Real Business Cycle Theory and the Great Depression : The Abandonment of the Abstentionist Viewpoint

M. De Vroey and L. Pensieroso

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de l'Université catholique de Louvain

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Real Business Cycle Theory and the Great Depression: The Abandonment of the Abstentionist Viewpoint[§]

Michel De Vroey[*] Luca Pensieroso[**]

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Abstract

Is the Great Depression amenable to real business cycle theory? In the 1970s and 1980s Lucas and Prescott took an abstentionist stance. They admitted that, because of its exceptional character, an explanation of the Great Depression was beyond the grasp of the equilibrium approach to the business cycle. However, while Lucas stuck to this view, we show that Prescott changed his mind at the end of the 1990s, breaking his earlier self-imposed restraint. In this paper we document this evolution of opinion and produce a first assessment of real business cycle models of the Great Depression. We claim that the fact of having constructed an equilibrium model of the Great Depression constitutes a methodological breakthrough. However, as far as substance is concerned, we argue that the contribution of real business cycle literature on the Great Depression is slim, and does not gain the upper hand over the works of economic historians. We conclude suggesting that historical episodes may exist for which the modelisation method of real business cycle theory is inferior to the "thick" sort of analysis that is proper to economic historians.

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[*] Department of Economics and IRES, Université Catholique de Louvain, Place Montesquieu 3, 1348, Louvain-la-Neuve, Belgique. E-mail: devroey@ires.ucl.ac.be.
[**] Department of Economics and IRES, Université Catholique de Louvain, Place Montesquieu 3, 1348, Louvain-la-Neuve, Belgique. E-mail: pensieroso@ires.ucl.ac.be.

1. Introduction

Macroeconomics experienced radical changes in the last quarter of the twentieth century. The most important was the dethroning of the Keynesian IS-LM paradigm and its replacement by a new paradigm centred on the study of growth and the business cycle, rather than unemployment. This started as new classical macro-economics with Lucas's work, to be recast as real business cycle theory by Kydland and Prescott. At present, it also goes under the names of neoclassical growth theory and dynamic stochastic general equilibrium models.

The aim of this paper is to reflect on one of the features of this approach, the way it deals with the Great Depression, probably the single most dramatic business cycle event of the twentieth century. The new approach is an equilibrium theory of the business cycle – equilibrium meaning optimising behaviour and market clearing. Can such an approach come to grips with the Great Depression? Our paper does not aim to address this issue straight on. Rather, we want to focus our attention on the evolution of opinion and research activity that took place within the community of real business cycle theorists with respect to the Great Depression. In the 1970s and 1980s Lucas and Prescott, the two towering figures of the new approach in macroeconomics, took what could be called an abstentionist stance. They admitted that, because of its exceptional character, an explanation of the Great Depression was beyond the grasp of the equilibrium approach to the business cycle. However, while Lucas stuck to this view, Prescott changed his mind at the end of the 1990s, breaking his earlier self-imposed restraint. Real business cycle theory, he ended up by stating, had succeeded in its endeavour to elucidate the Great Depression. The authors credited with this breakthrough were Harold Cole and Lee Ohanian.

We shall start our inquiry by exploring the abstentionist stance. In a second step, we shall document Prescott's change of opinion. Next, we examine Cole and Ohanian's work, which led Prescott to this change. The final section of the paper gives a general assessment of the Prescott–Cole–Ohanion standpoint both in terms of substance (does the real business cycle approach bring out new explanatory factors for the Great Depression?) and method (does it constitute methodological progress?).

2. Lucas on the Great Depression

2.1 The Lucas–Rapping 1969 paper

Lucas and Rapping's (1969a) paper, "Real Wages, Employment, and Inflation", is rightly credited with having initiated the move that led to real business cycle theory. It is also the paper to start with in our attempt to elicit Lucas's position on the Great Depression. [1]

The aim of this paper was to provide the micro-foundations for an analysis of labour supply. As explained in the introduction to Lucas's *Studies in Business Cycle Theory* (1981) and in his professional memoir (2001), Lucas and Rapping's motivation in writing this paper was "not to challenge the current, Keynesian orthodoxy but rather to contribute to it by constructing a 'micro-economic foundation' for the wage-price sector of macroeconomic models" (1981: 2). This attempt was in the same vein as Friedman and Modigliani's approach to households' consumption decisions and Eisner and Jorgenson's work on business firms' investment decisions.

At the time, Lucas and Rapping were barely aware of the full implications of the assumptions they adopted. In particular, they did not view their market clearing assumption as clashing with Keynesian theory – after all, they claimed, Modigliani's (1944) and Lange's (1944) models also included market clearing. Nevertheless, they felt the need to reconcile the existence of unemployment with market clearing.[2] Their solution was to question the usual interpretation of the census data on unemployment, according to which most unemployment was involuntary, and to declare that in fact these people were voluntarily unemployed.

> Measured unemployment (more exactly, its non-frictional component) is then viewed as consisting of persons who regard the wage rates at which they could currently be employed as temporarily

[1] This paper was a joint venture by Lucas and Rapping. However the latter gradually lost interest in this line of research line, so that the authors' rejoinder to Rees (1970), though co-signed by both of them (Lucas and Rapping, 1972), was in fact single-authored by Lucas, as he mentions in his Professional Memoirs (2001). Except when specifically discussing the 1969 paper, we will therefore refer to Lucas's views rather than to Lucas and Rapping's views.

[2] As Rapping said in his interview with Klamer, "We just thought about writing down a demand and supply for labour, which was the easiest way to proceed. Later when we got it worked out, we noticed that we did not have any unemployment in the model. We worried about that of course. We knew that this was always a problem with supply and demand models. One day, one of us said 'Look, let's define unemployment in this way'. Then we noticed that the equation we defined was a Phillips relationship. It was sitting right there" (Klamer 1984: 224).

