

# FRIEDMAN AND LUCAS ON THE PHILLIPS CURVE:

## FROM A DISEQUILIBRIUM TO AN EQUILIBRIUM APPROACH

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### INTRODUCTION

Friedman and Lucas have probably been the most influential economists of the second half of the twentieth century: between them they were able to throw the Keynesian paradigm off its pedestal. According to the standard view, their dismissal of Keynesianism was a two-step process; its first stage is associated with monetarism and the second with new classical macroeconomics. While such a statement is historically right, it incorrectly suggests a line of continuity between monetarism and new classicism. The aim of this paper is to criticize this interpretation and argue that important methodological differences separate Friedman and Lucas.

To this end, I will compare Friedman's [1968] "The Role of Monetary Policy" and Lucas' [1972] "Expectations and the Neutrality of Money", these two authors' most influential papers. Friedman's article launched the charge against Keynesian economists' will to seize upon the Phillips Curve as a tool for reducing unemployment, epitomized in Samuelson and Solow [1960]. Lucas' paper, while aiming at giving stronger micro-foundations to Friedman's claim of policy-ineffectiveness, paved the way to modern dynamic macroeconomics. The contribution usually attributed to Lucas is that he replaced adaptive with rational expectations. Although this is of course true, there is more to it. In particular, I will show that this replacement is underpinned by a change from a Marshallian to a Walrasian approach to equilibrium. Among other things, I will emphasize that Friedman's model is a *disequilibrium* model (a standard outcome of the Marshallian conception of equilibrium) whereas disequilibrium has no place in Lucas' model.<sup>1</sup>

Forerunners of the present paper are Laidler [1981; 1982], Hoover [(1984) 1990; 1988]; Howitt [(1986) 1990] and Hartley [1997]. For example, Hoover makes the following claims about the relationship between Friedman and Lucas:

Friedman argues that what separates monetarists from Keynesians are differences of empirical judgment, not of theoretical principle. His relation to the new classicals appears to be just the reverse: their empirical judgments are broadly similar while their theoretical paths

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to those judgments are, at times, strikingly different [(1984) 1990, 534].

Friedman, as one important monetarist, differs from the new classicals on a fundamental point of methodology: he is a Marshallian; they are Walrasians [(1984) 1990, 528].

This paper focuses on the differences between Friedman's and Lucas' concepts of equilibrium.<sup>2</sup> Moreover, it offers a new critical reading of Friedman's seminal paper, bringing to the fore its many ambiguities. In particular, I concentrate on the discrepancy between Friedman's narrative, which discusses variations in unemployment, and the model underpinning it, which actually does not allow for unemployment.

My attention will be focused only on Friedman's and Lucas' models. My aim is not to trace out either the antecedents of Friedman's model or the genesis of Lucas' ideas. This is why, for example, I do not broach Phelps's model [1967, 1968]. Nor will I examine papers that recast Friedman's insights in different lines, such as the search-theoretical perspective. Likewise, I do not mention the real business cycle literature. Finally, because I reflect only on theoretical and conceptual topics, the empirical dimension of the Phillips Curve problem will also be left aside.<sup>3</sup>

#### FRIEDMAN'S EXPECTATIONS-AUGMENTED PHILLIPS CURVE MODEL

One of the basic aims of Friedman's 1968 Presidential Address to the American Economic Association was to undercut the view that the negatively sloped Phillips Curve offered the opportunity for policy to permanently decrease unemployment. As put by Lucas:

The problem of reconciling the natural rate hypothesis with *some* adequate treatment of output and employment fluctuations is genuine and is not easy. To pass over it lightly is to ignore the motivation for virtually all recent developments in macroeconomic theory. What makes this problem difficult is that the basis of the Phelps-Friedman argument is the idea that monetary policy is basically a matter of unit changes, and unit changes should not have real consequences. If one accepts some idea of money neutrality as being central, how does one simultaneously accept the idea that instability in the quantity of money has been a principal source of real instability? If a particular policy variable has the power to account for historical employment movements up and down the business cycle, why can this power not be put to good use by deliberate manipulation? [Lucas, 1981b, 561]

Friedman's article has been tremendously influential.<sup>4</sup> Beyond doubt, its success was well deserved. Friedman put his finger on the right issues—expectations, misperceptions and credibility—thereby setting in motion a radical reorientation of

macroeconomics. Nonetheless, for all its path-breaking character, Friedman's address is far from being a model of clarity. Its central concepts are badly defined. It blends theoretical and empirical statements, jumping from assertions about Brazil's experiences to theoretical propositions that make sense only in a precise model—yet no precise model is present. In short, clarification seems necessary.

#### *Friedman's Argumentation*

Friedman's non-neutrality result follows from workers' misperception. In his words:

Because selling prices of products typically respond to an unanticipated rise in nominal demand faster than prices of factors of production, real wages received have gone down—though real wages anticipated by employees went up, since the employees implicitly evaluated the wages offered at the earlier price level. Indeed, the simultaneous fall *ex post* in real wages to employers and rise *ex ante* in real wages to employees is what enabled employment to increase. [1968, 10]

This passage states that the non-neutrality of money requires two conditions. First, monetary expansion must spill over differently in the goods and labor markets, so that nominal wages increase by less than average prices. Second, firms' and workers' expectations ought to be asymmetrical. While workers hold adaptive expectations about the goods prices, firms have perfect foresight.

Friedman's analysis starts from a state of equilibrium where the natural rate prevails. He assumes that the government nonetheless wishes to increase employment and therefore orders the central bank to engage in monetary expansion. Were agents without money illusion, the government's attempt would fail because agents would not respond to such a purely monetary shock. However, Friedman wants them to react positively in order to have a theoretical result supporting the empirical observation of a downward-sloping Phillips Curve. Hence the above two assumptions.

