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Market Nash Equilibrium, Secular Equilibrium and Centre of Gravitation from Hayek to Hahn

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a *Gingi.*

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ABSTRACT

The present dissertation attempts to shed light on the reasons behind the failure of marginalist price and distribution theory to explain the trends in actual economies, by examining the change undergone by the notion of equilibrium in the course of the development of the marginalist approach since the 1930's, when the value specification of the capital endowment starts being replaced by a vector of physically heterogeneous capital goods in the general equilibrium equations, therefore determining an equilibrium (or a sequence of such equilibria) which, contrarily to the traditional neoclassical equilibrium, is deprived of the sufficient persistence necessary for it to be approached by actual – time-consuming – adjustment processes, and is therefore unable to have the role traditionally assigned to equilibria: to be a *centre of gravitation* of actually observed variables. The thesis covers the works by Hayek, Lindahl, Hicks, Lange, Allais, Lucas, Malinvaud, Bliss, Arrow and Hahn; it claims that with the exception of this latter scholar, who is clear that modern general equilibrium theory cannot be argued to have any explanatory-predictive role, the rest of the authors here examined are in fact *inconsistent* with the notion of equilibrium they allegedly adopt: despite their claim to have abandoned the illegitimate traditional notion of capital as a single factor measured in value terms, they continue to believe in the neoclassical tendencies that stand or fall with that conception. Therefore, it is shown that in some way or another these authors still reason *as if* things continued working in the way justified by this illegitimate notion of capital, something that has been shown to be deprived of solid theoretical foundations during the *Capital theory debates* of the 1960's and 1970's.

The main conclusion of this dissertation can be briefly summarized in the following way: the marginalist approach is unable to satisfactorily incorporate capital goods within the supply and demand explanation of prices and distribution, and this seems to have condemned modern general equilibrium theory to face an *unpleasant dilemma*: either this theory is, like Hahn, truly consistent with its own theoretical object, a notion of equilibrium that must be *silent* on the issue of how actual economies work; or, alternatively, the theory attempts to provide some explanation of the working of actual economies and,

in so doing, it must inescapably rely on traditional gravitational ways of reasoning that must presume the illegitimate notion of capital in value terms. Either way modern general equilibrium theory runs into a blind alley, with the implication that, to predict with reasonable accuracy price, quantity and distributional trends as observed in actual economies, an explanation with different theoretical foundations is necessarily called for.

TABLE OF CONTENTS

PART I. SETTING THE GROUNDS: THE DIFFERENT NOTIONS OF EQUILIBRIUM WITHIN NEOCLASSICAL THEORY	1
CHAPTER 1. THE DIFFERENT NOTIONS OF EQUILIBRIUM WITHIN NEOCLASSICAL ECONOMIC THEORY	2
Introduction: aims of this dissertation and summary of the argument	2
1. Equilibrium as a centre of gravitation	12
1.1. A first feature of the centre of gravitation: stability	14
1.1.1. The tendency towards equilibrium and the mechanisms of substitution	19
1.2. A second feature of the centre of gravitation: persistence	22
1.2.1. Cost-of-production relative prices as equilibrium relative prices	26
2. Normal prices in neoclassical theory and the endowment of capital	28
2.1. Supply side roles of capital	28
2.1.1. The Role of capital as a single quantity in Schumpeter's The theory of economic development (1934)	31
2.2. Demand side roles of capital	35
3. The problems with the endowment of capital in value terms.	37
4. The notion of secular equilibrium	41
5. The notion of Market Nash equilibrium	46
5.1. Walras's inconsistency	46
5.2. Neo-Walrasian equilibria	47
5.2.1. Neo-Walrasian equilibrium cannot be a position that the economy tends to realize I: the hypothesis of complete markets	49
5.2.2. Neo-Walrasian equilibrium cannot be a position that the economy tends to realize II: Insufficient persistence.....	52
5.2.3. Neo-Walrasian equilibrium cannot be a position that the economy tends to realize III: the illegitimacy of the assumption of full-employment	54
5.2.4. Neo-Walrasian equilibria as Market Nash Equilibria	57
6. Concluding remarks.	59
 PART II. THE NOTION OF EQUILIBRIUM AMONG THE FIRST GENERATION OF NEO-WALRASIAN SCHOLARS.	62
CHAPTER 2. HAYEK'S THE PURE THEORY OF CAPITAL (1941).	63
Introduction	63

1. Equilibrium analysis in PTC	64
1.1. Hayek's assessment of traditional theory: the assumption of stationarity and the endowment of capital	64
1.1.1. The problem of the capital endowment	65
1.1.2. The demand for capital	67
1.1.3. Static or secular stationary equilibria? Hayek's assessment of the stationary assumption in traditional neoclassical theory.....	67
1.2. Market Nash Equilibrium in PTC	69
1.3. The secular equilibrium in the PTC	71
1.4. The notion of MNE as a tool for description of actual economic states: an example	75
2. The roundaboutness of production methods and the traditional conception of capital	78
2.1. The analysis of technical choices	79
2.2. Long period technical choices	90
2.3. The traditional notion of capital as a flow of savings	97
3. Concluding remarks	102
 CHAPTER 3. LINDAHL'S STUDIES IN THE THEORY OF MONEY AND CAPITAL (1939)	105
Introduction	105
1. Equilibrium analysis in STMC	106
1.1. The stationary state and the endowment of capital	106
1.2. Lindahl's uneasiness with respect to the temporary equilibrium method.....	112
1.2.1 The indeterminacy of the temporary equilibrium path.....	112
1.2.2. An economy that is <i>always</i> in equilibrium	115
1.3. Convergence of MNE prices to a stationary state	118
2. The interest rate and the savings-investment market	124
3. Concluding remarks	127
 CHAPTER 4. HICKS'S VALUE AND CAPITAL (1939)	130
Introduction	130
1. Equilibrium and the endowment of capital	131
1.1. Hicks's early writings and the traditional conception of equilibrium and capital	131
1.1.1. The plausibility of the factor substitution mechanisms	131
1.1.2. Walras's inconsistency	134

1.1.3. The relative persistence of the data.....	136
1.1.4. Adjustments take time.....	138
1.2. Hicks’s misgivings about the traditional concept of equilibrium in VC	140
2. The Rehabilitation of the traditional conceptions of equilibrium in VC ...	142
2.1. Hicks’s uneasiness with the notion of secular equilibrium	142
2.2. The “not very convincing” hypothesis of instantaneous equilibration	144
2.3. The factors of stability	146
2.3.1. Normal prices	146
2.3.2. Price rigidities	148
3. Investment and the rate of interest in VC	151
3.1. The ‘tilting’ of the production plan	152
3.2. Hick’s average period of the production plan	155
3.3. Some remarks on the assumption of given expected prices in the derivation of the investment function	160
4. Final considerations: on Hicks’s recantation of the neo-Walrasian approach	162
Appendix: On Hicks’s average period of production	164

CHAPTER 5. LANGE’S (1944) PRICE FLEXIBILITY

AND EMPLOYMENT.....	171
Introduction	171
1. Equilibrium as a centre of gravitation in Lange’s early writings	172
1.1. “Money-capital” as accumulated savings	173
1.2 The role of the <i>direct</i> and <i>indirect</i> labour	177
1.3. A persistent, non-stationary equilibrium	180
2. The shift in method in Lange’s PFE (1944): significant change in form, minor change in content	185
2.1. The problem relative to measuring the capital endowment in value terms. The investment function	185
2.2. The analysis of accumulation	189
2.3. Some ‘methodological’ difficulties ignored by Lange in PFE	191
3. Lange’s objections to the self-adjusting nature of the market	194
4. Some remarks on Lange’s equilibrium and stability analysis	197
4.1. Lange and Keynes	197
4.2. Lange, Patinkin and the neoclassical synthesis	198
5. Concluding remarks	203

PART III. MAURICE ALLAIS’S NOTION OF EQUILIBRIUM	205
CHAPTER 6. ALLAIS’S TRAITE D’ECONOMIE PURE	
AND ÉCONOMIE ET INTÉRÊT	206
Introduction	206
1. Allais’s conception of equilibrium	209
1.1. The realism of the assumptions	209
1.2. Equilibrium should be stable and persistent	209
2. Intertemporal equilibrium and stationary equilibrium in Allais’s work ..	216
2.1. Intertemporal general equilibrium (IGE) in TEP	216
2.2. Allais’s misgivings about the intertemporal equilibrium I: the unrealistic assumptions of complete markets and finite life-span	217
2.3. Allais’s misgivings about the intertemporal equilibrium II: the problematic assumption of perfect foresight	220
2.4. Allais’s treatment of the vectorial endowment of capital goods.....	225
3. Capital as a homogeneous magnitude in EI	229
3.1. The demand for capital in stationary equilibrium	230
3.1.1. Some qualifications on Allais’s average period of production	236
3.1.2. The connexion between the demand for capital as a stock and the demand for capital as a flow in stationary equilibrium.....	238
3.2. The supply of capital in value terms and the stationary state.....	241
4. Concluding remarks	243
APPENDIX A: The quantity of capital in Allais’s OLG model	245
APPENDIX B: The demand for capital originaire” and the rate of interest	253
APPENDIX C: The average period of production under compound interest	256

**PART IV. THE NOTION OF EQUILIBRIUM AMONG THE
SECOND GENERATION OF NEO-WALRASIAN SCHOLARS**.....261

CHAPTER 7. LUCAS AND MALINVAUD: NEOWALRASIAN	
EQUILIBRIA AS CENTRES OF GRAVITATION	262
Introduction	262
1. Lucas on equilibrium and capital	264
1.1. Friedman and the Philips Curve	264
1.2. Lucas: Rational expectations and instantaneous adjustments	266
1.3. Lucas as a traditional neoclassical scholar	269
1.3.1. The traditional conception of capital in Lucas’s framework	272

2. Malinvaud on equilibrium and capital	275
2.1. Adjustments take time	275
2.2. The plausibility of the full-employment hypothesis.....	278
2.3. Capital-labour substitution	280
3. Concluding remarks	283

CHAPTER 8. HAHN: NEO-WALRAISAN EQUILIBRIUM AS MARKET NASH EQUILIBRIUM

Introduction	285
1. Hahn’s critiques of neo-Walrasian equilibria as centres of gravitation ...	287
1.1. Objections to the assumption of uniqueness of the full-employment path	287
1.2. Hahn’s rejection of the auctioneer	291
2. Hahn’s assessment of neo-Walrasian equilibria	294
2.1. Bliss on neo-Walrasian equilibrium	295
2.2. Hahn’s position on equilibrium	298
2.2.1. Neo-Walrasian equilibrium as Market Nash Equilibrium	298
2.2.1.1. The “abstract” problem of equilibrium relative prices	299
2.2.1.2. Intertemporal equilibrium as a benchmark	302
2.2.1.3. Was it really “always understood” that Arrow-Debreu is not a description of reality?	304
2.2.2. Equilibrium requires “reachability”	308
3. General conclusion: Neo-Walrasian equilibrium as Market Nash Equilibrium, or the gradual abandonment of traditional gravitational ways of thinking.....	313

REFERENCES.....318

PART I

SETTING THE GROUNDS: THE DIFFERENT NOTIONS OF EQUILIBRIUM WITHIN NEOCLASSICAL THEORY

1.

THE DIFFERENT NOTIONS OF EQUILIBRIUM WITHIN NEOCLASSICAL ECONOMIC THEORY.

INTRODUCTION: AIMS OF THIS DISSERTATION AND SUMMARY OF THE ARGUMENT.

Currently, neoclassical theory appears particularly unable to provide an accurate understanding of the working of actual economies, and the present contribution attempts to shed some light on the reasons behind this failure. In particular, I will argue that this lack of correspondence between theory and observation is an *expression* of a major shortcoming: the supply-and-demand approach to prices and distribution is unable to provide a satisfactory treatment of two *basic* and *indissolubly* linked concepts in economics: the notions of *equilibrium* and of *capital*.