> low and who therefore choose to wait or search for improved conditions rather than to invest in moving or occupational change. The view that non-frictional unemployment is, in this sense, "voluntary" does not of course imply that high measured unemployment rates are socially costless. (Lucas and Rapping 1969: 748)

No discussion of the Great Depression per se is to be found in Lucas and Rapping's paper. They did not claim to have a market clearing model of the Great Depression. Nonetheless, this was the implicit conclusion of their work, since they tested their model against US time series data covering the years 1930–65 – including the depression years – and claimed that it performed relatively well from an econometric point of view.

2.2 Rees's criticism

In his professional memoir, Lucas candidly evokes the hope that Rapping and he were harbouring when writing their paper, namely that it would make a great stir. To their delight, this is what happened. Yet it also elicited outrage.

> Albert Rees refereed the paper for the Journal of Political Economy: his outraged reaction was later published as a comment. But why outrage? Another economist, we heard, called the paper "fascist economics". Fascists! For writing down a labour supply curve and taking it seriously. (Lucas 2001: 18)

Rees's comment (1970) is of particular interest for our inquiry, because it raised the issue of the relevance of Lucas and Rapping's model for the Great Depression. Rees was a labour economist, formerly at Chicago and at the time at Princeton. While mainly an empirical economist, he had published a theoretical article on involuntary unemployment in 1951 in the *Journal of Political Economy*. He was so convinced of the real-world relevance of the notion of involuntary unemployment that, when he came to face a contradiction between involuntary unemployment and profit maximisation, he claimed that if one of them had to be abandoned, it should be the latter.[3]

Small wonder then that Rees was shocked by the implication of Lucas and Rapping's model that unemployment in the Great Depression was voluntary:

[3] "Some economists may advocate abandonment of the concept of involuntary unemployment to save the idea of short-run profit maximisation. That would seem to me to be the abandonment of observed reality for the sake of a dubious and untested theory" (Rees 1951: 148, note 16).

> In assessing the reasonableness of [Lucas and Rapping's] view, it should be recalled that it is set forth in a paper that fits an econometric model to United States data for the period 1930–65, which includes all of the Great Depression of the thirties. Some measured unemployment may be essentially voluntary in the case of pockets of unemployment during conditions of general prosperity.… However, to extend this view to conditions of general deficiency in demand involves an aggregation fallacy. When all markets are depressed in varying degrees, a few of the unemployed might still be able to find work by shifting their location or trade, but it is surely not true that all of them could do so at once. This proposition seems self-evident in the case where wages are rigid downwards. Most Keynesians would argue that it also holds where wages are flexible, since if all workers cut their reservation prices the resulting short-run expectations of falling wages and prices would further depress aggregate demand. Though scientific discussion is supposed to be dispassionate, it is hard for one old enough to remember the Great Depression not to regard as monstrous the implication that the unemployment of that period could have been eliminated if only all the unemployed had been more willing to sell apples or to shine shoes (Rees 1970: 308)

Beyond doubt, Rees had what seemed at the time a strong point. Lucas and Rapping's model – and subsequent RBC models all followed suit – assumed market clearing. That is, it assumed away involuntary unemployment. If it is believed that the voluntary/involuntary unemployment distinction makes any sense, the Great Depression is the period *par excellence* for which the notion of involuntary unemployment is relevant. Arguing that the massive unemployment that existed at the time was composed of voluntarily unemployed persons stretched credibility. Hence Rees's claim that any model excluding market non-clearing as a matter of premise was ill-suited for studying the Great Depression.

2.3 Lucas's reaction

Lucas's reaction to Rees's criticism was twofold. On the one hand he refused to enter into a discussion on the voluntarity/involuntarity of unemployment.[4] However he accepted the standard Keynesian interpretation that the Great

[4] Lucas's definitive position on the subject of involuntary unemployment, namely that this notion should be dispensed with altogether, is set out in Lucas ([1978] 1981: 242). For a discussion of this point, see De Vroey (2004).

Depression was due to a deficiency in aggregate demand.[5] On the other hand, he agreed to delve deeper into the issue of the relevance of their model for the Great Depression. Here, stepping back from their previous conclusion, he admitted that the model failed to explain the data from 1934 to World War II.

> Rees also raises the important empirical question of whether our theory does succeed in accounting for labour–market behaviour during the period 1929-39. Further study on our part indicates that Rees's scepticism on this point is well founded: our hypothesis accounts for much, but *not* all, of the observed labour–market rigidity during this period (Lucas and Rapping 1972: 186).
>
> The expectations model we used implies that in 1934 the unemployment rate should have been at its 1929 or 1930 level, as opposed to the observed 22 percent level (Lucas and Rapping 1972: 190).

Lucas was enigmatic about the reasons behind the failure of their model, contenting himself with noting that it considered only one source of rigidity (adaptive expectations), while others might have been at work as well. In a footnote, he hinted at Alchian's (1970) conjecture that recovery was delayed by the succession of New Deal price- and wage-fixing measures, a claim that Cole and Ohanian were to take up again some thirty years later.

2.4. Lucas' subsequent standpoint

Lucas returned to the issue of the Great Depression on several occasions, mainly in interviews or book reviews, although never in any detail. In these pieces he repeatedly expressed the view that real business cycles models, which he otherwise fully endorsed, were unable to explain the Great Depression. This job, he constantly claimed, had been done by Friedman and Schwartz:

> Viewed as positive theory, real business cycle models do not offer a serious alternative to Friedman and Schwartz's monetary account of the early 1930s. …There is no real business cycle model that can map these shocks into anything like the 40% decline in real output and employment that occurred between 1929 and 1933 (nor, indeed, does anyone claim that there is). Even if there were, imagine trying to rewrite the Great Contraction chapter of *A Monetary History* with

[5] "The only aggregative economic policy implication we see for events like the Great Depression are the standard ones: if possible, avoid the aggregate-demand shifts which cause them; failing this, pursue corrective demand policies to make them as brief as possible." (Lucas and Rapping 1972: 187).