The consequence of monetary expansion is that workers and firms agree to trading a higher quantity of labor for a higher nominal wage. The snag is that the workers, because of adaptive expectations, associate a higher real wage with this higher nominal wage. In contrast, firms correctly associate it with a lower real wage. When the new nominal wage/employment level ratio is compared to the previous one, a trade-off between inflation and unemployment surfaces, confirming a downward-sloping Phillips Curve. However, as Friedman warns us, this is only half of the story:

But this situation is temporary: let the higher rate of growth of aggregate nominal demand and of prices continue, and perceptions will adjust to reality. When they do, the initial effect will disappear, and then even be reversed for a time as workers and employers find themselves locked into inappropriate contracts. Ultimately, employment

will be back at the level that prevailed before the assumed unanticipated acceleration in aggregate nominal demand. [1977, 14]

As goods markets close, workers realize that their expectations about their real wage were wrong. If the monetary expansion were a one-shot move, the labor market would quickly return to its normal equilibrium. To keep the higher level of employment, monetary expansion must continue at an increased rate. The lesson is clear: a departure from the natural rate is possible only if inflation is unexpected. Maintaining it requires an unsustainable permanent acceleration of the inflation rate. In other words, the labor market cannot permanently depart from the natural rate of unemployment.

### Critical Observations

**A Walrasian Definition of the Natural Rate of Unemployment?** According to Friedman, his notion of the natural rate of unemployment has a Walrasian lineage.

The "natural rate of unemployment" is the level that would be ground out by the Walrasian system of general equilibrium equations, provided there is embedded in them the actual structural characteristics of the labor and commodities markets, including market imperfections, stochastic variability in demands and supplies, the cost of gathering information about job vacancies and labor availabilities, the cost of mobility, and so on. [1968, 8]

In fact, this view is groundless. First, as many commentators have noticed [Hall, 1979, 154; Dixon, 1995, 64; Rogerson, 1997, 76], Friedman does not provide a real definition. For example, Hall states "this definition is hardly more than a list of things to think about" [1979, 154]. Second, there is no room in the Walrasian framework for notions such as jobs or unemployment, as Lucas has aptly argued [1987, 49, seq.]. To me, Friedman's reference to Walras should be viewed as a mere ecumenical gesture—and, for that matter, a rather surprising one, in view of Friedman's earlier, rather dismissive assessments of Walrasian theory.<sup>5</sup> Therefore, Friedman's definition should simply be put aside. Positively, the lineage of the natural rate notion should be looked for in Marshall's notion of normal equilibrium.<sup>6</sup>

**The Marshallian Conception of Equilibrium and Disequilibrium.** The Marshallian conception of equilibrium can be viewed as a sub-category of a broader conception, which Donzelli [1989] labels the "stationary equilibrium conception." Its hallmark is the intertwining of two equilibrium concepts, one of which is considered more fundamental than the other. Marshall called the more fundamental concept "normal equilibrium" and the less fundamental "market-day" or "temporary equilibrium."<sup>7</sup> This distinction spawns two further distinctions, between *market-day* supply and demand and *normal* supply and demand, on the one hand, and between adjustment towards market-day equilibrium and adjustment towards normal equilibrium,

on the other hand. The criterion for market-day equilibrium is market clearing, as expressed in the matching between market-day supply and demand. Market-day equilibrium is supposedly always realized. The condition for normal equilibrium is that agents have no incentives to change their behavior, which, put crudely, is tantamount to stating that the market-day and normal values coincide. Disequilibrium is defined as states where normal equilibrium is not achieved. Its existence is accepted as a recurrent outcome. This co-existence between disequilibrium and market clearing, which rings oddly to the ears of Walrasian economists, is a central feature of the Marshallian approach [De Vroey, 2000a].

On the related issue as to whether frictions are considered in the Marshallian framework, no single answer can be given. The non-instantaneous attainment of *normal* equilibrium can be explained in terms of frictions (i.e. the fact that adjustment is slow, e.g. because of a time-to-build factor). However, frictions play no role in attaining *market-day* equilibrium (i.e. market clearing).

The implications for the labor market are important. Accepting that this market should be analyzed on the pattern of the corn market—its specificity, as mentioned by Marshall, does not impinge on the formation of market-day equilibrium—we must conclude that Marshall's analysis does not have room for either involuntary unemployment or frictional unemployment, because both rationing and frictions (as related to the attainment of market-day equilibrium) are excluded.<sup>8</sup> Clearly, this conclusion is unpalatable. Marshall solved the dilemma by splitting the field of economics in two—value theory on the one hand and monetary and business cycle theory, on the other. Rationing (and in particular unemployment) was expelled from the former but was supposedly embraced in the latter. Nothing, however, was specified about the interrelationship between them.

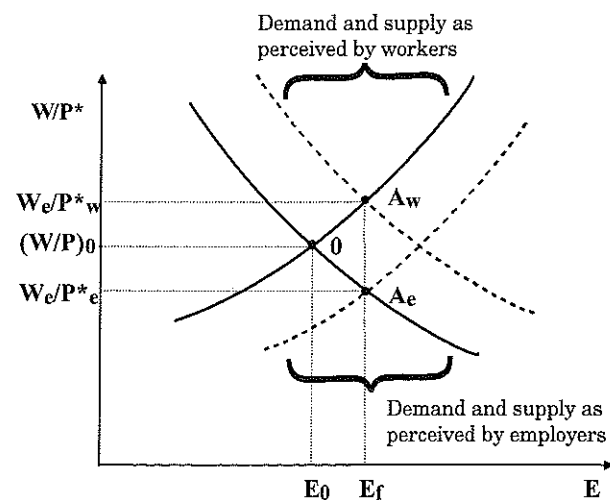
**Equilibrium and Disequilibrium in Friedman's Model.** My claim is that Friedman's model fits this Marshallian perspective. Admittedly, this cannot be clear from the onset. In particular, Friedman's address is about unemployment and its variations, while I have argued that unemployment is absent from elementary Marshallian theory.

The problem follows from the fact that Friedman's address is a narrative without model. Unemployment, understood in its vernacular meaning of job-searching, plays a central role within this narrative. The question to be raised is whether unemployment remains present when the model behind the narrative, in particular a theoretical account of the working of the labor market, is reconstructed. I will argue that it does not.<sup>9</sup>

The closest thing to a model that Friedman gives in his writings on the subject is his graphical exposition in his *Price Theory* textbook [1976, 221 seq.]. Its central graph is reproduced as Figure 1.