This dissertation is divided into four parts. In order to set the grounds of the discussion, this first part (chapter 1) introduces the reader the three notions of equilibrium that have played a decisive role in the development of neoclassical price-and-distribution theory since its birth in the late 19th century: the notion of ‘moving equilibrium’ or ‘static’ stationary state, the notion of ‘secular’ stationary state and the modern or ‘neo-Walrasian’ notions of equilibrium. It is first argued that only the static stationary state, the dominant equilibrium concept among marginalist theory up to the first decades of the 20th century, is susceptible of having the role of a centre of gravitation of actually observed variables, namely, a stable and persistent position of rest. The main reason is that an equilibrium understood as a position around which actual prices and quantities are continuously gravitating requires *per force* an *endogenous* determination of the capital stock, and within marginalist theory, this condition can be satisfactorily fulfilled *only if* the capital endowment of the economy is specified as a *given* scalar, a quantity of a single factor (‘embodied’ in the several capital goods) that, during the process of adjustment, is capable of changing ‘form’ (i.e. composition, the capital goods in which this factor is ‘embodied’) but

hardly changes in ‘quantity’^[1]. As I discuss in this first chapter, this quantity of capital must be measured as an amount of exchange value. But the notion of capital as a single factor measured in value terms is logically flawed and so, subsequent developments in neoclassical theory attempt to do without it.

However, it is precisely because the other two main notions of equilibrium that will be discussed here do not include a value measurement of capital among their data that they lose their possible roles of centres of gravity of actually observed variables, and hence they are unable to have a correspondence with observation. As to the secular equilibrium, I will argue that, despite the fact that this equilibrium too determines the composition of the capital stock endogenously, it presents two main drawbacks that appear to undermine the usefulness of this notion to indicate the actual trends of prices and quantities. First, actual economic conditions are far from being *strictly* stationary as implied by the secular equilibrium. As we shall see, this is the reason usually found in the literature to dismiss the relevance of this concept. But there is a second problem with the secular equilibrium that seems to have been little noticed so far: even granting stability, the data that determine this equilibrium are not sufficiently persistent relative to the adjustment process.

More importantly, also the modern versions of the marginalist approach in their several forms, that are argued to be the most rigorous formalizations of the theory, include impersistent variables among their data: the physical endowments of capital goods -and expectation functions in ‘temporary’ general equilibria. Therefore, these equilibria are unable to have a correspondence with observation because the lack of persistence of these data obliges the theory to assume *instantaneous* equilibration, making stability results irrelevant to understanding how actual economies work, since they cannot prove nor disprove that real economies behave as the theory presupposes. I will then proceed to argue that, in the attempt to overcome this insurmountable difficulty the theory was forced to abandon the traditional role assigned to equilibria, i.e.

¹ That the notion of capital as a single scalar in value terms is dominant among neoclassical scholars up, at least, to the 1930’s, has been carefully shown by, e.g. Garegnani (1960, 1990a) and then by Petri (2004). In this first chapter we attempt to give further confirmation to these scholars’ claims by reviewing in some detail Schumpeter (1934), Ramsey (1928) and Knight (1930, 1931).

a position that the economy tends to realize over sufficient time: equilibrium is now conceived as a Nash Equilibrium applied to the market phenomenon, i.e., a solution concept that limits itself to represent the mutual compatibility of plans of the agents that trade in markets, but *does not* need address the issue of how this position may actually come about in real economies. That is why I call this equilibrium notion a Market Nash Equilibrium (MNE). In other words, first in this chapter and then in the last one, it is argued that an *expression* of the inability of modern theory to determine a centre of gravitation of actual variables is the attempt by the most conscious contemporary general equilibrium theoreticians (in particular, F. Hahn) to *change* the original object of pure theory: they have *disentangled* the former link between the notion of equilibrium and the tendency towards it as two independent issues that bear no necessary relation to each other. In this connection, in this first chapter a little noticed point is shown: the founders of the marginalist approach (e.g. Walras, Wicksell and Marshall) would have found this attempt at ‘disentangling’ as illegitimate and liable to objection as for them, equilibria and centres of gravity of actual, disequilibrium, variables were one and the same thing.

Now, the reasons behind the change in the object of neoclassical theory were first addressed by Garegnani in his seminal contribution “*On a change in the notion of equilibrium in recent work on value and distribution*” (1976), and further developed in Garegnani (1999a; 2000, 2005, 2010a). The issue has been also taken up by several scholars, e.g. Ciccone (1999), De Vivo (1976), Eatwell (1982, 1983, 2012), Eatwell and Milgate (1999), Fratini (2002, 2007), Fratini and Levrero (2011), Gehrke (2003), Kurz (1985, 1987), Kurz and Salvadori (1995, 2003), Milgate (1979, 1983, 1989, 2008), Mongiovi (1999), Ravagnani (1989, 2000, 2010), Serrano (2011), Schefold (1985, 1997, 2000, 2005), Trabucchi (2006, 2011, 2012) and specially Petri (1978, 1989, 1991, 1997, 2003, 2004, 2006, 2007, 2008, 2009, 2011a, 2011b). Although not entirely homogenous, this group of scholars has followed and developed the original argument of Garegnani by identifying in the treatment of capital the main deficiencies of neoclassical equilibrium theory. In view of the foregoing considerations, it should be sufficiently clear that the present contribution will closely follow the original argument by Garegnani.

Yet, I believe that there are some issues in need of clarification and further discussion. In particular, it seems not to be sufficiently clear yet why and how this new and abstract notion of equilibrium which, as said above, is unable to accommodate time-consuming adjustments *at all*, was initially accepted by the profession, and eventually became dominant among neoclassically oriented scholars during the 1960s and 1970s. It seems hardly likely that the notion of equilibrium as a MNE would have become dominant among marginalist authors had they been fully aware of its evident shortcomings. It is much more likely that the change in the data of the equilibrium was not accompanied by a clear understanding of its implications both at theoretical and methodological levels. It is thus possible to conjecture that the traditional conception of equilibrium as a position that the economy tends to realize over sufficient time still prevailed in the profession even during the years when the formal change in the specification of the capital endowment in the general equilibrium equations started taking place, i.e. the 1930's and 1940's, and despite the new data being in fact *incompatible* with that traditional conception of equilibrium.

In other words, as it is suggested in Petri (2004: 156-165), there appears to be some ground for believing that, even after the general equilibrium equations are formalized by including the set of physical capital goods among the data, two notions of capital –and hence two different notions of equilibrium– somehow managed to *coexist*^[2]: I will bring support to the view that, while on the one hand the bulk of the forerunners of neo-Walrasian scholars appear to be aware that the notion of MNE entails with it certainly unrealistic assumptions, e.g. instantaneous equilibration, on the other hand these scholars believe that this and other ‘methodological’ shortcomings entailed by this new concept of equilibrium are of secondary importance, because the trend followed by the sequence of MNE prices and quantities is believed to describe, with sufficient approximation, those long-run tendencies envisaged by the founders of the neoclassical approach, but without the need to rely on the traditional conception of capital as a *given* amount of value. Therefore, these authors do not seem to suspect that, given that those traditional neoclassical gravitational ways of

² Actually, as it is argued immediately below, these two notions *still* coexist in the works of many contemporary general equilibrium theorists.

reasoning need, as a matter of logic, the notion of capital as a single factor of production, the plausibility of those reasonings and conclusions would be seriously called into question once this notion of capital is eliminated from pure theory. In fact, I also argue that, given that these scholars still envisage the same traditional neoclassical tendencies –in particular, the supply-and-demand explanation of distribution, and the tendency of the economy to follow a full-employment growth path- then, while on the one hand they attempt to do without the notion of ‘capital’ as a *stock*, on the other hand the traditional concept of capital as a single scalar will re-emerge as a *flow* in the savings-investment market^[3].

In the second part of the dissertation the contributions by Hayek (1941) (chapter 2), Lindahl (1939) (chapter 3) and Hicks (1939) (chapter 4), but also the less well-known, but not-less influential, work by Oscar Lange (1936, 1944) (chapter 5), will be examined under this interpretative key. Regarding Hayek and Lindahl, I will show that both authors argue that the sequence of temporary equilibria eventually converges to a secular equilibrium state. In their framework the stationary equilibrium provides the *direction of change* of temporary equilibria, which considered by itself is otherwise indeterminate; and that convergence is argued to rely on the traditional factor substitution mechanisms in terms of labour and capital as a single quantity, which in their framework operate *over* the sequence of perfect foresight temporary equilibria through the savings-investment market.

In regard to Hicks, while it is well known that in *Value and Capital* Hicks is sufficiently clear that, as a matter of logic, the notion of temporary equilibrium must presuppose instantaneous equilibration, it is not usually noticed that in his discussion of the stability of the equilibrium, Hicks reintroduces the traditional

³ Garegnani (1990a, 2000) has insisted upon this point in order to criticise the notion of Arrow-Debreu equilibrium. Some problems behind Garegnani’s argument have been addressed by Schefold (2008), Mandler (2002, 2005) and also by Petri (2009). It is not the object of this dissertation to discuss them in any detail. In any case, while Garegnani’s argument may be problematic when applied to the specific and imaginary Arrow-Debreu world with a finite horizon and complete future markets, etc., I still believe that in a broader sense Garegnani’s point is fully correct. As argued in the main text, I will attempt to show that, in most of the works analysed in the following chapters, and which, e.g., assume neither a finite-horizon economy nor complete future markets, the traditional notion of capital re-emerges as a flow of investment.

notion of “normal price”, i.e. the price of commodities that individuals may reasonably expect to find in markets, and that can only emerge after a sufficient interval of time as a result of the time-consuming interactions of the agents that trade in markets. Moreover, in his discussion of stability Hicks is forced to accept that the assumption of instantaneous equilibration is “clearly unrealistic”, and resumes the traditional idea (present in his early writings) that the forces of supply and demand are able to assert themselves rather slowly. In particular, agreeing with traditional neoclassical scholars, we will see that Hicks ends up admitting that a position of full-employment equilibrium due to a decrease in wages can plausibly emerge only after a sufficient interval of time, i.e. over the *sequence* of periods of the economy’s life-span. And in order to justify why full-employment is a position that the economy tends to realize over sufficient time anyway, Hicks relies on a process of substitution over time that presumes that as the interest rate decreases, firms are induced to increase their demand for present inputs at the expense of their future demand, and to decrease their supply of current outputs at the expense of their supply of future output. The result is that the *average-time lag* between inputs and output, measured by a sort of Average period of production, increases.

As to Lange, I will argue that under the influence of Hicks (1939) this author moves from an essentially Wicksellian position during his early writings (Lange, 1936a, 1936b) to a temporary equilibrium framework in *Price Flexibility and Employment* (1944), but with a much less clear understanding than Hicks of the implications of this change, both at methodological and at theoretical levels. At a methodological level, along that work Lange still conceives of adjustments mechanisms that take long periods of time to assert themselves, and that are incompatible with the data of a temporary equilibrium. At a theoretical level I will show that in some relevant parts of his work, e.g. in his analysis of accumulation, or in his examination of the determinants of savings, the traditional notion of capital as a single factor will re-emerge in a very clear form.

The third part of this dissertation (chapter 6) is devoted to the contributions on equilibrium and capital of one scholar only: Maurice Allais. And the reason to isolate Allais’s 1940’s work from the rest of the contributions here discussed

involves two main issues. First, despite being the first scholar to have formalized a finite-horizon intertemporal equilibrium *tout court*, and despite his influence on famous neo-Walrasian theoreticians, e.g. on Debreu and Malinvaud, Allais's contributions on equilibrium and capital seem to have escaped the deserved attention of the literature. Second and more importantly, Allais's theoretical work is rather unique because he seems to follow the *opposite* path than the contributions by the other forerunners of the neo-Walrasian approach. In fact, while in Allais (1943) the author develops an intertemporal equilibrium, awareness of its deficiencies as a tool for explanation and prediction of real world events seemingly makes him recant of this notion in Allais (1947). However, in that latter work, the author's concerns with the value notion of capital on the one hand and, on the other hand, his belief that the neoclassical approach is the correct one anyway, make him replace the notion of intertemporal equilibrium with an alternative equilibrium concept that, I will argue, is not better suited for the task of understanding how prices and distribution are determined in actual economies: the secular equilibrium.