> shocks of this kind playing the role Friedman and Schwartz assign to monetary contractions. What technological or psychological events could have induced such behaviour in a large diversified economy? How could such events have gone unremarked at the time, and remain invisible even to hindsight? (Lucas, 1994: 13).[6]

In short, Lucas's stance is that the real business cycle method is fine for periods of plain sailing but ill suited to more dramatic events such as the Great Depression:

> In Kydland and Prescott's original model, and in many (though not all) of its descendants, the equilibrium allocation coincides with the optimal allocation: fluctuations generated by the model represent an efficient response to unavoidable shocks to productivity. One may thus think of the model not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not. Viewed in this way, the theory's relative success in accounting for post-war experience can be interpreted as evidence that post-war monetary policy has resulted in near-efficient behaviour, not as evidence that money does not matter (Lucas, 1994: 13).

3. Prescott on the Great Depression

3.1 Prescott's early view

To the best of our knowledge, Prescott's first remark on the Great Depression dates from 1983 and is to be found in a Federal Reserve Bank of Minneapolis Working Paper discussing the methodology of the then nascent real business cycle theory. In this paper, Prescott examines four objections that can be raised to this theory. One of these is directly related to our inquiry: "How can a theory claim to explain the business cycle if it cannot explain the Great Depression?" (Prescott 1983: 11).

Prescott's answer is straightforward. He plainly admits that the Great Depression is beyond the reach of the equilibrium model of the business cycle. To him, this restraint is virtuous because it shows that the practioners of the new approach are aware of its limits. Equilibrium models of the business cycle, he admits, work only for empirical cases where the political and financial context is stable.

[6] Other references are Lucas ([1980] 1981: 273, 284), Lucas (1987: 87), Klamer (1984: 41–2), Snowndon and Vane (1998: 125) and McCallum (1999: 284).

> The answer to question (b) is simply that competitive equilibrium theory is not suited to modelling economic fluctuations in periods of great political and financial institution instability. The inability of either the equilibrium monetary or the technology shock theories to explain the Great American Depression is evidence of the discipline of the methodology. If any observation can be rationalised with some approach, then that approach is not scientific (Prescott 1983: 12).

Prescott stood by this point of view for several years.[7] This is witnessed by his reaction to Summers' rejoinder to his "Theory ahead of measurement" paper (Prescott 1986). One of Summers' points was that no equilibrium theory could deal with events like the Great Depression, because of the pervasive disruptions to the exchange system that characterise such periods (Summers, 1986: 26).[8] In contrast to the generally flamboyant tone of his response, Prescott remained subdued on this point, taking the same line as in his 1983 paper:

> Summers has perhaps misread some of my review of real business cycle research. There I do not argue that the Great American Depression was the equilibrium response to technology shocks as predicted by the neoclassical growth model. I do not argue that disruptions in the payment and credit system would not disrupt the economy (Prescott 1986: 29).

3.2 Prescott's present standpoint

Prescott did not return to the matter of the Great Depression until his 1996 interview with Rolnick for *The Region*, a journal of the Federal Reserve Bank of Minnesota (Rolnick 1996). Here, he departs from his earlier abstentionist view. While still referring to the political turmoil of the time, he points out that, after all,

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7 Prescott was not alone among the founders of the new approach, including Lucas, to hold such an abstentionist view of the Great Depression. For example, interviewed by Klamer, Sargent stated, "I do not have a theory, nor do I know somebody's else's theory, that constitutes a satisfactory explanation of the Great Depression" (Klamer 1984: 69).

8 "Between 1929 and 1933, the gross national product in the United States declined 50%, as employment fell sharply. … I submit that it defies credulity to account for movements on this scale by pointing to intertemporal substitution and productivity shocks… It seems clear that a central aspect of depressions, and probably economic fluctuations more generally, is a breakdown of the exchange mechanism. Read any account of life during the Great Depression in the United States. Firms had output they wanted to sell. Workers wanted to exchange their labour for it. But the exchanges did not take place. To say the situation was constrained Pareto optimal given the technological decline that took place between 1929 and 1933 is simply absurd, even though total factor productivity did fall. What happened was a failure of the exchange mechanism" (Summers 1986: 26).

the Great Depression is amenable to the real business cycle approach. Moreover, he now distances himself from Friedman and Schwartz's (1963) interpretation.

> With regard to why there was a Great Depression in the United States in the thirties, I just haven't seen the evidence that points the finger to the monetary side. Canada had the persistent and big decline in output – every bit as big as the United States – but not the bank failures. … My own speculation would have something to do with the unstable political times. I have looked at the time series of factor income shares. Labour's share of total income or, equivalently, of total output, was more or less constant up to the beginning of the Great Depression, at which time it jumped about 5 percent. Subsequently it has remained at that higher level. This strongly suggests there was a big change in the rules of the economic game that occurred just after the beginning of the Great Depression. (Rolnick 1996: 6).

It did no take long for Prescott's to fully turn away from his earlier position. His new views are to be found in two papers, which are commentaries on other authors' work rather than original research. The first is a short piece published in the *Federal Reserve Bank of Minneapolis Quarterly Review* in 1999 and entitled "Some observations of the Great Depression" (Prescott 1999), which comments on Cole and Ohanian's article "The Great Depression in the United States from a Neoclassical Perspective" (Cole and Ohanian 1999). The second article, written jointly with Timothy Kehoe, is an introduction to the 2002 special issue of *The Review of Economic Dynamic*s resulting from a conference held at the Federal Reserve of Minneapolis in October 2000 (Kehoe and Prescott 2002).

Both articles start with an expression of surprise:

> Why hasn't growth theory been used to study the Great Depression? Perhaps because economists are reluctant to use standard theory to study an event that historically was treated as an aberration defying an equilibrium explanation (Prescott 1999: 25).
>
> The general equilibrium growth model is the workhorse of modern economics. … Until recently, however, it has been taboo to use the growth model to study great depressions. This volume breaks this taboo. (Kehoe and Prescott 2002: 2).[9]

[9] The idea that a taboo needed to be broken is also evoked in Prescott's Richard Ely Lecture, "Prosperity and Depression" (Prescott 2002: 1).

Prescott's earlier argument that real business cycle theory should refrain from trying to explain the Great Depression is now rejected. According to Kehoe and him, the conventional view that "great depressions are unique events that occurred in the interwar period and are of historical interest only" ought to be dismissed. Instead, they claim that several other "great depressions" occurred in the 20th century, most of them close to the present time.