Without inflation, Friedman's reasoning runs, everything proceeds smoothly. Before entering the market, all participants know its equilibrium values, the coordinates of 0 in Figure 1, and the market outcome corresponds to the natural rate of

FIGURE 1  
THE FRIEDMAN PHILLIPS CURVE



employment ( $E_0$ ). However, as soon as inflation enters the picture, as the result of the monetary expansion, a different outcome surfaces. Consider the first trading round at which the impact of monetary expansion becomes visible in the labor market. Employees now develop a false conjecture about the prices of goods, the result of their adaptive expectations.

To them [the workers], the real wage that matters is their nominal wage divided by a price index of the goods and service they buy. As yet they have no reason to suppose a change in the price level, hence they have no reason to change their supply function. It will remain the solid supply curve on Figure 12.6 [Figure 1 above], if we interpret  $P^*$  as the price level *perceived or anticipated* by workers. To them, it will appear as if the demand for labor had shifted to the right, to the dashed demand curve. At each nominal wage rate (also real wage as perceived by them), employers are seeking to hire more workers. [Friedman, 1976, 224; his emphasis]

In other words, workers expect that a higher real wage will be associated with a higher nominal wage because they fail to anticipate that consumer good prices will rise. On the basis of this conjecture, they mentally construe the new market equilibrium as  $A_w$ , the intersection of the new perceived demand curve and the unchanged supply curve. As a result, they are ready to supply a greater quantity of labor.

Turning to firms, Friedman notes that:

Employers faced with an increased nominal demand for their products will count on being able to get a higher price or the equivalent. The same nominal wage means a lower real wage in terms of that higher price of his product. For employers as whole, it will appear as if the supply curve had shifted to the right to the dashed supply curve. [1976, 224]

This, in contrast, is not a mistaken assessment, because the workers' misperception is actually tantamount in its effect to a change in their preferences. Hence firms are ready to increase the quantity of labor hired to  $A_e$ , the point of intersection of their labor demand curve with the shifted supply curve. Endowed with their respective conjectures about the market equilibrium values, workers and firms come to the market with an exchange proposal that happens to involve the same increased amount of labor traded ( $E_f$ ) and the same nominal wage,  $W_E$  ( $W_E > W_0$ ). Exchange on this basis is possible and will take place.

This is Friedman's model of the labor market. The misperception assumption should not hide the fact that his model rests on a strong informational assumption. In Friedman's reasoning, the formation of labor market equilibrium involves workers and firms needing and being able to assess the market demand and supply schedules—and hence their intersection—on their own. Moreover, Friedman's model features market clearing: the market-day quantity of labor supplied and demanded are equal and amount to  $0-E_f$ . In other words, nothing in Friedman's graph points to the existence of unemployment, be it involuntary or frictional unemployment.<sup>10</sup> Except for the misperception aspect, this is a standard Marshallian market. Note finally that Friedman's natural rate of employment is nothing else than the quantity component of Marshallian normal equilibrium. In turn, the result described in Figure 1 can be viewed as a standard case of Marshallian disequilibrium, featuring a deviation of the market-day equilibrium values ( $E_f, W_E$ ) from their normal or long-period equilibrium values ( $E_0, W_0$ ).

Let me be more precise about the meaning of unemployment. It has two connotations. On the one hand, it implies that some economic agents are looking for a job without yet having one. On the other hand, the amount of employment, or the hours worked, must be unequally split across two categories of agents, the employed and the unemployed. None of these features is present in Friedman's model.<sup>11</sup>

Another pitfall is worth signalling. In reference to the real world it is usually taken for granted that an increase in employment is tantamount to a decrease in unemployment. This is not true in Marshallian theory (or in Walrasian theory, for that matter). Here, employment and unemployment cannot be converse concepts since there is no room for unemployment. The converse of changes in employment is rather variations in chosen leisure.

Friedman's model therefore significantly differs from his narrative. Because the former should prevail over the latter, the elements that are present in the narrative yet absent from the model, in particular unemployment, should be eliminated. De-

partures from the natural rate of *employment* rather than from the natural rate of *unemployment* then turn out to be the real object of Friedman's model. Every agent realizes his optimizing plan, at least in an *a priori* way. The issue under consideration is the possibility of variations in the level of activity. Assuming identical agents, the notion of a natural rate of employment indicates their normal equilibrium level of participation in the labor market as grounded in traditional microfoundations. In this context, a departure from the natural rate of employment indicates a level of activity below or in excess of this rate, that is either under- or over-employment.<sup>12</sup>

To repeat, any departure from the natural rate should be interpreted as a disequilibrium. However, disequilibrium should not be made tantamount to non-market-clearance, an association which is valid for the Walrasian equilibrium concept yet invalid for the Marshallian concept. Many authors have failed to perceive this point as the following examples illustrate.

Because of market frictions and structural changes, unemployment always is positive. By implication, then, there will be unemployment even when the economy is in general equilibrium, defined as the absence of excess demand in each market.... [Santomero and Seater, 1978, 515]

To me, such a statement is simply contradictory. How can market rationing—for this is what unemployment is tantamount to—and zero excess demand coexist?

Even Friedman, who with his natural rate hypothesis asserted the dominance of frictional unemployment interpreted as a consequence of voluntary actions, did not deny that involuntary unemployment was real. [Hoover, 1988, 36]

In this extract, Hoover takes it for granted that the category of involuntary unemployment is conceivable in Friedman's reasoning while I have argued for the opposite viewpoint.

He [Friedman] skillfully captured the mainstream by proposing that, while markets clear in the long run where classical results obtain, they do not necessarily clear at full employment in the short run because of a lack of perfect information on the part of all agents. [Carlin and Soskice, 1990, 74]

Here again it is taken for granted that any departure from the natural rate amounts to non-market-clearance.

In contrast, Tobin has it right.

Until I re-read Friedman's Presidential Address in order to write this chapter, I had the impression that Friedman accepted a Keynesian

non-market clearing explanation of unemployment in excess of the natural rate. [Tobin, 1995, 40]

***Introducing Expectations in a Self-Contained Period Analysis.*** Friedman wanted his model to feature a return to the natural rate. To this end, it was necessary to assume a stationary economic environment. That is, the basic data of the economy (underlying circumstances, technology, tastes, resources, etc.) are assumed to be constant over the period of analysis. The only variable liable to change is the money supply. In other words, the framework Friedman adopts is one of a self-contained period of analysis, an assumption typical of the stationary equilibrium conception.<sup>13</sup> The problem is that such a framework has little room for expectations. Why bother with them if no linkage between periods exists and the basic data remain unchanged in the period under study?