In the fourth and last part of this work (chapters 7 and 8) I review the contributions of some prominent contemporary neoclassical scholars: Lucas and Malinvaud in particular (chapter 7), but also Bliss, Arrow and Debreu (chapter 8), to provide further evidence that supports the view that traditional neoclassical reasonings that presume the notion of capital as single factor *still* persist in the modern versions of general equilibrium theory. To put it in a nutshell, I attempt to prove what appears to be a little noticed point: although with differences, the founders of the neo-Walrasian approach, but also some influential cotemporary neo-Walrasian scholars (the most important exception of whom is perhaps Frank Hahn), all seem to believe that the *sequence* of neo-Walrasian equilibria describes actual paths with sufficient approximation, and so that, along this sequence, the traditional tendencies postulated by the founders of the neoclassical approach, in particular, the tendency towards the full-employment of resources ensured by the traditional mechanisms of factor substitution in terms of labour and capital (the single factor), will eventually assert themselves. And so, we shall also see that in the aim to endow the theory with explanatory-predictive value, these scholars will be forced to rely on

arguments that conflict with the pure neo-Walrasian framework: although these authors attempt to present their reasonings and results as grounded on modern general equilibrium theory, they *still* rely, in some way or another, on traditional neoclassical gravitational reasonings that, as it is explained in this first chapter, as a matter of logic *must* presuppose the value conception of capital[4].

As to Lucas, we shall see that, while on the one hand this author assumes that equilibrium is instantaneously reached as neo-Walrasian theory presupposes, it is little noticed that, on the other hand, Lucas admits that the hypothesis of rational expectations presupposes a time-consuming process of learning on the part of agents acting under the *same* underlying conditions, an assumption that, in a heterogeneous-capital-goods framework, can only be defended if these capital goods are seen as embodiments of a single factor. Lucas goes on to argue that the auctioneer is a valid simplification because, assuming stability, the persistence of the data ensures that, by trail and error, the forces pushing towards a neoclassical equilibrium will eventually assert themselves fully. Finally, albeit implicitly, the traditional conception of capital as a single factor is behind Lucas's recourse to one-good models to explain the

⁴ The mistaken belief that the traditional neoclassical tendencies of the economy would still hold, but now along the sequence of neo-Walrasian equilibrium prices and quantities, is the reason that has convinced me that the original taxonomy suggested by Garegnani (1976), and followed by Petri (2004), namely, that the change in the notion of equilibrium consisted of a shift from a 'long period' to a 'short' or 'very-short-period' method of analysis, is misleading, and has contributed to obscure the severity of the issue which, as Garegnani and Petri have both incessantly attempted to clarify since the late 1970's, involves a major *qualitative* change in the object of the theory. In line with what we have said in the main text, by sticking to Garegnani's classification one could end up with the wrong impression that the shift in the object of study in neoclassical economic theory that starts taking place since the 1930's has only been a matter of *degree*, namely, that by replacing the single-value scalar endowment of capital with the datum relative to the physical endowments of capital goods, the theory has suffered from a relatively minor modification in its structure; the reasoning that one would be implicitly accepting would be that although some assumptions like instantaneous equilibration would be hard to digest, they would be in any case of secondary importance given that the sequence of neo-Walrasian equilibria would still be susceptible of having the role of a centre of gravitation of actual prices and quantities. Therefore, I believe that the issues involved owing to the major *qualitative* change in the object of study of neoclassical theory, and which imply that the theory has shifted from determining a centre of gravitation of actual magnitudes to determining an equilibrium that is unable to accommodate adjustments at all, are in my view more correctly captured by the notion of MNE than by the concept of 'short-period' equilibrium used by Garegnani. Of course, my concerns would be of secondary importance if it were not the case that, as suggested in the main text, the traditional neoclassical or "long-period" conception of equilibrium as the asymptotic outcome of intertemporal equilibrium or "short-period" prices and quantities *does find manifestations* in neoclassical literature since the 1930's.

trends in income distribution in terms of well-behaved factor substitution mechanisms.

As to Malinvaud, we shall see that this author is more cautious and openly accepts that adjustments are not instantaneous as neo-Walrasian theory assumes; however, he argues that over long periods of time, the assumption that the economy follows a full-employment growth path is an acceptable simplification because a decrease in the wage rate will eventually induce firms to adopt labour intensive techniques, and hence, although sluggish in their action, the supply-and-demand forces are always at work. The notion of capital as a single factor emerges again.

Finally, chapter 8 examines Hahn's position on intertemporal equilibrium. His position is worth reviewing because, like Allais's, it appears to be rather unique too: this author is very clear that the theory of intertemporal equilibrium does not have –and cannot have- explanatory-predictive value, and is harshly critical to those scholars who believe otherwise; in spite of this Hahn defends the notion of intertemporal equilibrium with reasons that I will critically discuss and will find insufficient; in particular, he defends the 'disentangling' of equilibrium theory from what he calls the 'theory of adjustments' on the grounds that equilibrium theory would limit itself to answer to an "abstract" question that dates back to Adam Smith: "under which conditions the optimal plans of the individuals that trade in markets can be mutually compatible?". Hahn will attempt to further defend this argument by claiming that *it was always understood* by the most prominent general equilibrium theoreticians of the second half of the 20th century that the theory of intertemporal equilibrium theory is not a description of reality. Finally, Hahn argues that abstract as it is, intertemporal equilibrium theory can be useful as a "reference point". I will counter that these claims by Hahn rely on some important misunderstandings: first, that classical and early neoclassical authors did never attempt to solve this purely "abstract" problem. Second, by relying on some relevant passages by Arrow and Debreu, I will show that, Hahn's claims to the contrary notwithstanding, even for the most famous general equilibrium theoreticians it was *not* always understood that the theory of intertemporal equilibrium is not a

description of reality. As to the identification of equilibrium with a benchmark, I will show that the weakness of this line of defence is eventually admitted by Hahn himself, who accepts that this role of intertemporal equilibrium is *not sufficient* justification for it.

The main conclusion that will come out of my discussion of Hahn is that this author does not seem to fully understand the destructive scope of his own critiques: while Hahn (1991) eventually accepts that any sensible notion of equilibrium must “*include* the notion of reachability”, i.e. must “be stable under some acceptable process”, where “acceptable” implies getting rid of the auctioneer, etc., on the other hand he does not seem to realize what this means, namely, he does not seem to understand that equilibria that are reached by “acceptable processes” *require* the endowments of capital goods to be determined by the conditions of equilibrium; the bitter implication for the neoclassical approach is that an *essential* and *unavoidable* link emerges, in that approach, between the notion of equilibrium as a centre of gravitation and the value specification of the capital endowment. Given that the notion of capital in value terms is actually indefensible on theoretical grounds, this link shows that an equilibrium that is reached under “acceptable” adjustments must be determined by forces that are different from the forces postulated by neoclassical theory.

The central message one may draw from this dissertation can be summarized as follows: a consistent abandonment of capital the value factor and subsequent turn to the MNE makes it impossible to attribute to general equilibrium an explanatory-predictive role, but consciousness of this fact has been very slow to spread among non-anti-neoclassical economists, emerging clearly really only in Allais and Hahn, all the other ones illegitimately adopting gravitational reasonings that their declared micro foundation, the MNE, did not authorize -as fully confirmed by the results of the Cambridge controversies.

In view of this, the main conclusion of this work may be briefly summarized in the following way: the blind alley in which neoclassical theory currently finds itself seems to be the outcome of the inability on the part of this theory to

incorporate capital goods in a satisfactory way within the supply and demand explanation of prices and distribution. Contemporary general equilibrium theory therefore seems to be condemned to face the following *unpleasant dilemma*: either the theory attempts, like F. Hahn, to be truly consistent with its own theoretical object, the notion of MNE, and therefore it must be *silent* on the issue of how actual economies work; or, alternatively, it attempts to provide some explanation of the working of actual economies and, in so doing, it must unavoidably rely on traditional gravitational ways of reasoning that *must* presume the *illegitimate* notion of capital in value terms. Either way, then, neoclassical theory runs into a blind alley, with the implication that, to predict with reasonable accuracy price, quantity and distributional trends as observed in actual economies, an explanation with different theoretical foundations is necessarily called for.

1. EQUILIBRIUM AS A CENTRE OF GRAVITATION.

Marginalist (or neoclassical) economic theory was born and erected in the attempt to isolate the persistent forces acting on market prices and therefore the sole ones to be considered to explain the average trend of relative prices and distributive variables we observe in markets. Indeed, it was found evident by the founders of the neoclassical approach that the theory cannot predict the exact magnitude of day-by-day relative prices because these variables are actually affected by *countless* sorts of events, whose behaviour cannot be reduced to any mechanical and general explanation; any attempt, therefore, to explain this day-by-day behaviour of actual prices would be doomed to failure because the contingencies and passing events that may affect relative prices of commodities in each isolated market round would condemn the theory to *indefiniteness*[⁵] [⁶]. In any case, since most of these causes are *transitory* in their effects, and

⁵ It seems worth stressing that in the case of Marshall, his “short-period” and “temporary equilibrium” prices have correspondence with observation only when they are the result of a long-period correct knowledge of the market fluctuations (*cf.* Petri, 1991, Ciccone 1999). In other words, the notion of temporary and short period equilibrium in Marshall *presupposes* the notion of “long period” equilibrium. As a matter of fact, Garegnani (1983) and Ciccone (1999) have convincingly argued that Marshall’s temporary equilibrium prices can be also interpreted as empirical market prices, in the sense that they are essentially indeterminate in their absolute level, and only determined in the direction in which they diverge from the long-period or normal price.

⁶ Suppose for example that some day it rains heavily, and because of the rain, both the quantity of buyers and sellers that regularly participate in the market of a certain commodity are

thus bound to disappear within a short interval of time without leaving any significant trace in the economy, the impossibility to account for the myriad of forces that constantly affect the value taken by observed prices was not seen as a seriously disturbing problem. In other words, not only was the determination of the exact price of a commodity at a given instant of time an impossible task to accomplish, but also a theoretically *uninteresting* one.

Therefore, the idea of an ‘equilibrium’ between opposing forces –which in a marginalist framework are the supply and demand forces (see below)- is a theoretical construction that attempts to capture the fact that, over a given interval of time, these few but dominant forces selected by the theory would eventually balance each other. And the resulting equilibrium would determine at a theoretical level -assuming these forces were *correctly* chosen- the set of relative prices and distributive variables that would likely be observed as an average in the economy over this interval of time. In other words, actually observed prices, constantly influenced by countless sorts of events, were nonetheless supposed to be continuously *gravitating* around the equilibrium determined by those more persistent forces because, unlike those other myriad but transitory phenomena, the latter are able to exert a dominant and determinate influence on the variables under examination. In short, for the founders of the neoclassical approach, equilibrium (theoretical) magnitudes act as a *centre of gravitation* of actually observed (empirical) variables. Consider for instance what Wicksell writes in his *Lectures on political economy* (1934): under free competition, he argues

[t]he relative prices of commodities will more or less rapidly approach a certain equilibrium position, or else oscillate about it. (Wicksell, 1934: 53)

And also Walras asserts:

Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping, and, before the goal is reached, it has to renew its efforts and start over again, all the basic data of the problem, e.g. the initial quantities possessed, the utilities of goods and services, the technical coefficients, the excess of income over

reduced. The effect of the rain on the price of the good that will prevail that day cannot be asserted with sufficient approximation. At most one can conjecture that the price will likely change relative to the price that prevailed the previous days. And even if one could justify the sign of the change in the price with respect to the previous periods, its absolute level would be largely indeterminate.

consumption, the working capital requirements, etc., having changed in the meantime. Viewed in this way, the market is like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it. But whereas there are days when the surface of a lake is almost smooth, there never is a day when the effective demand for products and services equals their effective supply and when the selling price of products equals the cost of the productive services used in making them. The diversion of productive services from enterprises that are losing money to profitable enterprises takes place in various ways, the most important being through credit operations, but at best these ways are slow. (Walras, 1954: 380)

The view of equilibrium as a centre of gravitation is also dominant among the most important neoclassical scholars of the first half of the 20th century. So, for instance, we find Robertson writing in his *Lectures on economic principles* (1958) that equilibrium is “a norm around which actual value oscillates” (Robertson, 1958a: 95)[7].

1.1. A first feature of the centre of gravitation: stability.

In light of the foregoing considerations, it is of paramount importance to remark that the attempt to find an ‘equilibrium’ determined by opposing forces *presupposes* that the theorist is renouncing to explain the details of the day-by-day behaviour of the variables under examination. Had it been possible to perfectly explain how actually observed market prices (that in a marginalist framework correspond to out-of-equilibrium prices) behave, the notion of equilibrium would be simply *useless*. As Parrinello notably points out (*cf.* also Piccioni, 1997: 108, fn. 1), should the stage be achieved where the actual trend followed by out-of equilibrium prices could be perfectly described by the theorist, then

[w]e would resort directly to the ‘perfect’ disequilibrium model and the method of approximation based on attractors [centres of gravitation] should be dismissed as a non necessary approximation. (Parrinello, 1990: 114)

This point has been also remarked by neoclassical scholar Frank Hahn, whose contributions on equilibrium and capital will be examined in the last chapter of this dissertation. Hahn writes:

[i]f a definite behaviour pattern can be established for all situations then nothing is gained by labelling any particular behaviour as equilibrium behaviour. (Hahn, 1973a: 21)

⁷ As I argue in section 1.2.1, for the prices of produced goods the classics have the same gravitational idea.