This trivialisation of the Great Depression is made possible by a definitional change. The standard definition is that the "Great Depression" term designates the period between the end of 1929 and 1933. In contrast, Kehoe and Prescott propose a quantitative definition resting on two conditions:

> To be a great depression, a negative deviation from trend must satisfy two conditions. First, it must be a sufficiently large deviation. Our working definition is that a great depression is a deviation at least 20% below trend. Second, the deviation must occur rapidly. Our working definition is that de-trended output per working-age person must fall at least 15% within the first decade of the depression (Kehoe and Prescott 2002: 9).[10]

If this definition is adopted, other twentieth century great depressions emerge:

> Argentina, Brazil, Chile and Mexico had depressions in the 1980s that were comparable in magnitude to those in Canada, France, Germany and the United States in the interwar period. … In recent times, New Zealand and Switzerland – rich, democratic countries with market economies – have experienced great depressions. If the current Japanese depression continues a few more years, it will be come a great one. (Kehoe and Prescott 2002: 2).

The suggested definitional change implies that the Great Depression of the 1930s covers the entire 1929–39 decade, since output remained below trend for the whole of that period. As will be seen below, this definition leads to two distinct puzzles: the onset of the decline; and the weak recovery from it (i.e. the 1929–33 and the 1934–39 periods). The merit of Cole and Ohanian's work, according to Prescott, is to have shifted the attention from the former to the latter issue, now considered the more important (1999: 26). Prescott also adheres to their

[10] Kehoe and Ruhl state that a third condition should be posited, that "there is no significant recovery during the period in the sense that there is no sub-period of a decade or longer in which the growth of output per working-age person returns to the trend" (Kehoe and Ruhl 2005: 762).

conclusion as to the cause of the weak recovery. As forcefully stated in the last paragraph of his 1999 article,

> In the 1930s, there was an important change in the rules of the economic game. This change lowered the steady-state market hours. The Keynesians had it all wrong. In the Great Depression, employment was not low because investment was low. Employment and investment were low because labor market institutions and industrial policies changed in a way that lowered normal employment (1999: 27).

4. Explaining Prescott's change of mind

As documented above, a 180 degree turn occurred between Prescott's early remarks pointing to the inability of the real business cycle approach to explain the Great Depression and his later (and present) standpoint. As to the reasons for this change, only conjectures can be made.

A first possible reason is that Prescott had not given the matter enough thought in his first comments. As a result, he adopted the common-sense viewpoint without realising that it was wrong, that a taboo was blocking a clear vision of things. In other words, the "young" Prescott was unaware of the full potential of the new methodology he had contributed towards establishing.

A second explanation relates to the process of development of new research agendas. The themes that are addressed first are presumably those which the new approach fits best. More controversial, less intuitive lines of research will be explored only when the new approach is well established. Therefore addressing the issue of the Great Depression, after having earlier neglected it, is nothing more than "science in progress".

The need to react to an outside challenge is another possible explanation. For an approach that claims to provide a general theory of business cycles, the admission that one of the most important depressions in history – and certainly the best publicised one – lies beyond its grasp appears to be a sign of weakness. Assertions such as Bernanke's (1995: 1) – "To understand the Great Depression is the Holy Grail of macroeconomics"– must have acted as a red rag to a bull to real business cycle theorists.

Finally, we must also evoke the underlying political dimension. Prescott is a stern and avowed defender of *laissez-faire* economics. So, if it turns out that applying the real business cycle method to the Great Depression allows a blatant case of "destructive policies" (to borrow an expression from Ohanian (2000: 26), i.e. state interference with the competitive process) to be brought to the forefront, such an opportunity should not be lost.

But the central question is whether Prescott was right in changing his mind rather than staying on Lucas's side. To answer this question, we need to turn to the research that, according to Prescott, has clinched the matter. Since Cole and Ohanian have played a pioneering role in this respect, we will limit our discussion to their work.[11] This is the task we undertake in the next two sections of this paper.

5. Cole and Ohanian on the US Great Depression

Cole and Ohanian were the first authors to look at the Great Depression through the lens of neoclassical growth theory.[12]

> We use neoclassical growth theory to study macroeconomic performance during the 1930s the way other economists have used the theory to study post-war business cycles. We first identify a set of shocks considered important in post-war economic declines: technology shocks, trade shocks, and monetary shocks. We then ask whether those shocks, within the neoclassical framework, can account for the decline and the recovery in the 1930s. This method allows us to understand which data from the 1930s are consistent with neoclassical theory and, especially, which observations are puzzling from the neoclassical perspective (1999: 2).

Cole and Ohanian's central message can be grasped by looking at Figure 1, in which we have graphed de-trended data from their 1999 paper for US output, TFP, total employment and real wages in manufacturing and non-manufacturing sectors.[13]

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[11] See Pensieroso (2005) for a more detailed survey of the real business cycle literature on the Great Depression.

[12] Their main papers on the subject are Cole and Ohanian (1999, 2000, 2002, 2004).

[13] TFP is defined as "any exogenous factor that changes the efficiency with which business enterprises transform inputs into outputs" (Cole and Ohanian, 1999: 5).

[Insert Figure 1]

One of Cole and Ohanian's original claims was that the Great Depression must be viewed as a ten-year episode, because de-trended output (the blue line in Figure 1) remained below the trend line for all the decade. Figure 1 also illustrates the strong pro-cyclical behaviour of TFP (the pink line). After dropping by almost 18% from 1929 to 1933, it returned to its trend level in 1936. Considering a standard real business cycle model, Cole and Ohanian fed in measured TFP as the impulse mechanism of the business cycle. Their growth accounting exercise led to a twofold result. First, the measured TFP behaviour accounts for about 40% of the initial decline in de-trended output. Second, it has almost no explanatory power for the post-1933 years.

