have already accepted that knowledge matters and should be part of capital input. But it is difficult, perhaps impossible, to measure. What do you think of the notion of "tacit" knowledge? If it exists, how do you incorporate it in growth theory?

Robert Solow: I have no idea. Economists, especially theorists, always worry when they come up with something which cannot be explained. They therefore name it, and think that it is explained. It is not only economists that are subject to this. I know business executives who do the same. Tacit knowledge is that sort of classification. It seems intuitively satisfying and I do not know how one can ever be certain.

Richard Day (University of Southern California): We have talked a lot about investment in industrial structure during the last few days. I wonder what you think about the role of investment in public infrastructure and welfare. I think about the gigantic education and research infrastructure, health service, sanitation services and all that. Could such investments possibly be interpreted as investments in human capital? How do these investments influence the rate of return calculations?

Robert Solow: I would like to change that subject just a little bit. The value of social infrastructure to the economy in the form of health and education is the subject of John Bishop's research. People like us normally presume that it is important

and we welcome research that attempts to quantify it. What has been neglected in economic research in the U.S. is public infrastructure in the more normal sense: transportation and communication networks, roads, bridges, harbors and so on. We do not measure those things very well in the U.S., because our government is not much given to capital accounting in any serious way. There has been some work attempting to estimate the productivity of what I call hard infrastructure investment. That I think is very worthwhile, and I wonder whether in Sweden any attempts have been made to estimate the aggregates. This is a case when aggregation is absolutely necessary. We are beginning in the U.S. to realize that in the interest of budget cutting the hard public infrastructure has been neglected for a number of years. Even without earthquakes bridges just collapse in the U.S. A serious cost benefit analysis is needed here. Is it done in Sweden? If I asked an official what is, at the current margin, the rate of return on investment in snow removal or something like that, could he give me that number?

Lennart Hjalmarsson (University of Gothenburg): Well, it depends. Most parts of the government sector performing investments in infrastructure and overhead capital, like electricity, telecommunication, stateowned administration buildings, airports, roads etc. use fairly elaborate investment appraisals based on a certain test discount rate. The Central Road Administration, for example, use a 6 percent test discount rate and cost benefit analyses in its project evaluation. However, there is no centrally determined discount rate for the public sector. As far as I know, no attempts have been made to estimate the aggregate productivity of investments in infrastructure.

In other parts of the public sector, particularly among the municipalities the answer is probably no.

Robert Solow: But are those discount rates set in any rational way?

Lennart Hjalmarsson: It is hard to tell. You need more research to figure out what research in this field is really about. In some areas, at least, the discount rates are set on the basis of recommendations from economists, e.g., on road construction and electricity.

May I ask a final question? What would you like to see on the research agenda in productivity and growth analysis for the next few years?

Robert Solow: It is a mistake to think that because you invite somebody to travel a long way to give a talk, that person therefore can say useful things on every subject. But there are two questions that have continuously been asked more and more often in the context of productivity research. The first is an obvious one. It has been with us for a long time, but nobody has bothered to, maybe except for yourself, to do very much. It is productivity measurement in non-goods producing industry. The mere fact that we speak of the service industry as an agglomerate suggests that we are not serious in measuring its output. It is far more diverse than that. I think that a serious attempt to measure productivity, which means in part to measure physical deflated output in the service sector, should be an important part of the research agenda. The other question, which is now beginning to turn up more and more frequently, is the question as to whether, when analyzing productivity change, we have adequately been able to take account of quality, design and similar things. All too often, when you talk to people who are directly concerned with marketing industry and things of that sort, it turns out that what they mean by technological progress, or even by productivity increase, is not what we measure. They talk about quality changes. I do not think that one should think about this as a simple measurement problem, it is a conceptualization problem. I would like to see some more work on that. There may as well be other things, but these are the ones which have occurred to me.

Gunnar Eliasson: Coming back to the exact title of your talk

today, and also referring back to your remark about the concerns for what the national research foundation thinks about the next quarter. Who is taking care of the long term? We had this discussion in the financial market context yesterday. Are the financial markets forcing a short-term perspective on the economy? Who is taking a long-term responsibility in society? And how is that related to the performance of markets?

Robert Solow: If there is anyone in the U.S., any group of institutions, which is responsible on the research side for thinking about the long run, it is presumably the universities. On the whole, that is the way things work out. A place like M.I.T. is especially interesting in this respect, because it has many more direct connections with industry and needs many more direct connections. When I say "needs" I do not mean in order that industry should make generous donations to M.I.T. M.I.T. has to know what is actually happening in the world of industry, to understand what the needs of industry are. This is the effective way to motivate engineering research at the intellectual frontiers of engineering. But even a place like M.I.T., which is directly involved in industrial research, manages to reserve a substantial part of its resources in engineering research for the long run. That is currently impractical research. I think that it is asking too much of industry to expect far-seeing industrial entrepreneurs to devote a lot of resources to studies of a kind which they cannot justify very easily to their shareholders. It is a good idea that there are institutions, which survive and prosper, which are not dependent on producing short-run results. This is the division of responsibilities in the U.S., and it makes sense to me.

Gunnar Eliasson: I say this was a conclusion responding exactly to the theme of today "The Rate of Return and the Rate of Interest". We have heard a very thoughtful presentation and an interesting discussion. For that I suggest that we give Bob Solow a very big hand.

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ROBERT M. SOLOW received the prize in economics in memory of Alfred Nobel in 1987. The award was given in recognition of Professor Solow's influential studies on the factors behind macroeconomic growth, and in particular his theoretical and empirical analyses of the importance and content of technical change.

Professor Solow was born in Brooklyn, New York, in 1924. He received his doctorate from Harvard University and is currently professor of Economics at M.I.T. He holds several honorary appointments and is known for his ability to present difficult research results in a language that can be understood by a wider audience. In this little book Professor Solow gives an easily accessible presentation of the most difficult problem of all, the relations between the rate of return, the rate of interest and the value of capital.

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