

SOLOW'S STRUGGLE WITH MEDIUM-RUN  
MACROECONOMICS: 1956-1995

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# Solow's Struggle with Medium-Run Macroeconomics: 1956-1995<sup>1</sup>

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## **Abstract**

Solow has repeatedly called for the development of models that combine equilibrium and out-of equilibrium outcomes or what he called a macroeconomics of the medium-run. This paper recounts the history of Solow's different attempts to address this issue. It starts in early 1950s when Solow developed his long-run growth model and it ends in the mid 1990s with the publication of *A Critical Essay on Modern Macroeconomic Theory* co-written with Frank Hahn. This narrative involves different economists associated with various research

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traditions, going from the neo-classical synthesis in the 1960s, the New Classical Economics in the 1970s and the New Keynesianism in the 1980s.

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**JEL Classification:** B22, O4, E12, E13, N1, B31

## 1. Introduction

Robert Solow won the Nobel Memorial Prize in Economics in 1987 for his analysis of economic growth. Besides the discussion of the achievements of his theory, an important part of his Nobel Lecture was dedicated to its main limitations<sup>2</sup>:

“Growth theory was invented to provide a systematic way to talk about and to compare equilibrium paths for the economy. In that task, it succeeded reasonably well. In doing so, however, it failed to come to grips adequately with an equally important and interesting problem: the right way to deal with deviations from equilibrium growth.” (Solow 1988: 311)

Since then, Solow (2000, 2012) repeatedly called for the development of models that combine equilibrium and out-of equilibrium outcomes or what he called a macroeconomics of the medium-run. The story I present here concerns Solow’s attempt to address that complex issue. My attention will be focused on his correspondence as well as on his unpublished papers. With these materials I hope to clarify the genesis of Solow’s ideas and to reconstruct his intellectual trajectory. This story takes place over a rather long period. It starts in 1956 with the publication of Solow’s seminal paper on long-run growth and ends in

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<sup>2</sup> Part of his Nobel lecture was also devoted to the limitations of the New Classical approach assuming market clearing and postulating the existence of a single representative consumer optimizing over an infinite amount of time with rational expectations (Solow 1988: 310).

1995 with the publication of *A Critical Essay on Modern Macroeconomic Theory*, co-written with Frank Hahn.

Two main ideas structure this study. First, I discovered that Solow paid much more attention to the connection between short-run Keynesian theory and long-run neo-classical growth analysis than is usually alleged<sup>3</sup>. Already from the mid-1950s, Solow was concerned with the difficulties of making the analytical connection between the short-run and the long-run. His main aim was to find a way to define a robust investment function – following a line of reasoning different from Harrod - capable of dealing with disappointed expectations of entrepreneurs once they are off the equilibrium path. Later, after having spent two years on the staff of Kennedy’s Council of Economic Advisers (CEA), Solow engaged in two research projects. The first consisted in developing an econometric model of employment determination, illuminating the short-run and long-run role of capital and the main determinants of the elasticity of employment with respect to output. The second, in

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<sup>3</sup> Recently, De Vroey and Duarte (2012) argued that Solow dismissed persistently the possibility and interest of bridging the Keynesian and neoclassical modeling.

collaboration with Joseph Stiglitz consisted in developing a short-run macroeconomic model likely to be extended to the long-run<sup>4</sup>.

In the 1970s, Solow became much less involved in this issue. The rise of the new classical approach is certainly the main factor that led him to address it again in the early 1980s. His 1985 Mitsui Lectures were an important step toward crafting a new line of reasoning. Resorting to imperfect competition, increasing returns to scale and new theories of the labor market, Solow finally came to a new approach to modeling and new insights to unify the short-run and the long-run. There is a clear line of continuity between these two sets of works. In particular, it will become clear that starting from new theoretical foundations in the 1980s, Solow ultimately aimed at dodging the obstacles that first impeded him in the 1960s.

Regarding the history of macroeconomics, focusing on Solow's attempt at developing a medium-run macroeconomics is of interest for at least two reasons. First, it contributes to a better understanding of the context of the development of the neoclassical synthesis, understood here as the integration of short-run Keynesian analysis and neo-classical long-

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<sup>4</sup> Roger Backhouse (2012) relates Solow's and Stiglitz's disequilibrium approach to the Cambridge controversy about income distribution theory. In my paper, I will argue that Solow's own motivation was mainly to develop a disequilibrium model ultimately aimed at filling the gap between Keynesian and neo-classical approaches. The present study is thus fully complementary to Backhouse's.

run growth analysis. Second, it partially illuminates the context of the emergence and development of New Keynesianism, Solow being one of its main protagonists in the 1980s.

## 2. “Stumbling towards the Medium-Run Macroeconomics” (1953-1968)

### 2. 1. In quest for a causal dynamics

In the mid-1950s, Solow developed a dynamical version of Leontief’s input-output system with constant and flexible coefficients of capital. Aimed at addressing the long-run growth issue, he came to transform this model into a one-commodity model<sup>5</sup>. Already in 1953, he expressed his willingness to address the possibility and consequences of disequilibrium with the help of this aggregative model. Attempting to construct an appropriate price theory that might go along with it, he wrote: ‘one can’t help wishing, however, for a more complete causal dynamics of the kind usual in business cycle theory’ (Solow 1953-54: 79)<sup>6</sup>. Along the same lines, in the concluding remarks of his 1956

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<sup>5</sup> See Halsmayer (2013) for a detailed examination of the origins of Solow’s long-run growth model.

<sup>6</sup> At that time, Solow (1951) had already explored the implications of adjustment lags in linear models. Just after the publication of his 1956 “Contribution”, Solow tried to make that point clear to Adolph Lowe, who urged him to clarify his treatment of the equilibrium path in the presence of constant capital-labor ratios in the short-run: “Samuelson and I have dealt with the problem of

“Contribution”, Solow acknowledged the importance of problems of short-run dynamics although his model, by relying heavily upon the assumption investment automatically equals saving, was mainly a statement of what would happen in their absence<sup>7</sup>.

Solow reiterated his determination to go further in connecting short-run and long-run modeling a few years later. His correspondence with Hahn in 1959 sheds some light on his motivations. At that time, Hahn was about to publish in the *Quarterly Journal of Economics* a paper on the stability of long-run equilibrium paths proposing an analysis of the behavior of the system when out-of-equilibrium<sup>8</sup>. The discussion focused on two models: a *Model A* - Hahn called it Wicksellian - and a *Model B* characterized by Keynesian features. Each displayed three stabilizing effects working by means of factor substitution, changes in the money interest rate, and changes in the income distribution.