Closer scrutiny confirms this problem. In Friedman's model, expectations bear on events to occur within the self-contained period of analysis considered. To this end, some time-related subdivision must be introduced, without removing the constancy of data assumption. The following assumptions will do and are in fact made implicitly by Friedman. First, the self-contained period needs to be sub-divided into different rounds of exchange, all of which are underpinned by the same fundamentals yet differ in terms of the importance of the monetary shock they undergo and their place within the adjustment process. Second, each round of exchange needs to be subdivided into two stages, taking place sequentially and bearing on the operation of factor and goods markets respectively. Friedman's reasoning ought then to be interpreted as stating that expectations are formed on the opening of the factor markets and bear on those magnitudes which will prevail in the final goods markets. The latter take place second yet in the same round of exchange. These "intra-trading round" expectations are a far cry from intertemporal (or "cross-trading rounds") expectations, as they are to be found in Hicks' *Value and Capital* [1946] and later on in Lucas.

***Changes in the Natural Rate of Employment.*** As just stated, Friedman's model is concerned with a self-contained period of time during which the basic data of the economy are assumed to remain unchanged. The fact that his analysis takes the existence of normal equilibrium as its starting point and considers only shocks of a temporary nature implies that the earlier natural rate of employment will reassert itself at the end of the adjustment process. In this context, the natural rate of employment cannot but be considered given and unique. In the same vein, Friedman's statement that the natural rate can change from one period to another is trivial because periods are unconnected and the separation across periods implicitly rests on the assumption that they are based on a different configuration of data.

Unfortunately, Friedman is hardly aware of the limitations intrinsic to the background he adopts. This is evident from his discussion of the return to equilibrium in which he raises the question of how long the temporary departure from the natural rate of employment can last [1968, 11]. Answering this question is possible at the theoretical level: the adjustment time span will depend on the assumptions made on matters such as the interval separating rounds of exchange, the intensity of money

creation, the threshold at which inflation will be considered unbearable, etc. Assuming that some magnitudes can be assigned to these variables, the time span separating the initial shock and the return to the natural rate of employment can be assessed. The snag, however, is that such a reasoning works only if the basic data of the economy are assumed not to change during this adjustment period. As a result, any conclusion reached at the theoretical level—especially if it involves a long delay of adjustment—cannot be transposed to the real world, in so far as in the latter economic data are incessantly changing. In this light, Friedman's statement that full adjustment may take a couple of decades [1968, 11]—a statement which he presents as the empirical facet of his theoretical reasoning—is hollow.<sup>14</sup>

*The Role of Errors in Friedman's Model.* Above I have hinted that in the earlier neoclassical tradition value analysis and business cycle theory were separate. Expectations were viewed as belonging to the second of these fields. Although the concept of bounded rationality had not yet been invented, to all intents and purposes it was already put in practice—agents' expectations were often taken as erroneous.<sup>15</sup>

Friedman is heir to this error tradition, with the difference that errors now are introduced in the field of value theory.<sup>16</sup> They are present in a twofold way in his model. First of all the workers misperceive the prices of goods. Noticeably, in their case, these errors elicit no learning, which is why their behavior can be considered unintelligent and irrational. However, there is a second agent who makes mistakes in Friedman's model: the central bank (or the government, in so far as it control the central bank). As stated, the triggering element leading to the departure from the natural rate is the central bank's endeavor to increase the level of employment beyond its natural rate, definitely a mistake.<sup>17</sup> Likewise, the eventual return to the natural rate results from the government's awareness that its aim is unattainable and jeopardizes the monetary system. Unlike workers, the government is thus able to learn from its mistakes, which restores equilibrium.

### *Concluding Remarks*

Two conclusions can be drawn. First, my reconstruction makes it clear that Friedman's model is part of the Marshallian universe. In fact, this should come as no surprise, since he recurrently pledged allegiance to the Marshallian perspective whilst lambasting the Walrasian approach.<sup>18</sup> The second lesson is that, even amended, Friedman's reasoning remains flawed on several scores. First, a discrepancy exists between its alleged and its effective subject matter (variations in unemployment and variations in employment). A second and better-known flaw relates to the choice of assuming adaptive expectations. In Friedman's model, agents are constantly fooled. They not only make errors but also fall short of drawing any lesson from them. Finally, Friedman's reasoning suffers from the standard drawback encountered by most economists of his generation, of addressing dynamic issues in a static framework. It was up to Lucas to remedy these defects.

### LUCAS' "EXPECTATIONS AND THE NEUTRALITY OF MONEY" MODEL

Lucas' motivation for writing his "Expectations and the Neutrality of Money" article was to strengthen Friedman's policy ineffectiveness claim by giving it stronger micro-foundations and casting it in an explicit general equilibrium framework.<sup>19</sup> As stated in an interview with Snowdon and Vane:

My most influential paper on "Expectations and the Neutrality of Money" came out of a conference that Phelps organized where Rapping and I were invited to talk about our Phillips curve work. Phelps convinced us that we needed some kind of general equilibrium setting. Rapping and I were just focusing on labor supply decisions. Phelps kept on insisting that these labor suppliers are situated in some economy, and that you have to consider what the whole general equilibrium looks like, not just what the labor supply decision looks like. That's what motivated me.<sup>20</sup> [1998, 126]

Lucas' model is an overlapping generations model with agents living for two periods. There is one perishable good, produced by the young generation yet consumed both by young and old agents. The young are self-employed. They acquire fiat money, which does not enter the utility function, by selling the good to the members of the old generation, and spend it to purchase goods when old. The overall size of the population is fixed with an equal proportion of young and old people. Production decisions depend on the relative price of the good across the two periods. Because the substitution effect is supposed to outweigh the wealth effect, young agents will plan to consume more when old if they expect the next period price to be relatively low with respect to today's, and vice-versa.

A decisive assumption of the model, marking a breach both from Friedman and from Lucas' earlier work with Rapping [Lucas and Rapping (1969), 1991], is rational expectations, borrowed from Muth (1961). That is, agents are assumed to hold expectations that are consistent with the predictions of the theoretical model. A rational expectations equilibrium is then a market equilibrium in which traders use equilibrium market prices to draw inferences about their uncertain environment.