Using the same growth accounting technique, Cole and Ohanian examined whether the behaviour of output could be traced back to the other variables invoked in competing explanations of the Great Depression – fiscal policy, international trade restrictions, monetary tightening, financial intermediation disruptions, changes in reserve requirements, and nominal wages rigidity. The result was negative: none of them, they claim, stands up to scrutiny.

To Cole and Ohanian, the most intriguing result is that output failed to return to its trend level in 1936, despite the fact that TFP was back on trend by that date. Hence their contention that the main puzzle raised by the Great Depression lies in understanding why the US economy remained depressed until the outbreak of WW II rather than in elucidating its onset. The lack of a marked recovery in output and employment (the yellow line in Figure 1), co-existing with a normal growth in productivity, suggests that another negative shock must have hit the economy in the mid-1930s. New Deal labour market legislation is Cole and Ohanian's main suspect.

The aim of Cole and Ohanian's 2002 and 2004 articles is to clinch the matter analytically. Cole and Ohanian (2002) claim that that a distortion occurred in the labour market, driving a wedge between the real wage and the marginal rate of substitution between consumption and leisure, the former exceeding the latter. Positing that agents were in individual equilibrium in 1929, Cole and Ohanian investigate whether this was still the case in 1939. Their answer is "no".

> Three of the four [first order] conditions are distorted. The marginal rate of substitution between consumption and leisure is 41 percent below the wage rate, and factor prices differ considerably from their implied marginal products. The wage rate substantially exceeds the marginal product of labor, and the return to capital is below the marginal product of capital. (Cole and Ohanian 2002: 30).

Between them, these data suggest that some factor raised the wage above its market-clearing level, and that this high wage prevented households from satisfying their marginal rate of substitution condition (2002: 30).

In their 2004 paper, Cole and Ohanian focus their attention on one specific New Deal policy measure, the National Industrial Recovery Act (NIRA). This allowed firms to set prices cooperatively within the same industry, conditional on their accepting collective bargaining with unions over wages. Moreover NIRA codes of "fair competition" prescribed a minimum wage per industry that was typically above the previous prevailing wage rate. Cole and Ohanian provide evidence that this enhanced high wages in the US economy. As can be seen from Figure 1, the data confirm that wages in manufacturing, which was 80% covered by NIRA, were above the trend line for the whole decade, with a marked increase in 1933, the year in which the NIRA was signed. On the contrary, real wages in non-manufacturing sectors, which were not covered by the NIRA, remained below the trend line throughout the decade (as also evident in Figure 1). Cole and Ohanian's 2004 article also provides a quantitative analysis of the role of New Deal policies in causing the weak recovery. To this end they built a multi-sectoral model with imperfect competition and wage bargaining between firms and insider workers. The cartel sector works on an insider/outsider pattern. Insiders set the wage and the employment level, conditional on firms' reservation profits. Whenever firms agree with workers, they are allowed to collude over production. This last assumption, Cole and Ohanian claim, captures the central feature of the NIRA (2004: 781). Insiders are thereby able to raise the cartel wage above the market-clearing level.[14]

The main quantitative result of their model is that

[14] Cole and Ohanian claim that monopoly per se is not responsible for the low level of employment. In effect, whenever monopoly is present without labour bargaining power, the cartel wage turns out to be close to the competitive wage and the reduction in output is small. Thus, the combined presence of monopoly and labour bargaining power is required in order to obtain the weak recovery result.

> New Deal cartelization policies are a key factor behind the weak recovery, accounting for about 60 percent of the difference between actual output and trend output (Cole and Ohanian 2004: 781).

On a broader level, they conclude their 2004 article with the following statement:

> New Deal labor and industrial policies did not lift the economy out of the depression as President Roosevelt had hoped. Instead, the joint policies of increasing labor's bargaining power and linking collusion with paying high wages prevented a normal recovery by creating rents and an inefficient insider–outsider friction that raised wages significantly and restricted employment. (Cole and Ohanian 2004: 813).

6. An assessment

6.1 The originality of Cole and Ohanian's analysis

Our first task is to ponder the originality of Cole and Ohanian's work. Here, we must examine, first, whether their analysis enriches our understanding of the Great Depression as concerns both its onset and its protracted character, and, second, whether they have inaugurated a new way of approaching it.

As far as the unfolding of events leading to the Great Depression is concerned, Cole and Ohanian's positive contribution is, as they themselves admit, almost nil.[15] The TFP story is quantitatively weak, because it accounts for only 40% of the drop in output, and qualitatively unsatisfactory, as it traces everything back to an undetermined exogenous shock. Thus, as far as the onset of the Great Depression is concerned, Cole and Ohanian's analysis can hardly compete with the existing historical explanations (Kindelberger,1973; Friedman and Schwartz, 1963; Eichengreen, 1992; Romer, 1990; 1992; 1993; De Long, 1997, to name but a few).

However, Cole and Ohanian's main investigation bears on the weak recovery from the Great Depression. Two remarks are in order here. First, their weak

[15] See Ohanian (2002). In the introduction to this paper, Ohanian notes, "The Depression remains one of the most important and enduring mysteries in macroeconomics, and identifying the causes of this productivity decrease may shed new light on this mystery" (2002: 12). Yet at the end of the article, he writes "I conclude that the Great Depression productivity puzzle remains largely unsolved" (2002: 14).

recovery characterisation brings out only one side of the picture, as aptly stated by Romer:

> The recovery of the United States from the Great Depression has been alternatively described as very fast and very slow. It was very rapid in the sense that the growth rate of real output was very large in the years between 1933 and 1937 and after 1938.…The recovery was nevertheless slow in the sense that the fall in output in the United States was so severe that, despite, the impressive growth rates, real GNP did not return to its pre-Depression level until 1937 and its pre-Depression growth rate path until around 1942 (Romer 1993: 34-5).

One aspect of this strong recovery, emphasised by Temin and Wigmore (1990), is the significant increase in investment that occurred from 1933 onwards and which was a distinctive feature of the US. Figure two illustrates the extent to which this recovery in investment occurred only in the US.

[Insert Figure 2]

In their view, the New Deal and the devaluation of the dollar acted as signals of a regime shift. More optimistic expectations and hence increased investment ensued.