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optimal capital accumulation under these circumstances in a chapter of a RAND corporation monograph which will go to a publisher soon. Even there it turns out that with the fixity of capital-labor ratios, it may, and in general will, be preferable to allow some capital or labor (but of course not both) to lie idle. In any case, my own position would be that while those bottleneck problems have their own interest and importance, a theory which claims to be about the broad sweep of long run growth ought not lean too heavily on them.” (Solow to Adolph Lowe, April 18, 1956)

<sup>7</sup> See Nikaido (1975) for a detailed discussion of the importance of that assumption with respect to the assumption of factor substitution. For a contrast between Solow’s 1956 growth model and Harrod’s theory of dynamics, see Hageman (2009) and Halsmayer and Hoover (2013).

<sup>8</sup> See Hahn (1960)

In *Model A*, the money interest rate is assumed to behave such that any perturbation of equilibrium triggers a divergence between the rent of capital and its marginal productivity while money wage flexibility is assumed to preserve, at every moment of time, equality between the real wage of labor and its full employment marginal product<sup>9</sup>. For any given expectation-formation assumption, any change in the actual level of price gives rise to expectations as to the future rates of changes in prices, entailing change in the real rental of capital and eventually, through the substitution between capital and labor, to the marginal product of capital. How, then, is equilibrium restored? The answer is found to depend on a comparison of the rate at which the real rental of capital and the marginal product of labor are changing (Hahn 1960 : 207). From the moment that the money interest rate is allowed to vary, Hahn shows that whatever the speed of factor substitution, equilibrium is certain to be stable.

In *Model B*, the assumption of perfect wage flexibility is dropped so that the real wage, when prices are varying, is assumed to depart from its equilibrium value. This has two consequences. First, during the adjustment process, the income distribution will change. Second, factor substitution will now result from divergences between the real wage of labor

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<sup>9</sup> When the production function is Cobb-Douglas, changing prices do not affect the distribution of income between wage earners and profit earners.

and labor's marginal product as well as from a divergence between the real rental of capital and its marginal product.

By paying attention to these three effects, Hahn suggests examines Kaldor's 1957 model of economic growth. What happens if the equilibrium is disturbed and an excess supply of goods occurs? Prices start falling, real wages rise and income moves in favor of labor. This – making the Kaldorian assumption concerning the propensity to consume out of profit and wages – increases demand from the consumption side (“Kaldor effect”). Meanwhile, however - regardless of any factor substitution – demand will be reduced from the investment side: “For if output falls below its equilibrium level, the capital-labor ratio rises so that even if no factor substitution were induced, investment may fall” (Hahn 1960: 208). Therefore, if the system were to be unstable under the assumption of perfect wage flexibility, the “Kaldor effect” will have to outweigh the reduction in investment and be “unrealistically large” for stabilizing the economy.

Although Solow fully agreed with Hahn's interpretation, he gave credit to Kaldor's attempt to explore out-of-equilibrium dynamics and to provide an explicit causal dynamics:

“Of course you're right and Nicky's [Kaldor's] model simply will not stand up under scrutiny. When it is not self-contradictory it is more or less completely arbitrary. As you

realize, however, there is one respect in which it is a step in the right direction. The Harrod-Domar legacy of paying attention only to equilibrium paths is by now an obstacle. All these ad hoc stability statements about what happens off such a path are useless without an explicit causal dynamics. But if we had the latter, then the equilibrium paths would appear as certain special-motion and one could deal with them directly.”(Solow to Hahn, March 23, 1959)<sup>10</sup>

However, with no adequate representation of expectations and therefore no solution to the touchy problem of the valuation of durable capital when the future is uncertain, Solow was skeptical about offering a consistent and empirically relevant solution:

“Once you have demolished Kaldor, it remains to work out an explicit more or less aggregative dynamic model with some of the characteristics of general equilibrium. My own feeling is that lacking a theory of uncertainty – and therefore, lacking a theory of investment – it is hard not to be almost as arbitrary as Nicky.” (Solow to Frank Hahn, March 23, 1959)

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<sup>10</sup> Solow knew that Harrod had addressed the problem of out-of-equilibrium dynamics. He simply was not convinced by Harrod’s treatment of investment: “Some of what Harrod called instability is, of course, a matter of the behavior of effective demand, especially investment demand, off equilibrium paths. Harrod never specified very clearly what he had in mind, and indeed there is very little in the literature even now that marries the theory of growth and the theory of effective demand.” (Solow to Helmstädter, February 11, 1966)

On the eve of his departure to Washington in 1961, just before joining the staff of the 1961 CEA, Solow was not embarrassed to admit to Kaldor he had not yet devised a way to unify short-run and long-run modeling:

“In the shortest run, given capital means given concrete items and of course you have no objection to that. In the shortest run capital is like land, unalterable, fixed, etc. In a longer run, some of the concrete items are not fixed. But something is fixed: asset preferences, savings habits, relation among these things and the rate of profits, time preferences, the amount of accumulation already done, whatever, so in a longer run, given these things, and given the technological input-output relations, the production function relating possible lists of physical inputs to possible lists of physical output, I deduce which among the possible forms of capital goods will be constructed in equilibrium. Now to get from one equilibrium to another, when a disturbance (like a change in the production function) occurs, I don’t know. But how to compare equilibrium, and to find something which can be common - described as substitution between labor and capital, I do know.” (Solow to Nicholas Kaldor, January 30, 1961)

## **2. 2. Solow’s involvement in the CEA (1961-1963)**

The new CEA appointed by President Kennedy was composed of the chairman Walter Heller and the members James Tobin and Kermit Gordon<sup>11</sup>. Solow joined the staff at the request of the Council in which he spent two years. Not yet fully organized in January 1961, Kennedy's new Council was immediately assigned major responsibility for elaborating the program the President set forth in his economic message to the Congress, transmitted on February 2, 1961. Its other immediate task was the preparation of a statement for the regular hearings of the Joint Economic Committee on the annual Economic report<sup>12</sup>. Broadly, this statement was the analytical exposition of the administration's general approach to economic policy. Three decades later, Solow and Tobin characterized it this way:

"The authors of these 1961 and 1962 reports were all, despite individual differences of interest and emphasis, exponents of the synthesis of Keynesian and neoclassical economics developed over the previous fifteen years. American economists, notably Samuelson, played leading roles in this intellectual development. By 1961 it was becoming the mainstream of

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<sup>11</sup> According to Solow (Solow 2004: 658), "James Tobin was clearly the intellectual leader" of the Council, which relied heavily on outside economists. Joseph Pechman and Paul Samuelson were constantly consulted. Others who assisted in writing the 1962 report included Kenneth Arrow, Otto Eckstein, Peter Kenen, Robert Lampman, Charles Schultze, and Robert Triffin.