The set-up of Lucas' reasoning is explicitly stochastic with two types of stochastic shocks—a real and a nominal shock (Friedman's model only needs a nominal shock). The nominal disturbance follows from the fact that the members of the older generation receive a beginning-of-period money transfer proportional to their pre-transfer holdings of money. The real shock results from the fact that trade is supposed to take place in two physically separate places, each of them organized under the auspices of an auctioneer. Young agents are allocated stochastically across the two trading places, whereas old agents are equally distributed across them. As a result, when a young agent happens to be in a market with a proportionally low young population, thus facing a higher per capita demand, he will produce more and consume less in his young age, to consume more when old. The young are supposed to know the density functions of the two stochastic variables yet to ignore their drawing at the present

period. As a result, they face a signal extraction problem, because they need to sort out whether the changes in price they observe follows from either a nominal or a real shock.

Three results are obtained. First, young agents correctly interpret exclusively monetary shocks. Consequently, such shocks elicit no real effects. Second, an exclusive real shock is likely to generate changes in output. Young agents in the market with a less-than-average number of suppliers will increase their production and hold higher real balances. The reverse will be true for those in the thicker market. However, the two opposite movements will not necessarily balance each other. Finally, agents are unable to distinguish between concurrent real and monetary shocks because the available information bears on their joint effect. When certain conditions are put on the densities of the two shocks, Lucas' result is that "monetary changes have real consequences only because agents cannot discriminate perfectly between real and monetary demand shifts" [(1972) 1981a, 78]. The bottom line is that Friedman was right: a positive relationship between the rate of inflation and the rate of employment can be observed yet cannot be used as a policy tool.

Beyond doubt, Lucas' model shows a tremendous progress compared to Friedman's. First of all, instead of facing a paper mixing two types of discourse, the reader is presented with a rigorous and elegant model that succeeds in realizing Friedman's initial aim. As Lucas writes in the paper's conclusion, "the Phillips curve emerges not as an unexplained empirical fact, but as a central feature of the solution to a general equilibrium system" [(1972) 1981a, 84]. Introducing rational expectations has a far-reaching impact. On the one hand, workers' unintelligent behavior, which the adaptive expectations amounted to, is now swept away. On the other hand, the rational expectations hypothesis provides an operational solution to the difficult problem of modeling expectations.<sup>21</sup> In short, as Sargent claims [1996], Lucas' contribution has been mainly methodological. It blazed the trail for real business cycle theory and dynamic macroeconomics. Finally, this episode also illustrates the fact that theoretical innovations are not necessarily planned. I surmise that when Lucas started to work with Rapping on the supply of labor function, he hardly knew that its end result would be a new theory of the business cycle. Rather, the latter came across as the more or less unintended by-product of Lucas' attempt to give Friedman's claim a better microfoundation.

### *Lucas' Conception of Equilibrium*

In the introduction of "Expectations and the Neutrality of Money" Lucas underlined that the results of his paper were based on a new conception of equilibrium [(1972) 1981a, 67]. In this paper he did not elaborate on this claim. However, he returned to the subject in several subsequent writings. The following extracts illustrate:

Keynes founded that subdiscipline called macroeconomics, because he thought explaining the characteristics of business cycles was impossible within the discipline imposed by classical economic theory, a

discipline imposed by its insistence on adherence to the two postulates (a) that markets clear and (b) that agents act in their own self-interest.... When Keynes wrote, the terms *equilibrium* and *classical* carried certain positive and normative connotations which seemed to rule out either modifier being applied to business cycle theory. The term equilibrium was thought to refer to a system at rest, and some used both *equilibrium* and *classical* interchangeably with *ideal*. Thus an economy in classical equilibrium would be both unchanging and unimprovable by policy interventions. In recent years, the meaning of the term *equilibrium* has changed so dramatically that a theorist of the 1930s would not recognize it. An economy following a multivariate stochastic process is now routinely described as being in equilibrium, by which it is meant nothing more than at each point in time, postulates (a) and (b) above are satisfied. [Lucas and Sargent, 1978, 58]

I refer to this theory [the neoclassical synthesis] as static.... The underlying idea seems to be taken from physics, as referring to a system "at rest." In economics, I suppose such a static equilibrium corresponds to a prediction as to how an economy would behave should external shocks remain fixed over a long period, so that households and firms would adjust to facing the same set of prices over and over again and attune their behavior accordingly. [Lucas, (1980) 1981a, 278]

*Drawing the Line Between the Old and the New Conception of Equilibrium.* The exact target of Lucas and Sargent's criticism is less clear than it appears at first sight. Is it the stationary equilibrium concept in general (to which Friedman's model belongs) or, more narrowly, Keynes' project of demonstrating involuntary unemployment? Pointing to the first possibility is Lucas' characterization of the old conception as "equilibrium as a point of rest." However, it ought to be realized that both optimizing behavior and market clearing belong to the set of premises characterizing the stationary equilibrium approach, wherein they can coexist with disequilibrium states. Hence this approach cannot be criticized on the grounds of its failure to adopt them. Its indictment ought to be motivated on other grounds (those of its static and atemporal character). As for Keynes, beyond doubt, he wanted to get rid of market clearing. Yet as far as may be surmised, he wanted to generate this result within the stationary equilibrium tradition.<sup>22</sup> Therefore, if the aim is just to criticize Keynes' specific theoretical project, it is unnecessary to attack the stationary equilibrium at large; such a criticism can be leveled from within. After all, this is exactly what Friedman did.

Consequently, the two postulates underlined in the above quotations, optimizing behavior and market clearing, should not be viewed as the real originality of Lucas' equilibrium conception. In my view it lies in a series of other factors. A first striking novelty bears on the extension of the field of relevance of value analysis—and thus of the equilibrium concept—bringing about the abolition of the earlier divide between

value and business cycle theory. Other differences concern the methodological status of equilibrium, the more rigorous treatment of expectations and, finally, the exclusion of disequilibrium from the range of possible effective outcomes. In the sequel, I will mainly insist on the first and the third of these factors.