Our second remark is that Cole and Ohanian are hardly the only authors to have criticised the NIRA policy. Attacking the latter has been a staple of defenders of *laissez-faire* from Simons (1934), the Chicago economist, to present-day authors (Powell (2003), Smiley (2002), Hall and Ferguson (1998)). Actually, most economists agree that this particular policy, as distinct from other New Deal policies, was inadequate.[16] To limit ourselves to one account, Eichengreen, a leading Great Depression specialist, wrote that "The National Industry Recovery Act … contributed, perversely, to the slow recovery of American output and employment" (Eichengreen 1992: 344).

So, our conclusion about the onset of the Great Depression must be extended to the topic of the protracted character of the crisis: Cole and Ohanian's novelty in terms of substance is slim.

[16] A classical piece assessing the NIRA policy is Hawley (1966). Even Keynes was dismissive of it (1933).

The fact is that their real contribution is methodological. While earlier authors made their claim in a qualitative, narrative way, Cole and Ohanian were able to construct a general equilibrium model of the Great Depression.[17] They must be hailed for having succeeding in this daunting task.

This being granted, it must be realised that while presenting undeniable advantages over the narrative approach, the modelling approach has also drawbacks of its own. Ohanian is right in pointing out that many stories are available. The problem is that not all of them –possibly including the most appealing ones – can be translated into models. Models are based on exclusions. But what if the excluded factors are crucial contenders in the explanatory trial? Let us mention two obvious factors for the onset of the Great Depression that are absent from Cole and Ohanian's model. The first, which has already been mentioned above, is the idea that the Great Depression witnessed a failure in the exchange mechanism. The second is the role played by the gold standard mechanism in the Great Depression, the factor favoured by Eichengreen (1992) and Eichengreen and Temin (2000). As aptly argued by these authors, this institutional mechanism had ceased to function well in the wake of WWI, and governments and central banks did not know what actions to take to fix it. According to Eichengreen and Temin, any analysis omitting this dimension is doomed to fail to provide a satisfactory explanation of the Great Depression.

To conclude, Cole and Ohanian's main contribution is to have inaugurated a new way of tackling the issue of the Great Depression. Lucas, Kydland and Prescott's theoretical breakthrough consisted of dismissing the view that business cycles were not amenable to equilibrium analysis. Cole and Ohanian's contribution is of the same order. Previously it was believed that no abstract quantitative model of the Great Depression could be constructed. They must be credited with having disproved this view.

[17] The following quotation drawn from Ohanian's interview in the *Economic Dynamic Newsletter* illustrates this point: "General Equilibrium theory is important for understanding the Depression. There are a lot of stories about the Depression, but without an explicit general equilibrium model you don't know if the stories hold water. One of the benefits of general equilibrium theory is that it forces you to look beyond the direct effects of shocks, and assess the indirect effects. Hal [Cole] and I are writing a paper for the NBER Macro Annual that uses general equilibrium models to study the two most popular shocks for 1929–33: the money stock decline and bank failures. Using general equilibrium models, we found that many of the indirect effects of these shocks offset the direct effects, or were at variance with the data." (Ohanian 2000: 6).

6.2 The singularity of the Great Depression

The standard view of a depression is that it constitutes a particular phase in the business cycle, characterised by the fact that output remains (significantly) below the trend line. In turn, a business cycle is defined as composed of four elements, the depression or decline, the trough, the recovery and the peak. In short, a business cycle is a set of peak-to-trough movements. To Cole and Ohanian, the Great Depression is yet another depression, its uniqueness lying solely in its amplitude.

As claimed by Lucas ([1977] 1981: 218), the fact that all business cycles manifest the same sequence of movements and time-lags is the very feature that allows a general theory of the business cycle, abstracting from the particularities of individual cycles, to be constructed. This is why little attention is given in this literature to the causes of any given depression.[18]

It remains true, nonetheless, that all business cycles – and their components – are a mix of singularity and recurrence. Real business cycle theory just assumes that the singularity dimension can be overlooked for the sake of theoretical analysis. But then the appropriateness of applying the real-business-cycle toolbox hinges on the assumption that the recurrence is more important than the singularities. If the reverse were true, the appropriateness of the business cycle framework for tackling the Great Depression would have to be questioned.

The alternative vision is that the Great Depression was not a depression in the standard sense (i.e. in the sense that slumps are necessarily followed by recoveries after some "liquidation" has come to an end).[19] At work was a system-failure phenomenon, analogous to that which occurred with the downfall of former communist regimes. In this view, the 1929–33 events brought the economy to a

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[18] Moreover, according to Prescott, identifying causes is quasi-impossible because what is called a "shock" may consist of the aggregate of a series of small, barely identifiable, events. As he stated in an interview wit *The Region*, the publication of the Federal Reserve Bank of Minneapolis, "We don't have a theory of what causes economy-wide productivity to change. We can measure how big the changes are, and we can use dynamic theory to predict the consequences of these random changes. Now the question is: Can we identify specific shocks? My answer is no. We can't even identify why total productivity of labor and capital is four or five times higher here than in India. Given this, how can we hope to identify why this productivity grew by 2 percent less than expected over some two-year period? Such an occurrence is all that is needed to induce a recession" (Prescott 1996: 8).

[19] See De Long (1990).

state of affairs where any speedy recovery through private-sector adjustments was excluded. To vanquish the threat of a system collapse, a strong signal announcing a change in regime was needed. The New Deal constituted such a signal. This anti-liquidationist vision is well captured in the following observation by Temin and Wigmore (1990):

> Our argument is that Franklin Delano Roosevelt established a new macro-economic policy regime shortly after his inauguration in March 1933. The Hoover administration had been financially conservative, adhering to the rules of the gold standard and fiscal orthodoxy. Its policy stance in the troubles of the early 1930s, therefore, was decidedly deflationary. Roosevelt broke with this ideology, devaluating the dollar within 6 weeks of his inauguration, promoting fiscal expansion, and championing the virtues of inflation – or reflation as he termed it. The devaluation of the dollar was the single biggest signal that the deflationary policies implied by adherence to the gold standard had been abandoned, that the iron grip of the gold standard had been broken. Devaluation had effects on prices and production throughout the economy, especially on farm and commodity prices, not simply on exports and imports. It sent a general message to all industries because it marked a change in direction for government policies and for prices in general. The elements of the New Deal emerged in the course of 1933; the devaluation of April–July 1933 was the proximate cause of the recovery (Temin and Wigmore 1990: 485).