This limitation that equilibrium theorizing *necessarily* imposes on the studying of economic phenomena, carries with it important restrictions on the kind of equilibrium that must be determined by theory. The first restriction is that, unless one is willing to renounce *ab ovo* the aim of reaching a correspondence between theory and observation, given that there is no guarantee that the neoclassical equilibrium theory correctly indicates the dominant forces and mechanisms (other forces might be proposed, and *are* proposed in non-neoclassical approaches such as e.g. the surplus approach), a necessary condition for the plausibility of these forces and mechanisms as determinants of the trend of observed prices is that there be a tendency toward the theoretically determined equilibrium, i.e. the equilibrium must be *stable*^[8]; lacking this stability, the lack of plausibility of the theory would also raise doubts on the existence of those forces, and therefore also on the choice of the data of the theory. In this sense, when analysing the relationship between observable and theoretical magnitudes, we find Garegnani arguing:

⁸ It might be worth noting that, essentially up to Samuelson's contributions to stability in the 1940's (*cf. Foundations of Economic analysis* (1947) for a summary of these contributions), neoclassical authors attempted to explain the tendency towards equilibrium in terms of static stability, i.e. by analysing the *direction of change* of the adjustment process (the sign of excess demands) but without relying on an explicit formalization of disequilibrium states, i.e. without explicitly modelling the *magnitude* (speed and lags) of the adjustments. The reason appears to involve two different issues. First, they arguably lacked the necessary mathematical tools for the task. But second and more importantly, they seem to be aware that i) out-of-equilibrium behaviour is extremely difficult to model in general terms and hence that ii) the results obtained may largely depend on the specific assumptions made by the theorist. As Vianello (1989: 98) explains, at a general level the only thing that the theory can predict with respect to market prices (out-of-equilibrium prices in neoclassical theory) concern "a) the direction in which they diverge from the natural prices and b) the tendency of this divergence to be eliminated". The Samuelsonian analysis added to this a specification of lags of speeds of reaction, already considered in fact in some analysis, e.g. the famous 'cobweb' hog cycle. This analysis admitted instabilities arising in spite of the 'correct' shape of supply and demand curves. But traditional authors could plausibly argue that as long as those selected forces were sufficiently persistent in their action and the direction of the adjustment was the correct one, the possible instabilities that could arise due to excessive speeds of reactions of the variables under examination caused, for instance, by mistake expectations, would be eventually corrected and eliminated (*cf. Garegnani, 1997: 159-160; Petri, 2010: 13, fn 17; 19-21; Serrano 2011*). So, e.g. we find Metzler (1945) justifying the importance of static-stability analysis on the grounds that "The conditions which govern price responsiveness are much more obscure than are the static supply and demand conditions in individual markets. To a large extent, speeds of adjustment are determined by institutional factors such as the willingness or ability of buyers and sellers to hold or to reduce inventories. Static conditions of supply and demand, on the other hand, depend largely upon maximum-profits conditions of producers and consumers. For this reason, economists are usually more confident of their knowledge of supply and demand conditions than of their knowledge of such dynamic factors as speeds of adjustment. If possible, it is therefore desirable to describe market systems in terms which are independent of speeds of adjustment." (Metzler, 1945: 204). In short, the conditions of static stability have a higher hierarchy than the conditions of dynamic stability, and that is why in this work I will consider only the static conditions of stability, e.g. in the savings-investment market.

[i]f what is asserted in the theory about those [theoretical] variables is to be valid, there must ultimately exist some forces that bring the actual magnitudes towards the levels determined in the theory, with which the former magnitudes can in fact never coincide exactly. (Garegnani 1990c: 347)⁹

We must postpone until section 3 a more thorough assessment as to why the lack of stability may seriously question that the selected dominant forces as determinants of the trend of observed prices are the correct ones; the reason is that we must explain first the mechanisms asserted by the marginalist theory to argue for the tendency towards equilibrium (section 1.1.1.), and the role played by the factor capital in value terms in this explanation (section 2).

The fact that equilibrium must be stable in order to be a suitable theoretical construction is, as we shall specially see in the last chapter of this dissertation, currently disputed by the most conscious neoclassical theoreticians as Hahn. However a very important thing has not been often noticed: the necessary relation between equilibrium and equilibration seems to have been largely taken for granted by the founders of the neoclassical approach, e.g. by Wicksell and Walras. In their view, unstable positions of rest do not even have the theoretical status of ‘equilibria’. For instance, in *Value, capital and rent* (1954), Wicksell defines equilibrium as a position where supply is equal to demand. However, immediately after Wicksell warns the reader that

However, a more detailed examination shows that equality of supply and demand is indeed necessary, but at least from the theoretical point of view, not a sufficient condition for equilibrium on the market, supposing the latter to be stable—if, that is to say, the proportion of exchange would automatically return to (approximately) the same position after an accidental shifting. (Wicksell, 1954: 85)

In other words, in order to be called an equilibrium position, this position must be stable. That is why Wicksell proceeds to notice that in those unstable positions where supply matches demand, “no *real* equilibrium of the price exists, but only a *temporary equality* of supply and demand” (Wicksell, 1954:

⁹ Also Duménil and Lévy (1985: 341) have stressed the necessary relationship between equilibrium and equilibration by writing that “dynamics and equilibria are related like mothers and sons”. They argue however that “In the history of economic thought, this connection is to a large extent overlooked. Equilibrium is the main concern, and dynamics is viewed as an inessential development of the theory” (*id.*:340). The authors seem to ignore or forget how comparatively recent this apparent ‘overlooking’ in fact is. As discussed below, traditional neoclassical scholars were well aware of the necessary relationship that emerges between equilibrium and the adjustment processes.

87, emphasis added). The temporary feature of unstable positions where supply and demand are equal is evident if we recall that all sort of transitory and accidental events are constantly affecting the actual behaviour of prices in the economy. Moreover, in his *Lectures*, Wicksell (1934: 59) describes those unstable positions as “*curious*” positions such that “the equality of supply and demand is merely *accidental*” (*id.*).

Although Walras’s appraisal of the problem of equilibrium is similar to Wicksell’s, it is in any case worth exploring it, given the importance of this scholar for the developments of the concept of supply-and-demand equilibrium that start taking place in the 1930’s with the works of Hayek (1941), Lindahl (1939) and Hicks (1939). In his *Elements of pure economics* (1954), Walras distinguishes between two methods to determine equilibrium prices: the “theoretical solution” and the “practical solution”. While the former consists of the solution of a system of equations, the latter aims at representing the main features of the adjustment process of out-of-equilibrium variables toward their equilibrium position that Walras argues to be constantly taking place under free competition^[10]. Walras then remarks that the mathematical and practical solutions must coincide, this is the “essential point” (*id.*: 162) that economic theory must prove^[11]. As a matter of fact, he widely acknowledges that the computation of the theoretical solution “would be absolutely impracticable in almost every case” (*id.*: 106). However, he proceeds to argue that the theorist can actually skip the computation of the equilibrium values insofar as the practical solution suffices to understand the general characteristics of the situation the economy tends to realize. The foregoing considerations could be summarized by saying that, in Walras’ system, the concept of equilibrium must explicitly encompass the idea of a tendency towards it. This fact is recognised explicitly, in chapter 7 of the *Elements*, when Walras remarks that “only [the stable positions] give solutions of the problem [of equilibrium], for the system designated by [the unstable positions] merely marks the boundary separating

¹⁰ “We are now in position to see clearly what the mechanism of competition is. It is the practical solution, reached through a rise or fall in prices, of the same problem of exchange to which we have just given a theoretical and mathematical solution.” (Walras, 1954: 106)

¹¹ “There remains only to show -and this is the essential point- that the problem of exchange for which we have just given a theoretical solution is the selfsame problem that is solved empirically on the market by the mechanism of free competition” (Walras, 1954: 162-163).

each of the respective solutions” (*id.*: 112). Evidently, for Walras, like for the other founders of the neoclassical approach^[12], equilibrium is a “position towards which things are tending” (Schlicht, 1985: 27), and hence the definition of equilibrium cannot be properly addressed without considering, at the same time, the issue of how it comes about.

The view that a necessary link must exist between equilibrium and stability seems to be the dominant view among 20th-century marginalist scholars, at least up to the first half of the 1900’s. A first example is provided by Knight (1930). Knight admits that, due to constant changes in the underlying conditions,

[a] considerable interval must elapse before equilibrium will be established. It follows that the system never really is in equilibrium... at any point. (Knight, 1930: 187).

Given, therefore, that the actual economy suffers from constant disruptions and hence is never in equilibrium, Knight concludes:

[t]he tendency toward such a state is the main feature to be made clear in a scientific description of it. (Knight, 1930: 187).

Another example of this close relationship between equilibrium and stability is provided by Harrod (1939) in his seminal contribution to the theory of economic growth. There, the author presents for the first time the notion of ‘warranted rate of growth’, which is the rate of growth such that “if it occurs, will leave all parties satisfied that they have produced neither more nor less than the right amount” (*id.*: 16). On why he decided to name this particular rate of growth the ‘warranted rate’, Harrod explains:

I use the unprofessional term warranted instead of equilibrium, or moving equilibrium, because, although every point on the path of output described is an equilibrium point in the sense that producers, if they remain on it, will be satisfied, and be induced to keep the same rate of growth in being, the equilibrium is, for reasons to be explained, a highly unstable one. (Harrod, 1939: 16)

¹² Also Marshall shares this common view: “though in the account of the oscillations of demand and supply about a position of stable equilibrium... it was tacitly implied, as is commonly done, that there could be only one position of stable equilibrium in a market: yet in fact under certain conceivable, though rare, conditions there can be two or more positions of *real equilibrium* of demand and supply, any one of which is equally consistent with the general circumstances of the market, and any one of which if once reached would be stable, until some great disturbance occurred.” (Marshall, 1961[1890]:806, emphasis added). It is implicit in this passage that by “real equilibrium” Marshall is implying that unstable positions where supply and demand are equal are not really equilibria.

So, as Harrod remarks, it is because the warranted path is an unstable growth path that Harrod refuses to call it an ‘equilibrium’ path. The same relation between equilibrium and stability emerges in the writings of some prominent marginalist scholars of the second half of the 1900’s. So, for instance, we find Kuenne (1963) summarizing the relationship between equilibrium and equilibration when he writes:

Equilibrium is not a formal property; there must be identifiable and meaningful opposition of forces tending toward net balance as reflected in a position of rest before solutions are acceptable as equilibrium solutions. (Kuenne, 1963: 18)[¹³]

1.1.1. The tendency towards equilibrium and the mechanisms of substitution.

The foregoing evidence strongly suggests that the dominant view among the founders of the marginalist approach, but also among many prominent neoclassical scholars of the 20th century, is that, in order for those prices determined by the balance of supply and demand forces to have the right to be called ‘equilibrium’ relative prices there must be a tendency towards them. This section examines very briefly the main mechanisms envisaged by marginalist authors to explain the stability of the equilibrium. But before pursuing our argument further, we must first recall that the idea that theoretical magnitudes act as a centre of gravitation of actual magnitudes is also shared by other schools of thought, e.g. the classical school of Smith, Ricardo and Marx[¹⁴]. The main difference between the classical and marginalist schools is that neoclassical authors apply the idea of gravitation to equilibrium also to factor markets, and they have different ideas as to what determines normal quantities[¹⁵]. Accordingly, the differences between the classical and

¹³ It is worth reporting Hicks’s view on this issue, given the central role played by this author in the development of marginalist equilibrium theory in the second half of the 20th century. In *Capital and Growth* (1965), Hicks stresses: “if the equilibrium assumption is to be justified... we should be able to assert the existence of a tendency towards equilibrium”. (Hicks, 1965: 17). In other words, Hicks’s conception of equilibrium is slightly different from the other scholars’: Hicks does not claim that equilibrium must be stable in order to be called an equilibrium position. However, he does openly admit that the usefulness of the notion of equilibrium as indicative of the behaviour of the economy requires that equilibrium must be stable.