<sup>12</sup> In addition to this statement, the Committee took extensive testimony, oral and written, from the three Council members.

macroeconomics. The council sought to set forth its principles and to apply them to the United States of the 1960s, in language accessible not just to economists but to all earnest readers.” (Solow and Tobin, 1988: 5)

For Solow, this synthesis meant the possibility of addressing both short-run and long-run issues without assuming for all that the economy possesses any tendency to adjust “automatically” towards a full employment state. Returning from England in 1964, this is how he clarified that point to Amartya Sen:

“I got a little annoyed in Cambridge last year by the indiscriminate use of ‘Keynesian’ as adjective meaning ‘mine’ and ‘neo-classical’ to mean ‘yours’. To the extent that “neo-classical” describes the belief that a capitalistic economy tends automatically to full employment, I am no neo-classical and neither is James Meade.” (Solow to Sen, October 26, 1964)<sup>13</sup>

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<sup>13</sup> Meade’s exposition of the neo-classical growth theory was indeed based on the assumption of “ideally successful” monetary and fiscal policy at every point of time managed to insure full employment (Meade 1961: ix). In the same spirit we can read in Swan’s reflection on “golden ages” that his “illustration will be Keynesian, in the sense of the future as Keynes did, and assume either that the authorities have read the *General Theory* or that they are socialists who don’t need to; in other words I assume that whatever is saved is invested.”

A year later, Solow again felt the need to clarify that idea in a letter addressed to Stiglitz,<sup>14</sup> who was at that time at Cambridge (England) for one year:

“When Samuelson or Tobin or Meade or I work out what happens if the system is left at full employment, we are supposing that the government sees to it that - [autonomous investment] or personal tax rates or something else is maneuvered so that the solution of the equations [defining the short run Keynesian system] always tracks full employment. [...]. Among us girls we can keep that in mind.” (Solow to Stiglitz, September 28, 1965)

The analysis of structural unemployment served as a foundation for this view. For this reason, the CEA included in the *American Economy in 1961* a thorough statistical analysis whose main finding was that demographic and industrial breakdowns of unemployment showed no evidence that structural unemployment was increasing:

“When I went to work for the Council of Economic Advisers in January 1961, one of the first things I had to do was to evaluate the argument over structural unemployment. My interest in the whole problem dates from then. We included something on it in our first public document, a statement to the Joint Economic Committee in March 1961. That same

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<sup>14</sup> At that time, Stiglitz took his Ph.D. under Solow on problems of economic growth, capital accumulation, and the dynamics of the distribution of income.

day or the day after, Martin [William McChesney], Chairman of the United States Federal Reserve Bank] testified and an important part of his argument was that even at that time an expansionist policy would be dangerous because of the large and rising volume of structural unemployment. Paul Douglas [Senator] observed that we and Martin had given diametrically opposed arguments and he made quite a stir about it.” (Solow to Gottfried Haberler, November 30, 1964)<sup>15</sup>.

This debate was closely related to the analysis of the shape and the location of the Phillips curve that Solow, in collaboration with Samuelson, expounded upon in their 1960 seminal paper. Arguing about that point with Leeson, Solow held that “the CEA was concerned almost entirely with locating a target unemployment rate compatible with reasonable price stability. I know: I was there.” (Solow to Robert Leeson, January 6, 1993)

Between 1961 and 1963, the CEA introduced a series of demand management policies, including counter-cyclical fiscal and monetary policy, deficit financing, a full employment goal, and a major tax cut : “Taming the business cycle and maintaining full employment were the first priorities of macroeconomic policy. But this should be done in ways that promote more rapid growth in the

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<sup>15</sup> Barber (1975) provides a detailed analysis of the divergent initial perspectives among the staff of the CEA.

economy's capacity to produce. We were not one-eyed demand-siders. Demand calls the tune in the short run, but advances in supply determine long-run progress.” (Solow and Tobin, 1988: 5-6)

Convinced that the government had critical control over the level of employment, by both implementing structural reforms and aggregate demand policies, Solow might have felt less urgency during these two years at the CEA to address the connection between short-run Keynesian and long-run neo-classical theories. Once back in academia, however, he soon came to change his mind.

### **2. 3. Making the analytical connection between the short-run and the long-run**

From 1963, Solow's main concern was to give a good theoretical explanation to the procyclical behavior of productivity revealed by several econometric studies from the NBER (Hultgren 1960) and MIT (Kuh, 1965). In his 1964 presidential address to the Econometric Society, he framed this empirical finding as a critical puzzle for economic theory<sup>16</sup>:

“Its importance goes far beyond the desirability of being able to predict how output per man hour will change from quarter to the next. Art Okun or Ed Kuh or Dan Suits can already do that. What I am looking for is a way to unify the economics we teach our students

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<sup>16</sup> Biddle (2013) examines in detail how this empirical finding was later woven with the labor concept hoarding.

and the economics we use when we advise governments and analyze passing economic events, and do it in a way amenable to econometric treatment. This patching up of theory to explain experimental uniformities is the way science usually proceeds.” (Solow 1964: 29-30, skipping inserted page 29a)

There were in particular “two big paradoxes” whose resolution was supposed to be a “major step toward the unification of long-run and short-run theory.” (Solow 1964: 20). The first was the “unsatisfactory” role of the capital variable. The notion that capital is a substitute for labor would suggest that, both in the short-run and the long-run, the bigger the stock of capital, the smaller the level of employment corresponding to any current output. This role of the capital stock did not, however, stand out in econometric tests. The other paradox was the persistence of what appears to be “long-run increasing returns with respect to labor alone”. When the production function is of the normal shape with diminishing returns to the variable labor, one would expect a sustained 1% increase in output to be accompanied ultimately by a larger than 1% increase in employment, less an allowance for technical progress. Instead, observations showed that the long run elasticity of employment was always lower than the one in the econometric test.