According to whether the emphasis is put on the recurrence or the singularities, the approach to the study of the Great Depression will be different. If the recurrence option is chosen, it will be considered amenable to modelling. In contrast, emphasising the singularities leads to the view that the Great Depression belongs more to the realm of economic history rather than to that of real business cycle theory.

In the light of these remarks, let us return to the issue of the rationale for Lucas's (and the young Prescott's) abstentionist stance. The statement that the Great Depression is beyond the grasp of real business cycle theory can be understood in two senses. Either that constructing a model of the Great Depression in the Lucasian sense of the term – "a parallel or analogue system – a mechanical, imitation economy" ([1980] 1981: 272) is an impossible task. Or that, even if the task is feasible, such models would be less useful for understanding the Great

Depression than an economic history perspective. Before Cole and Ohanian's work, these two standpoints were blurred. Afterwards, the first issue was decided – positively. Yet, the second question – is a modelised approach to the Great Depression superior to an historical approach? – remains undecided.

Our conjecture is that the abstentionist view of both Lucas and of the young Prescott was based on their intuition that the Great Depression was too exceptional a chain of events to be grasped by economic theory (understood as an abstract quantitative model). It should therefore be handled by economic historians constructing the sort of narrative provided, for example, by Friedman and Schwartz (1963). Such narratives may well be nurtured by economic theory and give rise to econometric verification, but they are not models in the Lucasian sense of the term. So the fact that a model of the Great Depression has been constructed is not a sufficient reason to abandon the abstentionist viewpoint.

6.3 One Great Depression or several great depressions?

Our next remark bears on Kehoe and Prescott's claim that several "great depressions" occurred in the last century. This claim follows from their definition of a great depression, according to which a 20% cumulative deviation of output below trend, with a 15% fall in the 10 first years, constitutes a great depression.

In our eyes, such a definition is hardly neutral, and has little to commend it. Ten years is too long a period. In the 1930s it took only one year for de-trended output to fall by 15% (and the overall fall between 1929 and 1933 was almost 40%). Kehoe and Prescott's definition is too general. This is particularly clear when it is observed that their criterion leads them to assert that countries like New Zealand and Switzerland are presently enduring great depressions. It is obvious that these countries' present situations do not bear comparison with the events of the 1930s.[20] With such a definition the specificity of the Great Depression is lost.

Moreover, Kehoe and Prescott's stress on the weak recovery aspect in their definition of a great depression may lead to a confusion between the explanation of the causes of the Great Depression and the causes of its protracted character. In our view, Prescott falls prey to such a mistake when writing:

[20] On this, see Abrahamsen et al. (2005).

> In the 1930s labor market institutions and industrial policy actions changed normal market hours. I think these institutions and actions are what caused the Great Depression (Prescott 1999: 26).[21]

If the changes that Prescott has in mind concern the effects of the New Deal, they can in no way be viewed as having caused the Great Depression. Sticking to his definition of a great depression allows him to focus his attention on the causes of the long duration of the depression, thereby putting aside the task of explaining the plunge in output in the early 1930s.

6.4 The return of involuntary unemployment

One of Rees's complaint s about the Lucas-Rapping model bore on its inability to integrate involuntary unemployment, which he believed was an important feature of the Great Depression.[22] Ironically enough, it may be observed that involuntary unemployment makes an unwitting return in Cole and Ohanian's 2004 model.

The latter marks a triple breach in the canonical real business cycle model, giving it a definite New Neoclassical Synthesis flavour. First, Cole and Ohanian shifted from a perfect to an imperfect competition framework; second, employment and wages were determined through an insider–outsider procedure; and third, they departed from the equilibrium discipline by introducing an involuntary unemployment result into their model.

The transition to an imperfect competition framework is hardly benign. The choice of the perfect competition framework in the first generation of real business cycle models must have been based on matters of principle rather than resulting from observation of the real economy. Why, then, move towards imperfect competition? Were the changes associated with New Deal policies so drastic as to warrant such a modification?

The central claim of Cole and Ohanian's 2004 article is that "some factor prevented labor market clearing during the Great Depression" (2004: 782). A distortion occurred in the labour market, with the real wage rate exceeding the

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21 "From the perspective of growth theory, the Great Depression is a great decline in steady-state market hours. I think this great decline was the unintended consequence of labor market institutions and industrial policies designed to improve the performance of the economy" (Prescott 1999: 27).

22 It is true that this model paved the way for the gradual exclusion of the notion of involuntary unemployment from the set of theoretically correct concepts in macroeconomics. See De Vroey (2004).

marginal rate of substitution between consumption and leisure. Although they do not use the term, this is nothing else than involuntary unemployment according to its standard definition, a state of affairs where agents would like to participate in the labour market yet, for some reasons, are unable to carry out this plan.[23] Here the reason is insider's ability to capture a rent.

Does this full circle move, amounting to the return to grace of the involuntary unemployment concept after its exclusion by Lucas, mean that Cole and Ohanian are Keynesians? Of course not. It is just that involuntary unemployment has changed sides. In the Keynesian tradition, it served the purpose of highlighting a market failure. Now, on the contrary, it epitomises a state failure and is pushed into the service of the *laissez-faire* cause.

7. Concluding remarks

Cole and Ohanian must be credited with having started to use the tools of modern economic theory to investigate the Great Depression The task was worth a try, and the fact of having been able to construct a model is no mean feat. However, as far as substance is concerned, our judgement is more reserved. At this juncture, we are unable to decide whether their weak recovery claim is stronger than the opposing claims. Our main bone of contention with their work concerns the claim that the weak recovery issue is more important than the issue of the onset of the Great Depression. On what grounds can such a claim be made? As far as the onset of the Great Depression is concerned, Cole and Ohanian's model does not gain the upper hand over the complex and subtle explanations to be found in the writings of the many economic historians who have studied this event.