¹⁴ For instance Smith writes that the “natural price”, which corresponds to the equilibrium price in the marginalist approach “is the central price around which the prices of all commodities are continuously gravitating” (Smith, 1960: 48-51, quoted in Garegnani, 1990a)

¹⁵ For an assessment of the classical theory of prices and distribution, cf. Sraffa (1960), Garegnani (1960, 1983, 1984, 1990b, 2007) and Kurz and Salvadori (1995). Cf. also Eatwell

neoclassical theories of prices and distribution is expressed in the set of data. While in their attempt to determine the rate of profits and relative prices classical economists take as data: a) the composition of the social product; b) the set of available techniques and c) the real wage, the marginalist school, on the other hand, in its more ambitious aim to explain prices and distributive variables in the same analytical stage in terms of an equilibrium between supply and demand forces, considers as data: i) the available technology; ii) the endowment and distribution among the population of each ‘productive’ factor (land, labour and capital) and iii) consumers’ preferences.

Neoclassical theory, by relying on the mechanisms of *direct* and *indirect factor substitution*, uses the set of data i)-iii) to assess the tendency towards an equilibrium on factor markets determined by supply and demand forces. The working of the direct mechanism of substitution can be briefly described by considering the subset i)-ii): the given factors of production included in set i) can be combined in different proportions according to the techniques described by set ii) to produce a final good. Then, profit maximizing firms will adopt those techniques that, for given rentals of the factors of production, minimize average costs, and hence they will tend to use in a higher proportion the factor whose relative price has decreased. The theory thus derives what Garegnani (1990) calls “employment” curves for the different factors of production, and that are negatively related with the respective factor rentals. If we introduce the additional assumption that rentals tend to vary according to excess demands, the employment curves can be interpreted as *factor demand* curves: when, say, the quantity of labour available in the economy increases, competition among workers tends to reduce the wage rate and profit-maximizing firms are induced to adopt those techniques that use labour more intensively relative to the given quantity of the other factors (assumed to be fully employed).

The working of the mechanism of *indirect* substitution can be explained in terms of the subset of data i) and iii) above. Let us now assume that two final products, corn and cloth, are produced in the economy under a fixed-

(1982), Vianello (1989), Petri (1989, 2011b), Piccioni (1989, 1998), Ravagnani (2001, 2002, 2012) Ciccone (2011) and Schefold (1997).

coefficients technology. Let us assume that corn is labour-intensive relative to cloth. A decrease in the real wage will now induce consumers to increase their consumption of corn, and hence, indirectly, it will induce also profit maximizing entrepreneurs to increase corn production. This will entail the transfer of some units of the other factors that were previously used in the production of cloth to the production of corn, but given that according to our assumptions corn is a labour-intensive commodity, in the attempt to satisfy the rise in corn-demand, entrepreneurs will increase their aggregate demand for labour. Therefore, even when the direct mechanism of substitution is absent, the indirect mechanism ensures the existence of a negative demand function of productive factors with respect to their respective factor rentals. In general however, the neoclassical argument goes, both mechanisms will be *simultaneously* at work in the economy, and they will therefore reinforce each other so as to ensure the negative elasticity of the factor demand curves.

As a result of both mechanisms of substitution, the demand for productive factors will be inversely related to their respective rental prices. Given the respective factor supplies -which as a first approximation can be assumed to be inelastic with respect to their respective rentals (e.g. the wage rate, the rent of land rate and the rate of interest) - the rentals that the theory envisages as centres of gravitation of actual variables, will thus be determined by the intersection of the respective aggregate supplies and the monotonically decreasing factor demand curves. Disregarding anti-neoclassical income effects, the equilibrium position, *slowly* reached in each factor market, will likely be *unique* and *stable* since, although the supply curve can be downward sloping in some parts, the existence of alternative methods of production gives plausibility to the fact that not only will the factor demand curve be downward sloping, but also *fairly elastic*, and hence the curves will intersect only once. As a corollary, the supply-and-demand explanation of prices and distribution also offers a plausible explanation of the tendency, under normal conditions, of resources to be fully-employed. In a nutshell, the factor substitution mechanisms, and the fairly elastic factor demand curves that are derived from them, constitute the core of the explanation of prices and distribution in terms of supply and demand forces.

1.2. A second feature of the centre of gravitation: persistence.

We have seen above that, in order to keep the problem under investigation tractable and to allow reaching determinate and sufficiently general conclusions regarding their effects, equilibrium theorising should concentrate on few but dominant forces at work. These forces, which exert a dominant and determinate influence on the variables under examination, are *persistent* in their effects, and are distinguished from those other forces whose effect on the economy is both accidental and transitory. There is, however, an additional reason to exclusively focus on those persistent forces that constantly act on relative prices: assuming equilibrium to be stable, it must be admitted that the reaching of the centre of gravitation *takes time*. And any sensible notion of equilibrium must take this feature into account when dealing with the equilibration process. In this sense, we have seen above that, when likening the market with a lake, Walras (1954: 380) openly admits that adjustments are “*at best... slow*”. And Marshall writes:

The actual value at any time, the market value as it is often called, is often more influenced by passing events, and by causes whose action is fitful and short lived, than by those which work persistently. But in long periods these fitful and irregular causes in large measure efface one another's influence so that in the long run persistent causes dominate value completely. (Marshall, 1920 [1970]: 291)

In order better to grasp the role of the dominant forces at work, we may consider the following example due to Petri (1978, 2004). Let us consider an economy with two productive factors, labour and land, rigidly supplied, and which are employed by entrepreneurs to produce consumption goods. Let us further assume that equilibrium is suddenly disturbed by labour immigration. Given that the supply of labour has suddenly increased, due to competition among workers, the wage rate will decrease so that a new position of full employment will eventually be established in the economy; this process however, will generally take considerable time. Indeed, when money wages start slowly to decrease in some sectors, the gradual adaptation of prices to their lower minimum average costs will make some firms yield extra profits, and transactions and production will generally take place at disequilibrium prices until, only after sufficient time, since e.g. new firms must be born, etc., the equilibrium determined by the new data (the same as before, but with a greater quantity of labour) is finally reached. But all along this reasoning we have implicitly assumed that during the adjustment process towards the new

equilibrium, the remaining data of the problem –the available methods of production and individuals’ preferences- *have not changed*.

So, if equilibrium is to be conceived as a position that is reached only after sufficient time, an assumption of sufficiently *given* –unchanging- data during the adjustment process appears necessary and must be justified. This essentially implies that these data must comply with two main requisites:

A) given that these data *are not* actually *invariable* and will presumably change over time (e.g. technological improvements will occur, consumer’s preferences will be modified with the emergence of new products, changes in socio-political circumstances, etc.; factor endowments, for instance labour, will change due to changes in the rate of population growth, etc.), in order to have the right to neglect these changes and assume the data as not changing during the adjustment process, the speed of change of these data must be of a lower order of magnitude than the speed of adaptation of prices and quantities towards their equilibrium levels. In “*Statics and dynamics: some queries regarding the mechanical analogy in economics*” (1930), Knight notices in this connection that:

[c]onditions undergoing slower processes of change serve for the theoretical analysis as a framework or setting of given conditions with reference to shorter-period of adjustments. These shorter-period adjustments, then have the character of ‘tending’ to establish equilibrium with reference to their respective given conditions at any given time, though with a certain lag due to the fact that the latter are also continuously changing. (Knight, 1930: 185)

And given that also the underlying given conditions are continuously changing, he adds that the

Elements which are to be treated as variable and those which are to be treated as fixed *cannot be chosen arbitrarily*. (Knight, 1930: 186, emphasis added)

And so Knight claims that *only* those variables that are undergoing slower processes of change with respect to the variables under examination can be taken as given when studying the tendency of equilibrium of those “short-period adjustments”.

B) Given that adjustments take time, production and transaction decisions will generally take place at out-of-equilibrium prices. Therefore, the data of the determination of equilibrium must not be significantly altered by the accidents of disequilibrium, otherwise equilibrium would exhibit *path dependence*, and a serious element of indeterminacy would be introduced into the theory. In this sense, Kaldor (1934: 127) argued that unless one adopts Walras's "slightly ridiculous" assumption of essentially instantaneous equilibration, then "equilibrium would be determinate... even if it is only gradually established [if the data] in any one period of time is not affected by [the] actions in the previous period" (*id.*: 127). Then, Kaldor continued:

[a]t the beginning of every-period [the individual] is confronted with the same initial situation; his only inheritance from the past is his gradually accumulated experience. We can assume that he has no experience at the beginning; his initial actions will then be accidental or irrational; but the gradual accumulation of experience will lead him, through a process of relative valuation, to a gradual change of his daily dispositions until he reaches a situation where no further accumulation of experience will induce him to change his dispositions any further –so long as the initial data ('the independent variables') remain unchanged. (Kaldor, 1934: 127)

Kaldor's message is that, if the data of the equilibrium remains unchanged for a sufficient interval of time, then, while in the first periods behaviour in production and exchange will be in all likelihood "irrational" due to the possible accidents of disequilibrium, a position of rest will be nonetheless attained (assuming stability) after a certain number of periods because through repeated interactions individuals will "gradually accumulate" the necessary *experience* over the ruling conditions of the market. It is then taken for granted in Knight's and Kaldor's arguments that equilibrium can only describe the trends of prices and quantities over sufficient time.

Hicks correctly summarizes the need of persistent forces by noting that "if the assumption [of equilibrium] is to be usable", there must be "a *strong tendency* [towards equilibrium]" (1965: 17). And then, he writes:

Even if the equilibrium exists, and the tendency to equilibrium exists, we may still have insufficient ground to justify the equilibrium assumption if the convergence to equilibrium is very slow. For then, in any period of reasonable length that begins from a position which is out of equilibrium, the time that is occupied in approaching equilibrium (and still remaining, perhaps, quite far away from it) will be long in proportion to the

length of the later phase, in which an equilibrium position is approximately realized. (Hicks, 1965: 19)^[16]

Even if the selected forces might give rise to stability if studied in isolation, their action might not be dominant when other forces are considered. Therefore, the lack of persistence of the selected forces would cast serious doubts on the stability results, and hence on the plausibility of the theory as an explanation of the trend of prices and quantities. Finally, it must be noted that although the requisite of persistent forces imposes quite stringent restrictions on the possibility to select the ‘given’ variables, it nonetheless gives flexibility to the system, in the sense that the latter becomes perfectly compatible with the equilibrium positions themselves changing over time. And that is why some prominent marginalist scholars, e.g., Knight (1930: 187), Robbins (1930: 203, fn. 3) and Frisch (1936: 103), usually refer to the equilibrium position determined by persistent although changing forces as a ‘moving equilibrium’.

Before concluding this section it must be noticed at this juncture that it is precisely the persistence of the data (i.e. not strict constancy, but a change at a much lower order of speed than the speed of change of the endogenous variables) that avoids the need to refer to those changes that equilibrium prices will experience over time in the definition of the equilibrium position itself. At a first level of abstraction, those changes can be taken as once and for all changes, and their effects on the economy addressed by comparative statics exercises. When necessary, the analysis of the ‘dynamic’ path followed by equilibrium prices over time could be thus examined at a second stage of analysis, and under more particular assumptions depending on the problem at hand^[17].

¹⁶ Or As Schlicht (1985: 19) remarks, the choice of the data cannot be “a matter of caprice” if this choice “is intended to be of substantive relevance” since “those factors transformed into data by means of the *ceteris paribus* clause have to be sufficiently stable with regard to the processes we want to explain such that the movements of the data do not destroy or supersede the relations we are studying in our model.”