**Equilibrium: Not a Property of Reality.** Economists of the stationary equilibrium tradition believe that it is possible to assess the existence of equilibrium in reality. Although few of them would defend that equilibrium is often attained in reality, they would all claim that a tendency towards equilibrium is always at work. Hence they would surely be ready to underwrite statements such as Viner's: "The ordinary economic situation is one of disequilibrium moving in the direction of equilibrium rather than of realized equilibrium" [1953, 206]. Although Friedman did not endorse the above conception explicitly, it may safely be presumed that he was in full sympathy with it.

However, Lucas views the matter differently, as the following statements testify to:

I think general discussions, especially by non-economists, of whether the system is in equilibrium or not are almost entirely nonsense. You can't look out of this window and ask whether New Orleans is in equilibrium. What does that mean? Equilibrium is a property of the way we look at things, not a property of reality. [Snowdon and Vane, 1998, 127]

Lucas' argument is not that the statement "equilibrium can exist in reality, yet in its absence disequilibrium is existing" should be replaced by the statement "equilibrium is always existing in reality." Rather, he wants to dismiss the common premise underlying these two statements, namely that both equilibrium and disequilibrium are possible characteristics of reality. Thereby, his remark amounts to circumventing the immediate criticism which an endorsement of the second statement would otherwise elicit, namely that the ever-existence of equilibrium is too strong a claim to make, as it flies in the face of evidence.<sup>23</sup> In fact, if Lucas' observation is taken seriously, it should be concluded that he holds an agnostic stance about whether markets effectively clear in reality. Either this point simply cannot be assessed or non-market-clearance, although an effective reality, may nonetheless be sidestepped when it comes to the construction of a theory, due consideration being given to its purpose.

His reflections on unemployment in his *Models of the Business Cycle* [1987] are worth referring to in this respect. His claim is not that unemployment does not exist but rather that a business-cycle model can dispense with it. His models are a case in point, as they study the variations of *employment* over the cycle, without assuming the existence of *unemployment*, i.e. the unequal allocation of total employment across workers.

In most such models [of the business cycle] unemployment as a distinct activity plays no role whatever. For many other economists, explaining business cycle is taken to *mean* accounting for recurrent episodes of widespread unemployment. From this alternative viewpoint,

a model with cleared markets seems necessarily to miss the main point, however successful it may be accounting for other phenomena, and the work of "equilibrium" macroeconomists is often criticized as though it were a failed attempt to explain unemployment (which it surely does fail to) instead of as an attempt to explain something else.<sup>25</sup> [(Lucas, 1987), 48].

**Banishing Errors.** Errors, as defined above, disappear from the scene in Lucas' model. Following Lindahl [(1929) 1939], and Hayek [(1928) 1984] when they addressed the issue of extending Walrasian analysis intertemporally, Lucas starts from assuming perfect foresight. Like them, he uses it as a negative benchmark from which to depart. However, his route is significantly different from theirs: Hayek and Lindahl (as well as Hicks) conceived the departure from perfect foresight outcomes as a result of the fact that agents, in particular firms, were making forecasting mistakes, thereby treading the standard "errors" and bounded rationality route mentioned above. Lucas' own line is different. According to him, departures from perfect foresight are due to objective imperfections in the information structure rather than agents' difficulties in coping with uncertainty. Hence agents' errors play no part in the fact that the economy behaves differently in an imperfect information and a perfect information context. Were agents given a second chance, they would make the same decision, which would not be true in the error paradigm. Their possible frustration is due to the stochastic character of states of nature rather than to their having made erroneous decisions. Lucas does not doubt that the changes he is making marks a progress. In his terms:

In an important sense the new scenario is an improvement since in place of the unexplained errors of judgment or ignorance that lie at the center of Hume's account, this one rests on an assumption that people lack complete information.<sup>25</sup> [Lucas, 1996, 676]

This marks a sharp contrast with Friedman's perspective. The banishing of the error idea results in abandoning its corollary, namely that agents ought to and can learn from their errors and correct them over time.

**The Exclusion of Disequilibrium.** Lucas has been a fierce opponent of the so-called disequilibrium approach, such as is found in the works of Barro-Grossman, Dreze, Benassy and Malinvaud, etc. All these authors, it should be noted, reason in a Walrasian framework and aim at demonstrating the possibility of market non-clearance at a given point in time (i.e. to have a temporary-equilibrium-cum-rationing result). Thus, their conception of disequilibrium is totally different from the Marshallian (and Friedmanian) view, where disequilibrium refers to a process occurring over time and goes along with market clearing. Lucas' argument consists of outflanking their claim, along the lines mentioned above, by admitting that the Walrasian approach is entirely unable to conceptualize notions such as an employment relationship, a job or unemployment, owing to the fact that it rests on the assumption of *tâtonnement*. If no room exists for unemployment, *a fortiori* there is none for involuntary unemployment [Lucas, 1987, 52-3].



Friedman's model has been characterized above as a disequilibrium model. It features states where effective market values are distinct from their normal (or long-term) values over a succession of trading rounds. Deviations from some benchmark values (perfect information) are also present in Lucas' model. Should the latter then also be viewed as a disequilibrium model? I do not think so. At issue is how such deviations should be interpreted. That is, should they be viewed as indicating a state of disequilibrium or as a deviation *tout court*, not to be put under the disequilibrium label? The answer to this question hinges on the existence of some convergence process, this being in turn related to the presence of errors and learning.<sup>26</sup>

My claim is that, contrary to Friedman, Lucas' model should not be interpreted as displaying any convergence from disequilibrium towards equilibrium. Several reasons can be given. First, there are no errors to begin with. Deviations are not caused by a central bank going astray. Agents commit no mistakes; their behavior is the most rational possible. Second, the time-path followed by the economy characterized by perfect information cannot be viewed as the attractor of that of the imperfect information economy. Nor should the observed deviation be called a disequilibrium, because it elicits no corrective or adjusting behavior. True, opposite deviations will cancel each other out after a sufficient number of trading rounds, yet this phenomenon expresses an averaging-out process. It does not result from any re-equilibrating pressure. Third and finally, in Friedman's model, out-of-equilibrium and equilibrium refer to two states of the same economy. In Lucas' model, two different economies are compared, one in which perfect information prevails, another in which one has imperfect information.