For addressing these two paradoxes<sup>17</sup>, Solow suggested using a simple version of the growth model he had just developed in collaboration with James Tobin, M. Yaari and Christian von Weizacker (Solow 1964: 22). The argument was based on empirical evidence that the short-run substitutability of labor and existing concrete capital goods is very small. If that is so, then short-run increases in output will be accommodated not by working already-active capital more-intensively, but by starting up previously idle capital. In an econometric development, Solow specified that there is at any instant an extensive margin between units of capacity ranked according to their efficiency, this margin moving up when output falls and down when output rises. As a consequence, in the short run, the relation between output and employment is essentially independent of the features of the stock of capital<sup>18</sup>: “That, in turn, depends on lots of things, including the age composition of existing capital.

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<sup>17</sup> Already in 1963, Solow presented a paper at the European meeting of the Econometric Society in Zurich in September in which he attempted to contribute a “ few unsystematic remarks about the long-run and short-run role of capital.” (Solow 1964: 20)

<sup>18</sup> A few years later, Solow emphasized that point this way: “I can’t really believe in short-run fixed coefficients for the aggregate economy. I do believe that, for any concrete existing plant, output is probably nearly proportional to employment. There would, therefore, be fixed coefficients if all plants were alike in technological characteristics. But I seems much nearer the truth to say that some plants are more efficient (i.e. have higher output per man hour) than others, usually because they are newer and therefore technologically more advanced, but perhaps sometimes because they were constructed to cope with a high real wage. Whatever the reason, if plants differ in productivity, the less efficient ones will become idle first when output falls. The result is exactly like conventional diminishing returns to labor in the short run.” (Solow to Vanek, June 2, 1967)

But it does not depend in any uniform way on the absolute volume of capital, active and stand-by, that happens to be in existence.” (Solow 1964: 24).

Concerning the second paradox, this model did not offer decisive help. Solow could show that the long-run elasticity of employment was lower than the one consistent with the assumption of the neoclassical long-run production function but could not get an estimate of the long-run elasticity to be less than one.

Solow was well aware that this econometric approach was only a modest step toward the resolution of the problem of the unification of the short-run and the long-run<sup>19</sup>. In 1965, he informed Svenlison, who was developing a model of cyclical-growth, about his aim to go further:

“Economics badly needs a unification of long-run production theory and short-run income analysis and I agree with you that this is the way to do it. Jim Tobin and I and two of our young colleagues have written a rather long paper along similar lines. [...] My personal project now is to put that model together with a system of effective demand determination,

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<sup>19</sup> On January 1965, he admitted to Walter Heller his difficulties in crafting a solution grounded on theory: “I talked about short-run movements in productivity in manufacturing and how this fits in with what we think we know about the long-run. It was one of those unsuccessful attempts to please both the rigorous and the vigorous. I have not written it up finally yet, but if I do I would love to send it to you.” (Solow to Heller, January 29, 1965)

the obstacle is exactly those short-run fluctuations in productivity I talked about at the Mandelshochuhiele in Stockholm” (Solow to Ingvar Svennilson , July 12, 1965)

In the 1966 published version of this paper, a short section examining the functioning of an economy limited by effective demand was finally added. It is, however, clear that Solow and his colleagues had made no decisive step towards the economics of the medium-run. Meanwhile, a letter sent to Hahn revealed that Solow still wondered how to tackle the problem of expectations in a dynamic model

“I [...] want to spend this summer getting back to honest research on the subjects that really interest me. Mainly non-steady states in general and the integration of effective demand and growth in particular. It remains to be seen whether my shattered mind can get anything done. One of the things I hope to do this summer is come to grips with your papers on many capital goods. The fact that most paths don't converge to any steady state is not surprising in view of the catenary character of efficient paths. What is needed, as you saw clearly, is a decent treatment of the determination of short run equilibrium in the absence of a complete set of perfect future markets.” (Solow to Hahn, June 28, 1966)

Aiming at addressing the problem of expectations and of “perverse” cyclical productivity, Solow came, in collaboration with Stiglitz, to develop a new “theory of underemployment

equilibrium”. In regards to the productivity problem, Solow believed that this theory was quite successful:

“We account for the ‘perverse’ cyclical behavior of productivity in two ways. We make employment adjust to changes in output with a lag, so that sometimes labor is hoarded and sometimes worked abnormally hard. In addition, we do not require the real wage to equal the marginal product of labor at all times; indeed whenever output is limited by inadequate demand prices may exceed marginal cost (which is the same thing as the wage being less than the marginal product of labor), though one would expect the price level to fall.” (Solow to Jaroslav Janek June 2, 1967)<sup>20</sup>

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<sup>20</sup> Thirty five years later, at the occasion of Joseph Stiglitz's 60th birthday conference, Solow (2003) prepared a paper entitled “Stumbling towards the Medium Run Macroeconomics” in which he wondered why his paper co-written with Stiglitz did not draw more attention: “Perhaps the Solow-Stiglitz paper left everyone cold because it was explicitly motivated by the wish to understand the precise relation between the neoclassical theory of distribution (factor prices related to factor supplies and technology via marginal products) and the theory espoused by Nicholas Kaldor and Joan Robinson (factor prices – or shares – related to differential saving rates for wages and profits). We showed that it all turned on whether the function of factor prices was to clear factor markets or to clear goods markets, and thus ultimately on whether aggregate output was in any particular instance limited on the side of aggregate supply or aggregate demand. Eventually all this came to appear merely quaint and not a part of serious macroeconomics. One of my goals here is to free the model from this distraction altogether.” (Solow 2003: 272). The correspondence between Solow and Stiglitz confirms Solow’s recollection. Stiglitz mentioned three main motivations for their paper: “(a) the doctrine question of the relation between the two theories of distribution; (b) construction of a macro-economic model which did not depend on the obviously empirically invalid assumption of rigid money wages and which did allow for inflationary and unemployment equilibria; (c)

Though the model was clearly a short-run model defined for a given stock of capital and a given level of investment which shunted aside the issue of long term expectations, it led a step further toward medium-run macroeconomics. Indeed, by bringing into the short-run picture demand-limited and supply-limited equilibrium, the model could help economists to reconsider the way the economy, depending on its initial position and the macro policy implemented, could move to its long-run path.