Our analysis lead us to conclude that what is at stake in the assessment of real business cycle models of the Great Depression is a territorial dispute between new classical economic theory and economic history, hinging on whether there are limits to the modelisation strategy in economics. If Prescott's standpoint amounts to claiming that the work of economic historians is pre-scientific and should be

[23] It is true that in Cole and Ohanian's model agents who are rationed in the cartel sector end up in another activity (search, domestic labor or the competitive labour market), which they choose optimally. However such a result has also been obtained in dual market models (see, for example, Akerlof and Yellen (1986: 3) or Hahn (1983: 225)).

replaced by abstract models, we definitely disagree with him. What about Lucas? He is likely to applaud Cole and Ohanian's methodological breakthrough and endorse their views on the negative role of the NIRA policy. Still we hope that he will stick to his abstentionist view and admit that some historical episodes exist for which the modelisation method is inferior to the 'thick' sort of analysis that is proper to economic historians.

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Figures

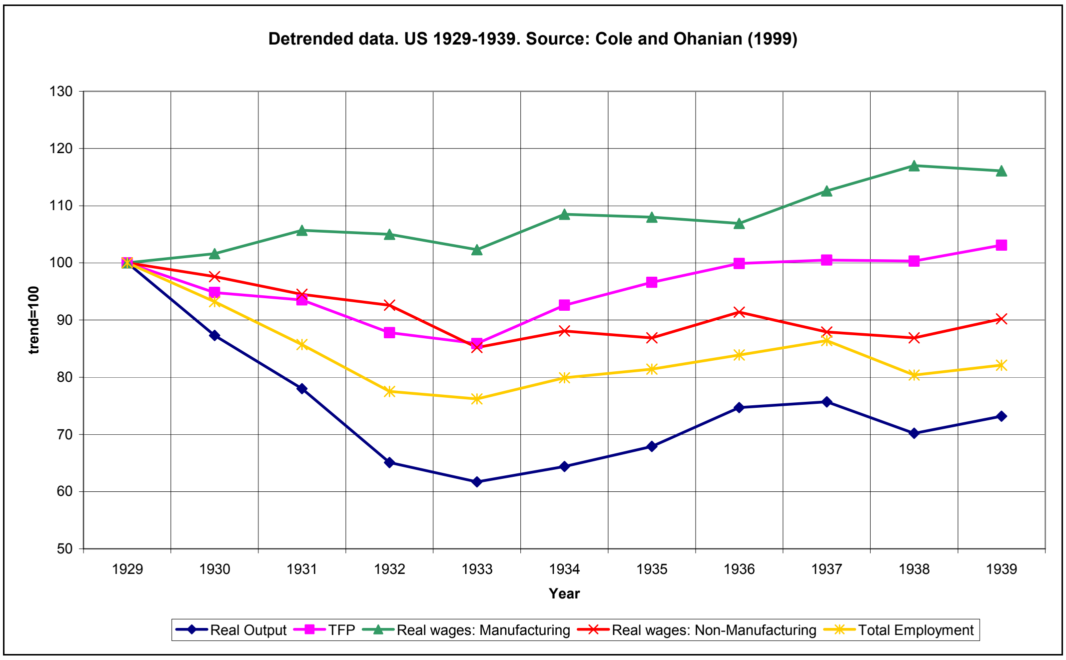

Figure 1

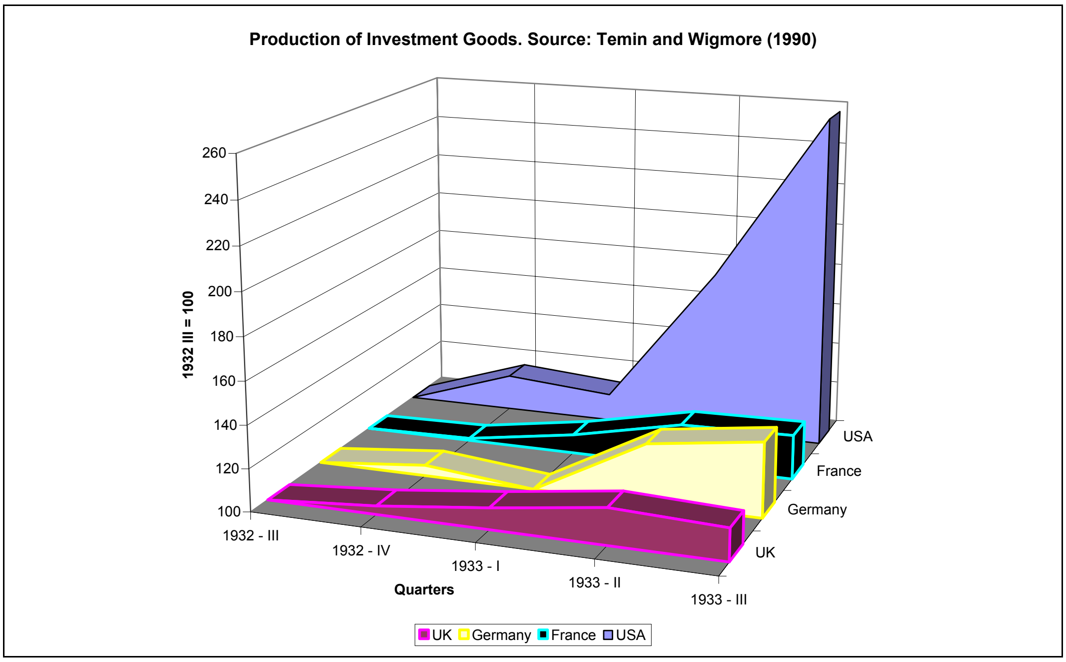

Figure 2

Département des Sciences Économiques
de l'Université catholique de Louvain
Institut de Recherches Économiques et Sociales

Place Montesquieu, 3
1348 Louvain-la-Neuve, Belgique

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