The difference between the two approaches can be shown in a slightly different way by reflecting on the statement that in the long-run the Phillips curve is vertical. This statement is true for both authors' models, yet in different ways. Set against the Friedmanian background, it means that the Phillips curve is vertical at equilibrium and only at equilibrium. This one-to-one association between equilibrium and the verticality of the Phillips curve becomes invalid when it comes to a Lucasian interpretation of the statement, since equilibrium exists in the latter even when the Phillips curve is not vertical. Moreover, the term of "long-period" receives a different meaning in the Friedmanian and Lucasian contexts. In Friedman's analysis, "long period equilibrium" should be understood as meaning "normal equilibrium" in the Marshallian sense. His suggestions to the contrary notwithstanding, the "long-run" modifier should not be taken in any literal sense. In fact, it should receive no connotation of duration—no implication that the convergence process is lengthy or that the equilibrium in point will prevail for a long duration should be made.<sup>27</sup> In contrast, in Lucas' reasoning the long-run term should be understood in its vernacular meaning. In a context of imperfect information, the long-run verticality characteristic of the Phillips curve simply means that deviations of opposite signs cancel out if the observation time is sufficiently long.<sup>28</sup>

To conclude, the change in model from Friedman to Lucas runs from a model where equilibrium is viewed as an attractor for effectively existing disequilibrium states to a type of modeling where any reference to disequilibrium has become totally

superfluous. It is relevant neither for point-in-time nor for intertemporal outcomes. To all intents and purposes, Lucas' construction abandons the concept of equilibrium over time. It only comprises a succession of point-in-time equilibria. The time-path thus formed may be evaluated against a benchmark time-path (the perfect-information time path) yet no convergence towards it should be conceived of. Hence no statements about the realization of equilibrium over time or the lack thereof should be made.<sup>29</sup>

## CONCLUDING REMARKS

The above analysis has given, I hope, sufficient ground to the claim that a basic methodological difference between Friedman's and Lucas' models lies in their being underpinned by different equilibrium conceptions. Three differences have been underlined. First, Friedman's model can be characterised as a disequilibrium model, while there is no room for disequilibrium outcomes in Lucas' model. Second, Friedman's model belongs to the epistemological perspective wherein equilibrium and disequilibrium are considered to be characteristics of the real world. In contrast, Lucas denies the relevance of such a characterization and claims that equilibrium is a discipline that economists need to adopt, a characteristic of the way they look at reality. Third and better known, as far as agents' individual equilibrium is concerned, Lucas has scraped away the error/bounded rationality paradigm, which underpinned Friedman's reasoning by introducing rational expectations.

Actually, Friedman and Lucas are well aware of their methodological differences, as the following extracts from interviews by Snowdon and Vane testify :

Question [to Lucas]: *You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you?* Answer: I like mathematics and general equilibrium theory. Friedman didn't.... Question: *His methodological approach seems more in keeping with Keynes and Marshall.* Answer: He describes himself as Marshallian, although I don't know quite what it means. Whatever it is, it's not what I think of myself. [1998, 132]

Question [to Friedman]: *Kevin Hoover has drawn a methodological distinction between your work as Marshallian and that of Robert Lucas as Walrasian. Is that distinction valid?* Answer: There is a great deal to that. On the whole I believe that is probably true. I have always distinguished between the Marshallian approach and the Walrasian approach. I have always been personally a Marshallian. [1997: 202]

My claim is not that Lucas and Friedman's approaches are poles apart in every respect. First, as seen, both of them discuss variations in employment rather than in

unemployment. This in turn reflects the similarity between the Marshallian and the Walrasian approaches on the realization of market clearing. The second similarity between Friedman and Lucas is that each resolves the issue of policy efficiency by assuming it away. Let me make the point in reference to Friedman, by using the following analogy. Testing the efficiency of a drug on somebody who is in good health and discovering that it makes him sick can hardly be considered a good test for asserting its effect on people suffering from the illness that it is supposed to cure. This criticism can be applied to Friedman's argumentation. Friedman assumes away any reason for engaging in monetary expansion to begin with. As soon as the analysis starts from the assumption that normal equilibrium is realized, its result is sealed. Even to Keynesians [Hahn, 1982, 74-5], it is nonsense to engage in expansionary measures in such a context. The flaw in Friedman's argument is that it justifies policy ineffectiveness only in cases where policy is blatantly unnecessary. The same conclusion can be drawn regarding Lucas: he demonstrates ineffectiveness in a context where policy interventions have no *raison d'être*, as issues of co-ordination are discarded by assumption. As stated by Hahn and Solow:

The irony is that macroeconomics began as the study of large-scale economic pathologies: prolonged depressions, mass unemployment, persistent inflation, etc. This focus was not invented by Keynes (although the depression of the 1930s did not pass without notice). After all, most of Haberler's classic *Prosperity and Depression* is about ideas that were in circulation before *The General Theory*. Now, at last, macroeconomic theory has as its central conception a model in which such pathologies are, strictly speaking, unmentionable. There is no legal way to talk about them [1995, 2-3].

Finally, a broader result of my reflections concerns the recent unfolding of macroeconomics. Put crudely, the standard view is that "Keynesianism" was overthrown in a two-step revolution, the first stage of which is associated with monetarism and the second with new classical macroeconomics. Such a formulation suggests a line of continuity between monetarism and new classicism. On the contrary, I argue that the watershed should be located between Friedman and Lucas. In other words, the real divide separates an era of "Marshallian macroeconomics" from one of "Walrasian macroeconomics." The first era was a period where the IS-LM apparatus was the cornerstone of macroeconomic thinking, shared by both friends and foes of Keynes, Friedman being the best example of the latter group. The second era marks the dethroning of IS-LM and its replacement by a new theoretical apparatus, the dynamic macro model. Whereas the latter was initially geared towards defending anti-Keynesian views, this will, and has already, started to change.

## NOTES

Comments on an earlier draft by Neil de Marchi, Dan Hammond, David Laidler, John Seater, Howard Vane and three anonymous referees are gratefully acknowledged.