It is worth paying attention to this point by considering that model in more detail. The Solow-Stiglitz paper approached aggregate supply through a short-run production function for a given stock of capital. It assumes short-run diminishing returns to labor alone, because less efficient capacity must be drawn into use at higher levels of output (Stiglitz and Solow, 1968: 539). It follows that whenever actual output is determined on the supply side, current output and employment is lower the higher the real wage. Regarding aggregate demand, the model was based on an IS curve with the marginal propensity to save from wage income smaller than that applying to profits. Investment spending was left exogenous. The obvious

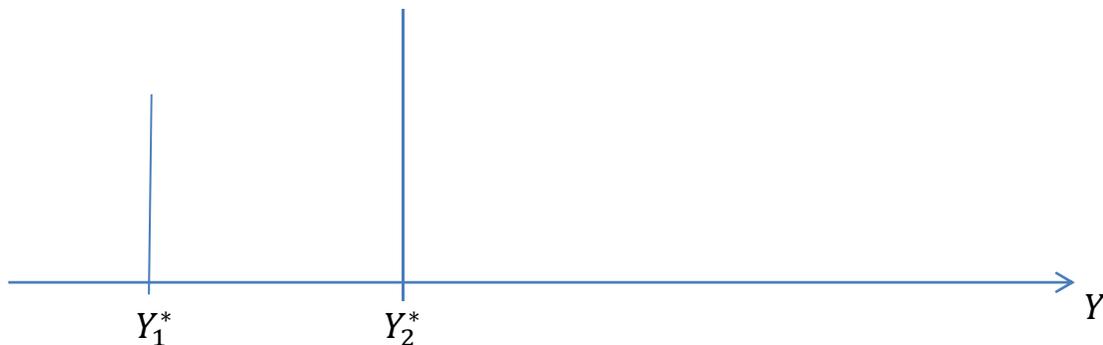
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constructing a macroeconomic model which did not require real wages to go up in depression. The second and third need I think to be somewhat strongly stressed” (Stiglitz to Solow, October 23, 1967). To this letter, Solow replied: “By the way, I agree completely with your point that the Introduction ought to stress all those motivations for the paper, including the need for a theory of underemployment equilibrium without rigid wages, and an explanation of why real wages do not necessarily fall when effective demand rises. (However, our theory is still a “rigid wage” theory in the sense that the wage does not clear the labor market instantaneously.)” (Solow to Stiglitz, 1967)

implication was that aggregate demand appears as an increasing function of the real wage<sup>21</sup>. The model postulated Phillips-like equations for both the inflation rate and the rate of change of nominal wages. The main influences on the rate of change of money wages were assumed to be the unemployment rate and the rate of inflation while the two determinants of the rate of inflation were assumed to be the ratio of aggregate demand to aggregate supply and the rate of change in money wages. The solution to these two equations gave the dynamics of the real wage. If the rate of inflation,  $p$ , is a “non linear function of the rate of capacity utilization, and therefore of  $Y$  [output level] in the short run” (Solow 1969: 40), we get this configuration:



<sup>21</sup> In a slightly simplified version, Solow (1969) discussed the relationship between real wages and aggregate demand with endogenous investment: “For a given amount of output and employment, a higher real wage necessarily redistributes income from other forms toward wages [...]. One ought to offset against this upward shift in the consumption function the possibility that the higher real wage reduces the prospective profitability of investment. If it does, the investment schedule may fall at each level of output. The net effect on the combined aggregate demand schedule is moot in principle, but one supposes that the consumption effect will usually outweigh the investment effect. In that case, aggregate demand is an increasing function of the real wage. In circumstances where actual current real output and employment will be higher the higher the real wage.” (Solow 1969: 8) “I would not expect this relation to be very strong (by which I mean I would not expect the elasticity of real aggregate demand with respect to the real wage rate to be very large. [...] Not much is known in fact about differences among the marginal propensities to spend different sort of incomes, primarily because there are no official data on disposable income by income-type. In the absence of evidence, the safe course is probably to regard aggregate demand as a gently function of the real wage. “(Solow 1969: 41)



Solow's 1969 diagram

To the right of the smaller equilibrium, the rate of inflation,  $p$ , exceeds the rate of change in the money wage,  $w$ , so the real wage falls. To the left, the rate of changes in the money wage exceeds the rate of inflation, so the real wage increases. The larger stationary point is just the opposite. In this non-linear case, the relation between the real wage and current output is hence U-shaped (Solow 1969: 40). Suppose now that aggregate demand considerations predominate at low levels of output. In that case: “There is plenty of spare capacity and unemployment. Real output would be higher if there were a market for it at the going level of prices. In this state of affairs, a higher real wage will expand aggregate demand.

It will also reduce margins, but since the limit of output is on the demand side, output will expand. If aggregate demand is only a very slowly increasing function of the real wage, then this effect will be small.” (Solow 1969: 43). At very high level of output, there is more likely to be excess demand while aggregate supply considerations will predominate: “Real output will be higher if it could be profitably produced at the going wage-price. In this state of affairs, a higher real wage corresponds to a lower level of output” (Solow 1969: 43).

Depending on initial conditions, the economy can hence be stabilized either in an excess supply ( $Y_1^*$ ) or an excess demand equilibrium ( $Y_2^*$ ). In the first case, the short run equilibrium is demand-limited; there is excess supply in commodity markets and unemployment in the labor market. In the second case, the equilibrium is supply-limited. In each case, the price level and money wage are rising. They are rising, however, at the same rate so that the real wage is not changing. The distribution of national income is also constant. Hence, with a multiplicity of equilibrium:

“The interesting possibility emerges that the economy might be jolted out of an underemployment equilibrium and transferred to a new ‘initial position’ from which it might find its way to an inflationary excess-demand equilibrium, or vice versa.” (Solow 1969: 47)

For dealing with the long-run dynamics, the model needed to be extended in two directions. The first extension concerned the assumption related to the capital stock. Solow's reply to Janek, who had developed a model attempting to connect growth and the cycle, makes it clear Solow had yet to amend his model by dropping the assumption of exogenous investment:

“We treat investment as exogenous in the short run and assume that everything takes place fast enough so that the stock of capital) can be taken as fixed, while wages and prices reach an equilibrium. We would do better if we incorporated a theory of investment, as you do, and maybe someday we will.” (Solow to Janek, June 2, 1967)

Obviously, such a change, with changing wages and prices, should imply the integration of a monetary sector:

“We have already mentioned some directions in which this theory needs to be extended. First and foremost, it needs a monetary mechanism. We have refrained from providing one in this exposition to keep the analysis two-dimensional. Under our assumptions, the dynamics and comparative statics could be analyzed in terms of the real wage and the level of employment. As soon as an explicit monetary system is introduced the analysis will have to be three-dimensional, in terms of the money wage, price level, and employment. We do

not think that offers difficulties of principle, but there will be a loss of transparency. We intend later to extend the model in this direction.” (Solow and Stiglitz 1968: 559)

In the following year, Solow did not pursue these two lines of extension although there is strong evidence that the extension of this model to the medium-run was central in his view. As co-organizer of the 1970 conference of the International Economic Association on growth, Solow especially tried to rally economists on that topic<sup>22</sup>. A letter addressed to Hugh Rose reveals Solow’s mindset during that period:

“The international Economic Association is planning to hold a small conference on Growth Models, to take place in or near Jerusalem for about 8 days beginning near the end of March, 1970. On behalf of the Program Committee, I would like to invite you to write one of the paper to be discussed at that conference. We were particularly hoping that you would write on the incorporation of effective demand in growth models. It is not specifically the monetary side that we had in mind – we may try to get a survey paper on that subject too – but the more general problem of giving a long-run model a demand side capable of producing unemployment and idle resources.” (Solow to Rose, September 18, 1969)

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<sup>22</sup> Solow’s main preoccupation was not the emerging literature about money and growth, whose main protagonists were Mirrlees, Uzawa, Stein, and Fisher.

It is worth seeing how Solow assessed the value of the work of Rose:

“For the past few years he has been working on one of the central problem of macro theory. The theoretical structure as it stands contains a gap. There is a long run theory that analyses mainly the evolution of aggregate supply and slight demand consideration. There is also a short-run theory that pays little attention to the aggregate supply of commodities but makes a detailed investigation of the determinants of aggregate demand. Rose has writing a series of papers attempting to bridge this gap, and is now working on a book with the same objective. I am not sure that his solution will satisfy everyone. But his work will certainly be received as a major contribution on an important subject.” (Solow to Kindball, March 10, 1970)

Solow’s latest attempt to focus on the medium-run issue is attested by his proposal to the National Bureau of Economic Research in 1971 in which he expressed his intention to provide a new model which accounts for the movement of short-run productivity by paying attention to the fluctuations in the utilization of the capital stock and from then to “find a good way of splicing this model of what happen in cyclical fluctuation to a standard model of what happen in periods of more or less steady-growth” (Solow’s National Bureau of Economic Research proposal, March 5, 1971). Solow later produced a new explanation of

the movement in short-run productivity in 1973. It is, however, only in the mid-1980s, starting from new theoretical foundations, that he achieved this second goal<sup>23</sup>.

### **3. Starting from New Foundations: Imperfect competition and increasing returns (1981-1995)**

During the 1970s, the New Classical Macroeconomics approach started dominating the macroeconomic research agenda<sup>24</sup>. In his Presidential Address to the American Economic Association delivered at Atlanta in 1979 as well as in his lecture delivered at Queen's University the same year, Solow tried to knock this approach off its pedestal. This dismissal involved three lines of arguments. First, he disqualified the idea that market clearing was the unshakable principle of macroeconomics. Next, he renewed the analysis of the functioning of markets and especially of the labor market. After completing both of these steps, the remaining problem was to explore medium-run macroeconomics. Solow thought he could accomplish this by resorting to imperfect competition and increasing returns to scale<sup>25</sup>.

#### **3. 1. Questioning the market clearing assumption**

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<sup>23</sup> In 1973, Solow, in collaboration with Alan Blinder, focused on the long-run effect of fiscal policy based on an IS-LM model in which the capital stock was an argument of the investment function. Although it addressed the issue of the connection between the short-run and the long-run, it was not related to the ideas underlined in the 1968 paper.

<sup>24</sup> See Hoover (1992) for accounts of the development of new classical thinking.

<sup>25</sup> Solow also attempted to repel the assaults of the Post-Keynesians. But his main opponents were the New Classical economists.

In a reply to a letter by Allan Meltzer about his Presidential Address, Solow advocated for his methodological commitment in favor of non-market clearing in those terms:

“My interest in explicit disequilibrium theory reflects my inability to resist the idea that what we actually see in recessions is excess supply of labor and goods-in general. If the serial correlations were very small or vanished after a few weeks, I would be willing to say OK, call them random fluctuations around “some sort” of longer-run equilibrium. But there’s enough persistence so that my eyes report to me that excess supply does not correct itself in negligible time. I translate that as persistent disequilibrium and I naturally want to model it.”  
(Solow to Allan Meltzer, January 26, 1981)

It is precisely that observation which is at the core of Solow’s research strategy that Grossman intended to undercut<sup>26</sup>. In his view, questioning the market clearing principle was simply inconsistent with the general theory of neoclassical economics.

“You observe that markets fail to clear, where I ‘observe’ that the sun revolves around the earth. Both ‘observations’ are suspicious for the same reasons: [...], they are inconsistent

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<sup>26</sup> Before adopting the New classical methodology, Grossman, with Robert Barro, had published an influential article in 1971 on disequilibrium macroeconomics.

with the general theory of neoclassical economics in one case and the theory of gravity, in the other case.” (Grossman to Solow, February 13, 1981)

But according to Solow, the assumption of market clearing cannot achieve the status of a universal principle. It is an assumption no more legitimate than the assumption that markets do not clear:

“Persistent disequilibrium does not violate neo-classical general equilibrium [NGET] theory in quite the same way that the other [the revolution of the sun around the earth] violates the theory of gravity. NGET doesn’t deduce market clearing from some other verifiable assumptions. It just assumes market clearing. The theory of gravity doesn’t assume helio centrism but deduces it.” (Solow to Grossman, February 20, 1981)

Meanwhile, Solow was preoccupied with the disentanglement of the market clearing assumption from the notion of equilibrium.

“The choice of a disequilibrium or equilibrium description of events is to some extent a matter of analytical tactics. There is a sense in which any situation in economics can be described as a kind of equilibrium, in the sense that any agent could have done something differently from what he is now doing, so that there is an element of Nash equilibrium in whatever happens. Malinvaud prefers to describe his set-up as “equilibrium with quantity

rationing.” There is the thing in a name. I do find too free use of “equilibrium” however evokes automatic responses that are only known to be appropriate to Walrasian equilibrium.” (Letter to Meltzer, 1981).