¹⁷ The possible difficulties that may emerge in the attempt to accurately describe the dynamic path followed by the economy are addressed by Sraffa in one of his unpublished documents. He notes that “It is a fatal mistake of some economists that they believe that by introducing complicated dynamic assumptions, they get nearer to the true reality; in fact they get further removed for two reasons: a) that the system is much more statical than we believe, and its short periods are very long, b) that the assumptions being too complicated it becomes impossible for the mind to grasp and dominate them - and thus it fails to realize the absurdity of the conclusions.” (Sraffa: D3/12/11 (33), quoted in Kurz and Salvadori, 2002: 385)

1.2.1. Cost-of-production relative prices as equilibrium relative prices.

We have seen in section 1.1.1 above that classical and marginalist authors differed in the variables taken as given in their respective explanations of prices and of the rate of return on capital. As a result, while for the latter the distribution of income is explained in terms of supply-and-demand functions, the former envisage the distribution of the social product among the different classes as being determined by historical, cultural and political factors, whose interaction is not susceptible of any mechanical and general representation. However, it must be remarked that, once distribution is determined and the cost-minimizing methods have been accordingly chosen, those prices which must act as centre of gravitation of empirical values coincide in both theoretical paradigms, since the admitted tendency of supply to adapt to its forthcoming demand as an expression of the *competition* among entrepreneurs in the search of the highest possible return on investment implies that, over sufficient time, market prices will tend to coincide with their minimum average costs^[18]. These prices are called ‘natural prices’ or ‘prices of production’ by the classics and ‘normal’ or ‘long-period prices’ by neoclassical authors.

It must be also recalled that the same tendency of production to adapt to forthcoming demand must be at work in industries that produce capital goods, and therefore, by varying the existing quantities of capital goods –that, therefore, like the quantities produced of consumption goods, must be *endogenous* variables, *i.e.* to be determined by the conditions of equilibrium– also capital goods’ prices must coincide with their minimum average costs in equilibrium. In turn, the search for the highest possible returns on investment ensures that the rate of return on the supply prices of capital goods will be uniform in equilibrium. We can e.g. explain this process by assuming that consumers save by purchasing capital goods that are then hired by firms. So, given that generally capital goods do not give any direct utility to consumers,

¹⁸ As an example, we find Walras arguing that “It never happens in the real world that the selling price of any given product is absolutely equal to the cost of the productive services that enter into that product, or that the effective demand and supply of services or products are absolutely equal. Yet equilibrium is the normal state, in the sense that it is the state towards which things spontaneously tend under a régime of free competition in exchange and in production” (Walras, 1954: 224–5).

they will tend to buy those capital goods whose return is the highest, raising the demand for them. This process will raise the prices of the capital goods and this will prompt the firms that produce them to increase their supply. But when capital goods' endowments increase, their rentals will go down. The contrary is the case for those capital goods yielding the lowest returns. This process will continue until the rate of return on the several capital goods is equalized, and this rate must allow firms to yield zero-extra profits, i.e. to exactly cover their cost of production (neglecting risk for simplicity). Therefore, the relevant condition for relative prices to be at rest over sufficient time –i.e. before the persistent data that determine them change- is that these prices must yield a uniform rate of return on the supply prices of the existing capital goods (henceforth, URRSP), and this entails, as a matter of necessity, an endogenous determination of the *composition* of the capital stock. The role of the URRSP condition as the key equilibrium condition was widely admitted by both classical and neoclassical scholars. Even Walras thus writes:

Capital goods proper are artificial capital goods; they are products and their prices are subject to the law of cost of production. If their selling price is greater than their cost of production, the quantity produced will increase and their selling price will fall; if their selling price is lower than their cost of production the quantity produced will diminish and their selling price will rise. In equilibrium their selling price and their cost of production are equal. (Walras, 1954: 271)

The theoretical interest of prices that yield the URRSP is easily understood in view of our discussion of persistent forces of the previous section. In fact, these relative prices are sufficiently persistent so as to emerge as the average of market prices over sufficient time. Moreover, the remarkable persistence of these relative prices, and thus of the data that are supposed to determine them, also allowed neoclassical authors to neglect, as a first approximation, their changes over time owing to gradual changes in those data, e.g. slow changes in the quantities of productive factors due to accumulation or population growth. On the same footing, they could safely disregard the effect that disequilibrium transactions and productions could possibly have on these data, and hence could conceive these relative prices as representing the average of market prices over a sufficient interval of time.

2. NORMAL PRICES IN NEOCLASSICAL THEORY AND THE ENDOWMENT OF CAPITAL.

We have seen in the previous section that the determination of ‘normal’ or ‘long-period’ prices requires an endogenous determination of the capital stock. Now, within marginalist theory, the endogenously determined composition of capital goods needs a given, single-valued scalar endowment for the factor ‘capital’ to be part of the data (*cf.* Garegnani 1960, 1990 and Petri 1978, 2004^[19] for formalizations. I will provide an example of such formalizations in my assessment of Lange (chapter 5)). The analytical and methodological reasons for this requirement are discussed below. It is worth stressing first that even those scholars who, like Wicksell (1934), attempted to present a fully-disaggregated specification of goods produced and of production functions in a general equilibrium system, were anyway forced to specify the capital endowment of the economy in value terms. In other words, in traditional theory the several endowments of capital goods are *not replaced* by the single quantity of value capital, but they *coexist* with the latter. The point is worth stressing because there still seems to be some misunderstanding on this respect: some prominent contemporary scholars still believe that a single quantity of capital means that one is adopting a one good-model, aggregate-production function model^[20].

2.1. Supply side roles of capital.

We will follow the distinction introduced by Garegnani (1990a: 36) and consider first the *supply side roles* of the capital endowment in value terms,

¹⁹ No traditional author actually modelled a position of general equilibrium as Petri (1978; 2004) does.

²⁰ As an example of these kinds of misunderstandings, consider for instance what Hahn (1982) writes: “The Sraffian picture of neoclassical theory is this. At any moment of time we can observe something physical called the stock of capital (K) as well as the amount of labour (L). There is a concave production function $Y = F(K, L)$ where Y is output. In a neoclassical equilibrium all inputs are used and must be paid their marginal products. The latter are known once (K, L) are known. Hence the rate of profit of capital, the real wage and the distribution of income are all known once $F(\cdot)$, K and L are known. The concavity of F further implies that the rate of return on capital is non-increasing (generally decreasing) in K . This construction, to be called the parable, Sraffians claim to be not logically watertight except in the single-good economy” (Hahn, 1982: 370). Of course this is not the picture of neoclassical theory of the “Sraffians”, who know very well that even Clark was aware that capital is heterogeneous and aggregated in value terms. But the same misunderstanding arises in authors that are critical to neoclassical theory. Referring to Sraffa’s work, Hodgson (1997: 88) for instance writes: “*Production of Commodities by Means of Commodities* constituted a devastating logical attack on the aggregate production function of neoclassical theory and theories of supply, distribution and growth based upon it... In such terms, Sraffa’s work was a highly destructive *internal* critique of the type of neoclassical theory which made extensive use of aggregate production functions”.

which involve three issues. First, this specification allows neoclassical theory to *endogenously* determine the equilibrium composition of the capital goods to satisfy the URRSP equilibrium condition, while at the same time makes it possible to treat the endowment of the factor capital as a ‘given’ magnitude. This endowment of capital represents *past* or *accumulated* savings that are, so to speak, *incorporated* in the several endowments of capital goods. The logic of the approach requires that capital, being heterogeneous, must therefore be measured in the same units as are savings, some composite unit of consumption that individuals ‘refrain’ from consuming when they save. The underlying idea is that the services that could be used in the production of consumption goods and are instead saved and devoted to the production of capital goods, have the same value of those consumption goods that are not produced.

On why the endowment of capital must be necessarily measured as an amount of value, the following argument due to Petri (2004: 91) is particularly instructive: when the rental earned by a factor of production is uniform, the rewards to different portions of the same factor is proportional to the amount of factor they contain: e.g. if we consider two fields of land of the same quality, α and β , and the reward to α is twice as much as the reward of β it must be because α ’s surface is twice as big as β ’s. If now α and β are capital goods, with α earning a rental twice as big as β , and their rentals are seen as the productive contribution of the factor ‘capital’ that is embodied in these capital goods, then α must contain twice as much ‘capital’ as β . But the value of the capital goods is obtained by capitalizing their rentals at the market interest rate, so the value α will double the value of β . It follows that the relative values of the different capital goods *must* be a measure of the relative amounts of ‘capital’ embodied in them. If one measures ‘capital’ in units such that a capital good of unitary value embodies one unit of ‘capital’, then the value of capital goods will also measure the endowment of ‘capital’. It follows that only the value of capital will do as a measurement of the endowment of ‘capital’, because no technical aggregable measure of capital goods –e.g. their weight– would satisfy the proportionality with their value.

The quantities and forms of the several capital goods that are thus to be used and reproduced in equilibrium are accordingly seen by the founders of the neoclassical school as ‘crystallizations’ of this given endowment which, being measured as an amount of value, is assumed to be capable of changing form without changing in quantity (*cf.* Garegnani, 1978). Consider for instance what Clark writes:

Where there is a capital of five hundred dollars for each worker, that fund is in one set of forms; and where there is a capital of a thousand dollars per man, it is in a different set. That the relative *amounts* of labor and capital should change, means that the *forms* of both should change (Clark, 1925: 159-160, quoted in Petri, 2004: 29)

And also Wicksell:

At first sight all these requisites [the different capital goods] have only one quality in common, namely that they represent certain quantities of exchange value, so that collectively they may be regarded as a single sum of value, a certain amount of the medium of exchange, money. (Wicksell, 1934: 145)

And finally Knight:

[f]rom the standpoint of the general theory of capital we must maintain the familiar view, advocated in abstract form especially by J. B. Clark, that the capital of an economic society or system is a continuous organic whole, a fund measured in value units, though at any moment it is largely (not entirely) embodied in things of a sort which more or less regularly wear out, are used up or become obsolete, and are replaced by other items of the same or different description. (Knight, 1936: 460)^[21]

Second, although it is obvious for neoclassical scholars that the ‘given’ quantity of capital would change with capital accumulation due to individuals’ decisions to make positive net savings, they convincingly argued that this process is slow enough relative to the adjustment of market prices towards equilibrium so as to make it *legitimate* to neglect the issues related to capital accumulation when studying the main causes of prices and distribution. That is to say, given the persistence of the capital endowment in value terms, in order to determine long-period prices the bulk of marginalist authors legitimately assumed net savings to be zero whatever prices and distribution, i.e. they assumed the economy to be stationary^[22]. The equilibrium thus determined is a

²¹ Stigler (1952: 275-276) argues on these same grounds when he says that capital can be viewed as “an accumulated fund of general productive power: as past income incorporated in particular physical forms, or as particular forms which will yield income in the future”.

²² Although this assumption is not strictly necessary (*cf.* Petri, 2004: chapter 4 for a formalization of a long-period equilibrium along neoclassical lines, but with positive net savings)

static stationary equilibrium, and is to be distinguished from a *secular* equilibrium where also the quantity of capital is determined endogenously as one of the conditions of equilibrium. For instance, Knight explains that “[i]t is possible, legitimate, and necessary to consider the character and meaning of ‘static’ demand and supply curves for capital, if one is to explain the interest rate as it is *at any time*, i.e., at any point in the course of the growth in the capital supply.” (Knight, 1931: 206), and a few paragraphs later, he justifies the assumption of zero net savings, i.e. of ‘static’ capital supply and demand curves, on the grounds that

[t]he total supply of capital is ‘very large’ in comparison with possible variations in it, and that the opportunity for further investment is on a similar scale with the total. (Knight, 1931: 208)

And then he adds:

Manifestly no possible variation in the amount saved in a year could make enough of a variation in the total supply to affect the interest rate visibly. (Knight, 1931:209)

The conclusion from reasoning which assumes a fixed demand curve is that capital accumulation leads to a slow fall in the interest rate, that savers' choices can at most exert a very little effect on the rate of the decline. The cumulative result would be detectable after a considerable number of years. (Knight, 1931:209)

(We shall have reasons to return to the difference between static and secular equilibrium in section 4 below, when we examine the main features of the latter notion of equilibrium, and the main objections raised by traditional neoclassical scholars against it). Third and finally, the persistence of the data relative to the capital endowment allows the theory to conceive of equilibrium as being reached slowly, i.e. after several market rounds, and by a process of *trial and error* and *experimentation*.