1. It should be immediately observed that characterizing Friedman's as a disequilibrium model does not imply that it features non-market-clearance.
2. My claim is not that the above mentioned authors have totally neglected the issue of equilibrium. For example, Hoover [(1984) 1990] has a section dealing with equilibrium and dynamics. However, I believe that my paper puts the contrast between Friedman and Lucas is a sharper way than Hoover, which should come as little surprise in view of the time span which elapsed from his paper to mine. In particular, Hoover does not underline that Marshallian disequilibrium goes along with market clearing. Nor is he aware of the contrast, to be emphasised below, between equilibrium as a characteristic of reality and equilibrium as a characteristic of the way in which we look at reality.
3. For a more contextual analysis and a discussion of Friedman's originality, see Leeson [1997a; 1997b].
4. A testimony to this is its citation counts as recorded in the Social Sciences Citation index. On this, see Snowdon and Vane [1998, 117].
5. On this point, see Friedman [(1949) 1953: 90] and Gordon [1974, 145]. Hartley [1977, 91] comments on Friedman's position.
6. De Vroey [1999a; 1999b; 2000b] argue that the Marshallian and the Walrasian approaches should be separated more sharply than they usually are.
7. Even if the Marshallian terminology is hardly used today, I will stick to it because its modern incarnation, a distinction between short- and long-period equilibrium, is beset with ambiguity.
8. Of course, it has room for chosen leisure and the latter is sometimes labeled "voluntary unemployment". This terminology, however, should be rejected: being unemployed has the meaning of looking for a job while the hallmark of chosen leisure is to dismiss the opportunity of taking a job. Nor has the elementary Marshallian model room for voluntary unemployment in the present-day common-sense meaning of this term, designating the case of people choosing to be on the dole while pretending to be looking for a job.
9. The investigation in point must bear on the characteristics of Friedman's own model rather than on the issue as to whether any natural rate model, incorporating unemployment as its basic category, can be conceived of.
10. It is tempting to assume that unemployment exists in Friedman's model under the form of search unemployment. However, this view cannot be accepted, because, as stated by Hahn, "Traditional search theory finds no *formal* representation of the economy in macro theories of the natural rate. It is referred to, or better appealed to, but it is not connected with the theory proposed" [1995, 52]. See also Rogerson [1997] who confronts Friedman's categories with the search approach and makes it clear that his model is far from being a search model.
11. Several reasons may explain the contrary impression: first, his narrative; second, the fact that empirical discussions about the Phillips Curve have always been concerned with variations in unemployment; and third, the fact that other models are effectively about unemployment.
12. Friedman awkwardly uses the expression of "overfull employment" [1976, 223].
13. In Donzelli's terms, self-containedness amounts to assuming that "all activities performed by the economic agents during a given time period can have no effect outside that period, so that all intertemporal links between different periods are severed" [1989, 35].
14. Friedman should have learned from Stigler, his old compeer, that theoretical concepts should not be confused with statistical averages: "The average price of wheat over a 30-year period is not its long-run normal price. Long-run demand and cost conditions do not remain fixed for 30 years" [1946, 148].
15. According to Laidler [1999], "The central factor at work in the cycle theory, which Lavington set out in 1922, and of which Pigou was the major exponent, was encapsulated by the word *error*. In Pigou's view, the forward-looking nature of investment decisions required that 'expected facts are substituted for accomplished facts as the impulse to action' and that in turn would create scope for 'errors of undue optimism or undue pessimism in ...business forecasts'" [1999, 84].
16. By error I mean any decision in which agents would not engage a second time if they were receiving a new chance to make it. It will be seen that errors, so defined, are absent from Lucas' model.

17. The monetary expansion should be viewed as an error in that a target has been set (a sustainable increase in employment beyond its natural rate) which *ex post* is proven to be unattainable and to require a policy reversal. It could be argued that the government is well aware of the unsustainability of its policy yet is just engaging into intertemporal substitutability. Yet this hardly fits what both defenders and foes of the employment/inflation trade-off had in mind.
18. Hammond states "When Friedman put a label on his methodology, it was Marshallianism. Though he did not use this label in the 1953 essay, he used it regularly over a half century, and it is the best descriptor for his methodology" [1996, 30].
19. An interesting yet unfortunately unpublished paper on the genesis of Lucas' ideas is de Marchi [1990]. For an investigation of the methodological and epistemological foundations of his theory, see Vercelli [1991].
20. See also Lucas' introduction to *Studies in Business-Cycle Theory* [1981a].
21. To gauge the progress made in this respect, one has only to compare Hicks' conceptualization of expectations in *Value and Capital* with Lucas'.
22. The standard Marshallian stance is that disequilibrium can exist as a temporary phenomenon, characterized by the fact that markets clear at values different from their normal values. Keynes' aim was then to turn this result upside down, by conceiving of states of market-day disequilibria which could be considered as featuring normal equilibrium [De Vroey, 1999c].
23. Most interpreters have assumed that Lucas was making such a claim. Two examples are worth giving: "The 'new classical macroeconomists' claim that markets do clear at every instant—in the Walrasian sense—in actual economies" [Grandmont, 1983, 2]. "I have probably to remind you that an important school of thought in modern economics chooses to deny everything. Its members argue that supply and demand actually do balance in the labor market as they do in the fish market" [Solow, 1990, 28]. See also Malinvaud [1984, 18 and seq.].
24. As far as unemployment *per se* is concerned, Lucas argues that it should be studied "as an individual problem, identical in character in business cycle peaks and troughs (though more people have this problem in troughs)" (1987: 67), an investigation to be undertaken under the distinct banner of search theory rather than that of the business cycle.
25. Lucas' assertion that progress has occurred should not be accepted at face value. His stance should rather be viewed as a clever way of avoiding the thorny issues of learning and convergence.
26. As stated by Hicks in *Capital and Growth*, "it is necessary, if the equilibrium assumption is to be justified, that we should be able to assert the existence of a tendency to equilibrium" [1965, 17].
27. This is why I dislike the replacement of the Marshall market-day/normal equilibrium divide by the short-run/long-run distinction.
28. If agents hold perfect information, the Phillips curve is vertical over any timespan, short or long.
29. This conclusion pertains only to Lucas' Neutrality model. It should be extended neither to temporary equilibrium models *à la* Grandmont [1977], as they belong to the error paradigm, nor to subsequent real business cycle models. On this, see De Vroey [2000b].

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