Referring to his work in progress on Nash-bargaining and the real wage employment relationship, Solow tried to make that point clear to Grossman<sup>31</sup>:

“ I don’ know if McDonald-Solow is to be thought of as an equilibrium model. It wouldn’t bother me if it were – I am not trying to maintain purity of any kind. [...]. Any model of bilateral bargaining must lead to some sort of “equilibrium” if it doesn’t lead to a strike or other breakdowns. But it needs have nothing to do with Walrasian equilibrium, and need have no properties of social optimality. (Beside I want to use the model account for the “habitual wage rigidity”). It could then be embedded in a model of non-clearing markets.” (Solow to Grossman, 1981)

A few years later, Solow raised the same argument in the first part of his Mitsui Lectures given at the University of Birmingham in 1985. Arguing about the necessity to start from the assumption that the labor market does not clear, he wrote:

“One ought not be too defensive about this. It is not as if neo-Keynesian are brashly proposing to abandon the tried and true Walrasian model of the labor market in favor of

some baseless alternative. The Walrasian model of the labor market has been tried all right, but it is precisely because it is not true it is shaky. Economists familiar with real-life labor markets have never liked it. The burden of proof is not entirely on neo-Keynesians. That would hardly be worth saying, except that it is not uncommon to find neo-Walrasians making arguments that boil down to the assertion: What you are saying must be wrong because it violates my assumption.” (Solow 1985 : 8)

Solow’s reluctance to resorting to the market clearing assumption did not, however, mean he rejected the assumption of rational expectations, a point he raised in his presidential address to the American Economic association and gladly shared with his young colleague, John Pettengill<sup>27</sup>:

“I have been preaching for sometimes that “rational expectation” is an imperfect frill on macro theory and that the powerful consequences attributed to it really come from an assumption about market clearing wages and prices; [...]. I think you will also be interested in

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<sup>27</sup> “Actually, as I have tried to indicate, nearly all the work is done by the much more sweeping (and even less credible, if I may intercept a little partiality) assumption that the economy is almost always in equilibrium.” (Solow 1979: 342). Along the same lines, wondering what to call the New classical economics, Solow wrote: “the customary nomenclature describes such people as “rational expectations” but that label is dangerously misleading. Rational expectation is an interesting hypothesis that may be true in one context and false in another, but is not what does the theoretical work here.” (Solow 1985: 6)

my presidential address in the March *American Economic Review*.” (Solow to John Pettengill, March 11, 1981)

Solow developed several lines of reasoning for explaining why the labor market may fail to clear. His view was that the background of optimizing agents was capable of yielding quite nonclassical results as soon as the constraints faced by the agents were well specified. Several mechanisms were explored<sup>28</sup>. Solow was, however, a bit annoyed by the absence of a unified theory about why prices and wages may fail to clear the market<sup>29</sup>:

“I confess that I don’t know exactly what to think about the labor market or more precisely exactly to model it for macro. I suppose that’s why I keep trying thinking on for unconventional clothing. In fact several such models have been prepared, and they have

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<sup>28</sup> “What I’ve been doing is applying standard and non-standard bargaining theory concept to the wage-bargaining context to see under what circumstances the negotiated wage tends to be insensitive to product-market conditions. We have some results (I have an Australian collaborator) but haven’t written up a draft yet. I’ll send it to you when I do. I don’t think our stuff is the complete answer either.” (Solow to Pettengill, March 5, 1980)

<sup>29</sup> Solow had been working on labor markets in three papers: “On Theories of Unemployment”(1979), “Wage Bargaining and Employment” (1981) and “Insiders and Outsiders in Wage Determination” (1985).

attractive features, [...] I am not yet satisfied by what we have done it quite right.” (Solow to Hahn, 1980)<sup>30</sup>

Despite this lack of homogeneity among alternative theories of the labor market, Solow thought it possible to renew the analysis of medium-run macroeconomics by resorting to imperfect competition and increasing returns.

### **3.2. Medium-run Macroeconomics reconsidered**

Solow returned to the issue of medium-run macroeconomics in the fall of 1983 with a bi-sectoral model, a model that he first presented in Cambridge (U. K.), in January 1984, and at Oxford (U. K.) in the spring of the same year. In 1985, in his three Mitsui Lectures at the University of Birmingham, Solow came to write an updated version of that model.<sup>31</sup>

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<sup>30</sup> It is again that point that Solow raises a few years later to one of the protagonist of the New Keynesian approach: “I agree that one needs a theory to explain how the labor market can be at rest with the wage exceeding the marginal revenue of leisure. There are several such theories on offer though I’m not entirely happy with any of them. Meanwhile we can’t suspend thinking about the rest of macro. That’s why I think it is legitimate to ask how the rest of the economy might behave if there is involuntary unemployment for whatever reasons.” (Solow to Richard Startz, March 24, 1986)

<sup>31</sup> In the introduction to these Lectures, Solow acknowledged that he had been influenced by Frank Hahn and Martin Weitzman and had benefited from the help of Marco Pagano Solow recalled in the foreword that his collaboration on macro issues with Hahn went back to a long time ago. Solow noticed he had been influenced by Weitzman’s 1985 paper on increasing returns and

This model contains distinct capital and consumption goods sectors. The industries that produce investment goods are assumed to be competitively organized while monopolistic competition and increasing returns prevail in the industries that produce consumption goods. Capital goods are produced by labor alone under constant returns while consumption goods are produced by labor - under decreasing costs - and a disappearing stock of capital. Regarding the household sector, Solow resorted to a two-stage overlapping generations model and assumed that there is a fixed, inelastic supply of labor. In the context of economic fluctuation, this device implied a “serious” limitation on times-scale limitation. Because each generation is assumed to pass through two stages – young and old – it results each period of time was about half a life-time in length:

“Thus if economic fluctuations occur, or entry and exit of firms, or any inter-temporal events, they necessarily occur in a succession of (roughly) twenty-five year periods. [...] My friend Hahn tells me: better a muddle about time periods than a muddle about saving and inter temporal budget constraints. I agree, but grudgingly.” (Solow 1985: 17)

In that framework, Solow divided the analysis into three time-horizons: a short-run, a medium-run and a long-run. In the short-run, neither entry nor exit of firms takes place.