2.1.1. The Role of capital as a single quantity in Schumpeter’s *The theory of economic development* (1934).

Schumpeter’s (1934) work is perhaps a currently-not-widely-recognized but very illustrative example of traditional neoclassical gravitational ways of thinking that, as argued above, must presuppose the value conception of capital. This author’s view is worth examining because it seems to accurately summarize the dominant vision during the 1930’s, exactly when the new formulations of general equilibrium start being introduced and developed. In the first chapter of his book, Schumpeter deals with “static theory”, namely the part of the theory

that studies the “tendency towards equilibrium” and the effects of quantitative changes in the data by e.g. comparative statics exercises. Now, when dealing with static theory, Schumpeter attempts to explain how production decisions tend to adapt to forthcoming demand over sufficient time, and like Kaldor (1934), he stresses the role played in this process by the *gradual* but *continuous accumulation of experience*. For instance, he writes that if someone

[w]ere to observe that a farmer produces corn to be consumed as bread in a distant city, he would be impelled to ask how the farmer knew that this consumer wanted bread and just so much. He would assuredly be astonished to learn that the farmer did not know at all where or by whom it would be consumed. Furthermore, he could observe that all the people through whose hands the corn must go on its way to the final consumer knew nothing of the latter, with the possible exception of the ultimate sellers of bread; and even they must in general produce or buy before they know that this particular consumer will acquire it. The farmer could easily answer the question to him: long experience, in part inherited, has taught him how much to produce for his greatest advantage; experience has taught him to know the extent and intensity of the demand to be reckoned with. To this quantity he adheres, as well as he can, and only gradually alters it under the pressure of circumstances.

The same holds good for other items in the farmer’s calculations... He knows ordinarily, within certain limits, the prices of the things he must buy... he knows the method of cultivation –all from long experience. From experience also all his people from whom he buys know the extent and intensity of his demand. Since the circular flow of the economic periods, this most striking of all economic rhythms, goes relatively fast, and since in every economic period essentially the same thing occurs, the mechanism of the exchange economy operates with great precision. (Schumpeter, 1934: 5-6)

And a few paragraphs below, he continues:

The individual households or firm acts, then, according to empirically given data and in an equally empirically determined manner. Obviously this does not mean that no changes can take place in their economic activity. The data may change, and everyone will act accordingly as soon as it is noticed. But everyone will cling as tightly as possible to habitual economic methods and only submit to the pressures of circumstances as it becomes necessary. (Schumpeter, 1934: 8)

Where the given data are the neoclassical data in the theory of prices and distribution: individuals’ preferences, the different methods of production and the endowments of factors of production, which of course include capital goods.

But in a footnote he explains with respect to capital goods that

As every reader of J.B. Clark knows, it is strictly speaking necessary to consider these stocks, not in their actual shapes –as so many plugs, pairs of boots, and so on- but as accumulated productive forces which can at any moment and without loss or friction be turned into specific commodities wanted. (Schumpeter, 1934: 10, fn. 2)

At the end of the chapter, Schumpeter better explains what he means by “accumulated productive forces”. He queries: “What is capital then if it consists neither of a definite kind of goods nor of goods in general?”, and he answers:

By this time the answer is obvious enough: it is a fund of purchasing power. Only as such can it fulfil its essential function, the sole function for which capital is necessary in practice and for which alone the capital concept has a use in theory, which cannot be just as well replaced by enumerating categories of goods. (Schumpeter, 1934: 119-120)

The previous passages are particularly interesting because Schumpeter explicitly argues that equilibrium is to be reached slowly, and only after that, through repeated interactions, which will generally entail disequilibrium transactions and productions, individuals can gain the required experience about the quantities to be produced and their prices on the basis of the underlying conditions of the market (the “empirically given data”). But then, the plausibility of the argument requires that these underlying conditions (tastes, technology and factor endowments) do not change during the adjustment process towards equilibrium, so that agents can have enough time to learn and correct their past mistakes. Schumpeter plausibly justifies the assumption of given data on the basis of the fact that the process of equilibration along the economic periods “goes relatively fast” –fast, of course, relative to the possible changes in the data, that vary very little from period to period (as he adds, “in every economic period *essentially* the *same* thing occurs”) -and that is why “the mechanism of the exchange economy operates with great precision.”

Schumpeter is therefore aware of the requisite that the data must comply if the adjustment process is to be explained in plausible terms. So, in view of his remarks, he then invokes the authority of Clark and stresses that capital goods must not be considered in their “actual shapes”, but only as materializations of “accumulated productive forces” that, at any moment of time, “can be turned into specific commodities wanted” without considerable “loss or friction”. In effect, as we further discuss in section 5, the existing vectorial endowment of capital goods at a given time cannot be considered as part of the “underlying conditions” if, like Schumpeter, one (realistically) assumes that equilibrium is to be reached slowly and only after several market rounds. In effect, the several quantities and forms of capital goods can be quickly altered if so decided, much

before equilibrium can emerge as the outcome of the repeated interactions of agents in markets. Schumpeter must therefore conclude that it is only those “accumulated productive forces” that, changing sufficiently slowly, can be considered as given when assessing the tendency towards equilibrium. In a nutshell, Schumpeter’s considerations show that in order to encompass the possibility of time-consuming adjustments, the theory necessarily needs to conceive the capital goods as endogenously determined crystallizations of accumulated savings, of a “fund of purchasing power” that cannot be replaced by simply enumerating “categories of goods”, i.e. by describing the data relative to the factor capital as a given vector of heterogeneous capital goods, because then the basis would be missing for allowing the ‘form’ of capital to change as required by the reaching of equilibrium (or of a new equilibrium).

In the above passage Schumpeter also notices that the “underlying conditions” may eventually change; as he further explains later in the book: “The position of the ideal state of equilibrium in the economic system, never attained, continually ‘striven after’ (of course not consciously), changes, because the data change” (*id.*: 62). However he adds that, as a first approximation, these changes in data relative, for instance, to “population” or “wealth” (i.e. the amount of value capital) (*id.*: 63) can be neglected because

[t]hese changes are small per annum and therefore do not stand in the way of the applicability of the static method. (Schumpeter, 1934: 63, fn.63)

As we have argued above therefore, it is the slowness of capital accumulation or population growth over time relative to the process of equilibration of market prices towards their equilibrium values what allows neglecting those changes without necessarily implying that the ‘static’ economy under study must be strictly stationary, that is to say, a stationary state in a secular sense. Indeed, Schumpeter explains that the theory can accommodate change, e.g. by comparative statics exercises:

‘Static’ theory does not assume a stationary economy: it also treats of the effects of changes in data. In itself, therefore, there is no necessary connection between static theory and stationary reality. Only in so far as one can exhibit the fundamental form of the economic course of events with the maximum simplicity in an unchanging economy does this assumption recommend itself to theory. (Schumpeter, 1934: 82-83, fn. 1)

2.2. Demand side roles of capital.

We now turn our attention to the *demand side roles* of capital, although briefly since they have been thoroughly described by Garegnani (1990a) and Petri (2004). The notion of value ‘capital’ of variable form justifies the treatment of this factor of production as analogous to that of labour and land, and this makes it possible to view the demand for capital as a negative function of its price, the rate of interest. So, in the first place, this conception of capital as a single factor made it possible to assume that the factor substitution mechanisms work in the ‘*right*’ direction even in the relevant case, i.e. economies that produce capital goods, the rate of interest being the variable that brings into equilibrium the demand for capital with its given endowment. And given that, as I discuss below, the demand and supply functions of capital as a stock are manifested concretely in the savings-investment market, then the treatment of capital as a single factor provides a plausible foundation for Say’s Law, which I take it to mean that investment adapts to (full-employment) savings^[23].

In the second place, the treatment of the capital goods as embodiments of a single factor of variable form justifies the *sufficient* substitutability among the different factors, which in turn justifies the sufficient elasticity of the factor demand curves even in the relevant case: economies that produce capital goods. In fact, as Garegnani (1990a: 57-58) notices, unless the several endowments of capital goods are treated in this way, equilibria will suffer from insufficient factor substitutability, what Petri (2004) calls the “substitutability problem”: given that the alternative methods available for the production of one commodity will generally call for *different* kinds of capital goods into use, if one conceives each capital good as a distinct factor of production, the factor demand curves will generally be highly inelastic. The very little substitutability among factors implies that many equilibrium prices will be zero, and that the wage rate may be very low or plausibly zero, or so high as to reduce all other factor

²³As Mongiovi (1991: 78-79) explains in this connection, Say’s Law as understood by the classics, i.e. that aggregate expenditure will accommodate to *whatever* level of output is produced in the economy, cannot be applied in the marginalist approach, because only the level of income consistent with full-employment savings can persist. So, in neoclassical theory, Say’s Law must be taken to mean that, “whatever the full employment level of income happens to be, total expenditure will (in equilibrium) be sufficient to support it”(*id.*), because the rate of interest will vary to equilibrate investment decisions with full-employment savings.

rewards to zero; moreover, drastic changes in the wage rate will likely occur if e.g. labour supply increases by only a very slight amount. Given that these drastic changes in distribution do not seem to find an empirical counterpart, the notion of centre of gravitation would be undermined, since it presupposes a correspondence with observation^[24] ^[25].

I conclude with an important remark with respect to the role of the rate of interest in traditional theory that is sometimes overlooked: while in this framework the interest rate is seen as the variable that brings into equilibrium the supply of and the demand for capital as a stock, it is largely implicit in traditional neoclassical authors that the rate of interest must also bring into equality the demand for capital as a *flow* (investment) and its supply, also a *flow* (savings) (Garegnani, 1978: 344-345; Petri, 2004: 127-128). Indeed, given that traditional neoclassical authors realistically acknowledge that adjustments take time to assert their effects fully, they also have to admit that, when distribution changes (owing for instance to changes in total labour supply), the new methods that would be adopted by entrepreneurs in their attempt to implement the new optimal proportions of capital to labour, and which imply that a different kind of capital goods would have to be produced, can initially be implemented in *new plants* only. In fact, at each instant of time most of the existing capital would be crystallized in the ‘old forms’, and only *gradually* the

²⁴ Petri (2009: 9-10) has also noticed that, in the presence of a backward bending labour supply curve, the lack of substitution among factors and hence the highly inelastic labour demand curve can possibly imply a “practically indeterminate” equilibria. In other words, although equilibrium might be unique, there might be an interval of wages such that excess demand remains extremely close to zero. Within this interval, the tendency of wages to change must be assumed to be very weak and hence, to all practical effects, the wage established by the interplay of supply and demand can be considered indeterminate.

²⁵ The fact that to derive sufficiently *elastic* factor demand curves through the action of the substitution mechanisms these capital goods must be seen as embodiments of the same factor of production, seem not to have been properly understood by some contemporary neoclassical scholars. For instance, when analysing the likelihood of unemployment owing to a highly rigid labour demand curve, Dorfman *et al.* (1958) write: “Certainly, the notion that it may be technically impossible to employ the given quantities of all factors seems to be inextricably bound to the idea of fixed, or at least limited coefficients. If a fall in the price of an initially redundant factor decreases the supply and at the same time induces all industries to use the factor at higher intensity, both blades of the scissors are working to wipe out the margin of unemployment” (Dorfman *et al.*, 1958: 365). Notice that according to Dorfman *et al.* (1958), who treat each capital good as a *distinct* factor, the required elasticity of the labour demand curve could be easily obtained by assuming a variable-coefficients technology, i.e. that the process of substitution operates through changes in the *proportions* of the *same* capital goods in existence at a given time. But as argued in the main text, the solution by Dorfman *et al.* is clerly unsatisfactory because unrealistic: it ignores or forgets that in real economies alternative methods of production will usually call for different capital goods into use.

value of capital embodied in the old capital goods, and set free by the closure of the oldest plants, would be able to adopt the new cost-minimizing forms. The implication of this is that the ruling rate of interest would regulate the optimal capital-labour ratio in new plants, where the flow of free capital would meet the labour freed by the gradual closure of old plants. The importance of this qualification will be clearer in the following section.

3. THE PROBLEMS WITH THE ENDOWMENT OF CAPITAL IN VALUE TERMS.

In section 2 we have seen that the value specification of the capital endowment emerges as a *necessary* requisite for neoclassical theory in its attempt to explain, in terms of supply and demand forces, normal or cost-of-production relative prices in economies that produce capital goods, i.e. prices that, although not invariable, can act as a centre of gravitation of actually observed (disequilibrium) prices. However, by the 1930's, it becomes increasingly evident for neoclassical scholars that the traditional conception of capital as a single quantity of value carries with it insurmountable problems. In this section we briefly account for these issues, which can be distinguished between those affecting the supply of capital (supply side problems) and those affecting its demand side (demand side problems).