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imperfect competition while he acknowledged he had benefited from the technical assistance of a young assistant at MIT, Marco Pagano.

The incumbent firms make whatever maximum profit that current demand conditions permit. In the medium-run, profits have an opportunity to generate either entry or exit of firms, after which the economy is driven toward a long-run equilibrium given by the zero profit condition (Chamberlinian tangency solution).

Since there is imperfect competition and decreasing costs, entry will involve a struggle for market share. As a result, the outcome may be risky and problematic:

“There may well be temporary losses if the struggle is fierce and prolonged. There may even be worse: you and I may suspect – and potential entrant may fear – that the outcome of this medium-run process will be another long-run equilibrium configuration with zero profits. So the costs and risks of entry investment have to be recouped in the course of the medium-run struggle, and that is not guaranteed. » (Solow 1985: 40)

This point is of great analytical convenience for tying the short-run and the long-run. Under the assumption that capital is liquidated each period, each entry is akin to a lumpy investment in new capacity. The model allows economists therefore to examine the effects of the change in the capital stock out of the long-run equilibrium path. More precisely, this “process of entry-cum-investment” illuminates the endogenous market forces that drive the economy toward any of the multiple long-run equilibriums. Naturally, in this kind of model,

local dynamics in the medium-run will fairly depend on “animal spirits”. In that respect, this model comes very close to Keynes’s conception of dynamics and long term expectations. As for Keynes’s conception, expectations will govern both the choice among long-term equilibrium states and the timing of cyclical recovery:

“This is an important point: it is not possible in this model to know where the short-run-medium-run-long-run process will ultimately wind up. Animal spirits may play an essential role. That does not mean that the outcome is merely random, or undetermined, or inaccessible to thought. But the long run state is not fully determined by the general properties of the model. To find out more requires specific investigation of specific circumstances, what sailors call “local knowledge.” One cannot depend on factors of such a level of generality that they would normally include in a basic model.” (Solow 1985: 56)

It is worth seeing how Solow attempted to make clear the specificity of that approach with respect to the purely endogenous dynamics approach advocated by his old friend Richard Goodwin:

“I am likely to see shocks where you see endogenous dynamics. I am less interested in cycles than you are, and more interested in the possibility of multiple equilibriums, among which the system circulates in accordance with shocks and local dynamics. Ah well, churches

always have these little differences of opinion. But if you think I am going to let your branch have the Italian peninsula while I take Constantinople, you are barmy.” (Solow to Richard Goodwin, 1989)<sup>32</sup>

It is now time to see how that approach helped Solow tackle some central problems he encountered in the late 1960s. Remember, at that time, Solow’s main goal was to incorporate the process of capital accumulation as a first step toward connecting modeling of the short-run and the long-run. Solow thought in particular that the stock of capital, built up by investment decisions and long persisting afterwards, was the main link between the short-run and the long-run. The problem is that there is no general and tractable way to treat the valuation of durable capital goods when the future is uncertain. The longer the effect of a current decision will last, the more that decision will be affected by uncertainty, and the larger the number of inferences that the decision maker has to risk until the game is over. It

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<sup>32</sup> It is precisely by developing models along these lines that Solow thought he could address the “central problem of macro theory”: “My nominee for the central question is to understand how the economy can get stuck in a sub-optimal equilibrium or near-equilibrium for purely macroeconomic reasons. So I am especially interested in models that account for multiple equilibria, some better than others. Maybe that can be characterized as neutrality of a different sort. The fundamental reason may lie in the goods markets, as in papers by Kiyotaki and Heller, rather than the labor market. Something must be afoot in the labor market, because we have to account for ubiquitous unemployment. But the goods market can be the source of self-justifying inferior equilibria.” (Solow to Cross, December 11, 1990)

is precisely for that reason that Solow found it so hard to handle durable capital out of steady states in the 1960. Hence by assuming that the capital goods wear out in one period, Solow was able to circumvent the difficulty. As soon as one thinks of firms as “lumpy investment in new capacity”, it becomes possible to relate the creation and destruction of capital capacity to entry and exit of firms and to explore the medium-run dynamics without resorting to any particular conception of expectations. It is worth seeing that it is precisely this argument that was central in Hahn and Solow’s 1995 book.

“For some purposes, the tradition in economics is too identify the firm with the ‘entrepreneur.’ For macroeconomics, it may be more to the point to identify the firm with its durable capital. That is why the firms that appear in the book are so ephemeral and shadowy. They come into existence unencumbered by any leftovers from history; they finance and perform some investment, produce and sell goods, and then die off, essentially.” (Solow and Hahn 1995: 142)

The model constitutes also a major step toward a consistent analysis of the “perverse” behavior of the real wage in the short-run and the long-run, a problem closely related to the dynamics of short-run and long-run productivity analysis that Solow explored in the 1960s (Solow 1964: 4). Indeed, in the long-run, the model concludes that employment and the real

wage will be positively correlated across zero-profit equilibriums but will be either negatively or positively correlated in the short-run. In this way, the model offers a plausible explanation of the empirical puzzle which attracted Solow's attention in the 1960s, that econometric tests uncover no reliable correlation between the real wage and output in the short-run and increasing returns in the long-run (Solow 1985: 55). As a result, there is no good reason to expect that any policy which would try to reduce the real wage - by reducing the nominal wage - would drive the economy from a position of unemployment equilibrium to something better (Solow 1985: 54). Ten years later, developing a variant of this model, it was precisely this conclusion that Solow and Hahn were keen to highlight (Solow 1995: 135).

#### **4. Conclusion**

In this paper, we focused on Solow's attempts to develop a macroeconomics of the medium-run. We argued that there was a remarkable continuity in Solow's work. From the mid-1950s, Solow developed a vision that the standard macro model was not functioning in the short-run to medium-run – when the capital stock is allowed to vary - in a manner consistent with neo-classical theory. Determined to address the problem of the connection between these two time-horizons, Solow came to develop two types of models in the 1960s: a long-run growth model displaying fixed short-run coefficients of capital and a short-run

model macro model displaying a multiplicity of equilibriums with an emphasis on price and wage adjustments. Later, his work in the mid-1980 culminated in a book written with Frank Hahn further reinforcing this basic view. By resorting to imperfect competition and increasing returns, Solow came to commit himself anew to his approach. More than six decades later, however, Solow however recently observed: “I don’t think that that the problem is solved and I hope one can continue to try to solve it.” (Solow 2012: 273)

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