The *supply side problem* essentially points out that it is not possible to determine prices and distribution by taking as given a quantity of capital in value terms since, being measured as a sum of value, this quantity depends –for a given crystallization in a vector of physical capital goods- on prices and distribution and on the numéraire, precisely those variables that must be explained by the theory. So, given that the quantity of capital changes with changes in the rate of interest, it would be meaningless to say that given supply and demand functions for capital determine the value of the interest rate in equilibrium, since the supply of capital is indeterminable a priori. The supply side problem is, for instance, explicitly pointed out by Hayek (1941) and Lindahl (1939) who, jointly with Hicks (1939), can be considered as the forerunners of the notions of equilibrium currently used by general equilibrium theorists (see

section 5 below). However, this difficulty was admitted by traditional neoclassical scholars as well. Wicksell for instance writes:

But it would clearly be meaningless – if not altogether inconceivable – to maintain that the amount of capital is already fixed before equilibrium between production and consumption has been achieved. Whether expressed in terms of one or other, a change in the relative exchange value of two commodities would give rise to a change in the value of capital, unless its component parts underwent a more or less considerable change. (Wicksell, 1934: 202)

This problem of capital points to the most evident shortcoming behind the value specification of the capital endowment and, although it seems in itself *sufficient* to demolish the whole explanation based on supply and demand forces since equilibrium becomes *indeterminable*, it does not, at least directly and in an obvious way, demolish the pillars of the theory, i.e. the idea that the factor substitution mechanisms work in the right direction, and therefore, that sufficiently elastic factor demand curves can be derived from these mechanisms so as to determine a unique and stable equilibrium. In other words, despite their uneasiness with respect to the value specification of the factor capital, up to the 1960's the bulk of neoclassical scholars seem to unproblematically accept the idea that the demand for capital as a *flow*, i.e. investment (still a value magnitude), is a decreasing function of the rate of interest, and therefore, that the rate of interest is capable of bringing into balance investment decisions with full-employment savings, without suspecting that, once the notion of capital as a value magnitude is questioned, the whole explanation of the tendency towards a full-employment growth path in terms of supply and demand forces is seriously at risk. (As sated in the introduction, this point will be further discussed in the following chapters with reference to some of the works analysed in this dissertation.)

It is only during the 1960's, i.e. during the *Cambridge Capital Controversies*, that it is rigorously and definitely established -and accepted by the neoclassical side of the debate- that in economies that produce capital goods it can well be the case that, unlike in a-temporal production economies (economies where land and labour are the sole factors of production), it is not generally possible to order techniques according to their degree of capital intensity, as is assumed in the explanation of the factor substitution

mechanisms. For instance, it can well be the case that the same technique is used over two different ranges of the rate of interest -this possibility is known as ‘reswitching of techniques’- and as a result, no monotonic inverse relationship can be generally established between the demand for capital and the interest rate. That is to say, it can well be the case that, at least for some ranges of the rate of interest, as the interest rate-wage rate ratio decreases, profit maximizing firms find it optimal to decrease their (long-period) employment of (value) capital relative to labour (*cf.* Garegnani, 1966, 1970)^[26] ^[27]. In other words, the ‘factor employment’ curves may well be upward sloping with respect to their rental prices (incidentally, the positive relation between the employment of capital and the rate of interest is known as ‘reverse capital deepening’). As I discuss below, the consequences of this are far reaching.

The first consequence is that the neoclassical claim that the forces of supply and demand will push the economy to the full-employment of resources is devoid of legitimacy: suppose for simplicity a rigid labour supply; then, under upward-sloping employment curves, wage competition in the face of unemployment will decrease, rather than increase, the aggregate level of employment; in other words, contrarily to what neoclassical theory asserts, the distance between the given labour supply and labour demand will tend to widen with wage-rate reductions. No bases remain to believe in Say’s law either: as argued in section 2.2 above, the neoclassical justification for it rests on the possibility that variations in the rate of interest may bring into line investment decisions with full-employment savings. But this role of the rate of interest is devoid of justification if the demand for capital as a stock, and hence the demand for capital as a flow –investment- are increasing functions of the interest rate.

²⁶ Where, let us recall, the value of capital –the logic of the approach implies- must be measured in the same representative basket of consumption goods used to measure income, and capable of measuring the ‘quantity of consumption’ from which consumers abstain when by saving they create capital.

²⁷ Some attempts to determine a negative relationship capital employment and the rate of interest rely on the Average Period of Production (e.g. Böhm Bawerk’s, 1891 and Wicksell’s, 1901). The problems behind this notion, in particular, the assumption of only one homogeneous factor and single –instead of compound- interest, will be examined in some detail in chapter 6, since also Allais attempts to use this measure to determine the ‘degree of roundaboutness’ of different productive methods.

But the most important implication of reverse capital deepening and reswitching seems to be that the supply-and-demand explanation of income distribution is deprived of foundations. In our discussion of the mechanisms of substitution (section 1.1.1.), we have noticed that neoclassical theory *validates* the interpretability of the ‘employment curves’ as factor demand curves by introducing the additional hypothesis that wages, profits and rents react to factor excess demands. Granted that the factor substitution mechanisms work in the *right* direction also in capital-goods’ producing economies, this additional hypothesis justifies the claim that the forces of supply and demand are always at work in the factors markets, and hence that, e.g. an excess supply of labour will eventually be eliminated by causing sufficient decreases in the wage rate. But once the possibility of an upward-sloping labour employment curve is accepted, there is no reason to argue that an excess supply of labour will trigger wage reductions, because this would “compel us for the possibility of an absurd process of unending deflation of prices and wages” (Garegnani, 1988b: 206). Given that an indefinite fall in wages in the face of unemployment finds no correspondence with observed facts, the criticism to neoclassical distribution theory owing to reswitching and reverse capital deepening entails that in market economies there *must* exist some kind of ‘wage rigidity’ that prevents the emergence of this outcome. As Garegnani declares:

The assumption of money wage rigidity would appear to be a *consequence*, and *not a premise* of the thesis that there exists no tendency to the full employment factors. (Garegnani, 1988b: 206, emphasis added)^[28]

However, to refer to these factors as ‘rigidities’ is to some extent misleading: one must not see these factors as impediments to the otherwise correct working of the forces of supply and demand, but as indispensable elements to allow for the normal functioning of market economies -for instance, by impeding that indefinite wage reductions in the face of unemployment may bring about unbearable social conflicts. (We shall return to these issues in section 5.2.3.

²⁸ Garegnani’s claim that wage rigidities are a consequence of unemployment is not new. One can find it, e.g. in Keynes (1936) (*cf.* Aspromourgos, 1997 for a detailed discussion) and in other scholars. For instance Klein (1947) writes that “Within the framework of Keynesian economics wage flexibility does not correct unemployment and leads merely to hyper-deflation if carried to its logical conclusion. But in the real world one observes neither hyper-deflation nor full employment. The explanation is that wages *are* sticky; they are not flexible... Because workers do not bid against one another, we do not experience the hopeless downward spiral.” (Klein, 1947: 90)

There we shall see that, although for different reasons, modern general equilibrium theory is equally unable to provide a plausible justification for the tendency towards full-employment, and ultimately, for the explanation of distribution in terms of supply and demand forces).

4. THE NOTION OF SECULAR EQUILIBRIUM.

I have noticed in the previous section that, during the 1930's, neoclassical scholars start showing evident dissatisfaction with respect to the value specification of the capital endowment in the determination of prices and distribution. From a *formal* point of view, it is possible to overcome this problem by *endogenously* determining the equilibrium quantity of capital. In other words, the assumption made in section 2 that net savings are zero *whatever* prices and distribution can be dropped: the aggregate supply of savings can be envisaged as a function of prices, incomes and distribution derived from consumer intertemporal choices. Equilibrium is defined as a price-quantity configuration such that individuals are *induced* to make zero net savings (a formal model that endogenously determines the quantity of capital in equilibrium will be analysed in chapter 6, in my examination of Allais)[²⁹].

Now, with an endogenous determination of the quantity of capital the issue relative to the specification of capital is only 'formally' overcome because, by introducing this condition in the equilibrium equations, the equilibrium determined is no longer a static stationary equilibrium but becomes a *secular* equilibrium, and represents a position where also the endowment of capital would have adapted to its secular equilibrium value. A first evident deficiency of the secular equilibrium is that it is determined on the basis of a contradictory assumption: on the one hand, the value of the capital stock corresponds to the one to be reached in the very far future; on the other hand, the data relative to preferences, population and technical knowledge are the same as those

²⁹ The intuition can be grasped by considering a one-good economy where corn is produced by corn and labour: under opportune assumptions about preferences determining a propensity to net savings that goes to zero when the rate of interest becomes sufficiently low (and e.g. a uniform distribution of the corn-capital stock among consumers assumed to have the same preferences as to saving), a model can be constructed such that if the stock of corn-capital is sufficiently big and hence its marginal product and the rate of interest sufficiently small, the propensity to net savings becomes zero. The latter condition will then endogenously determine the secular equilibrium stock of corn-capital.

determining a static equilibrium, and these data attempt to represent actual economic conditions. But in a truly secular equilibrium the quantity of labour should be also endogenously determined. And the assumption of *given* preferences and technical knowledge must also be removed (*cf.* Garegnani 1960, appendix E). However, it seems impossible to predict at a sufficient level of generality those preferences or technical knowledge ruling in the further future, i.e. when capital accumulation and population growth would have come to a halt. And even if this could be somehow ascertained, the connection between secular equilibrium conditions and actual economic conditions would be completely lost (see below).

A second deficiency of the concept of secular equilibrium determined on the basis of supply-and-demand forces is that this notion still presupposes the demand-side roles of capital, in particular that the factor substitution mechanisms work in the ‘right’ direction: e.g. if a neoclassical economist compares two secularly stationary economies, A and B, with the same data except that in economy A preferences are such that the rate of interest needed to bring about a secular equilibrium is lower than in B, she would predict that the secular value of capital in economy A is higher than that in economy B. But there is not reason why this may be the case, because there is no reason why the stationary demand for capital is a decreasing function of the rate of interest.

In any case, we have seen in the previous section with reference to Knight’s and Schumpeter’s work that generally traditional neoclassical scholars were aware of the difference between a static and a secular equilibrium. Further evidence confirms this. For instance, in a well known article, “*On a Certain Ambiguity in the Conception of Stationary Equilibrium*” (1930), Lionel Robbins argues that the main distinctive feature of the static equilibrium is that “population and capital are to be constant -they are not allowed to vary” (*id.*: 204), and he continues, “this constancy is the condition of equilibrium” (*id.*), that is to say, the quantities of labour and capital are part of the data. On the other hand, he proceeds to remark that, according to the secular notion of equilibrium, the constancy of the factors of production “is simply one of the *resultants* of the equilibrating process” (*id.*), i.e. these variables become

endogenous magnitudes. The point is that traditional neoclassical scholars had good reasons not to determine an equilibrium position on the basis of an endogenous determination of the quantity of capital, and to specify instead the factor capital as an amount of value in the general equilibrium equations.

In line with what has been argued above, the main objection raised was that actual economies are far from being stationary in a secular sense. Indeed, in real economies population is growing, and positive rates of capital accumulation are typically observed. So, some prominent neoclassical economists like Robbins or Ramsey (see below) sensibly argued that relative prices determined under secular conditions could not act as centres of gravitation of actually observed prices. On the other hand, the given amounts of factors (among which capital the value single factor) did not in the least prevent the analysis from asking about the effects of slow, or once-and-for-all, effects of changes in those amounts. These reasons arguably explain why we find Robbins (*id.*: 204, fn. 3, emphasis added) arguing that “A conception of equilibrium that is to be *fully realistic* must be a conception of moving equilibrium”, i.e. an equilibrium determined on the bases of *given* factors of production.

In this connection, it seems worth reviewing Frank Ramsey’s reflections on these issues in his seminal contribution to the theory of economic growth (Ramsey, 1928). Ramsey’s considerations are interesting given that, with minor modifications, his canonical growth model is still currently used in neoclassical macroeconomic textbooks to show the accumulation path followed by the economy in the very long-run; however, these same textbooks overlook or simply ignore that in his 1928 work, Ramsey openly acknowledges that these very-long-run or secular conditions are essentially irrelevant for understanding the actual trends in prices and distribution, and hence he also puts forward an explanation of distribution for the actual (growing) economy. The author first shows that under secular equilibrium conditions net savings must be zero. This means that society’s rate of time preference (ρ) must equal the marginal productivity of capital (*id.*: 556) and hence that, given ρ , the quantity of capital ruling under secular equilibrium conditions passively adapts to its stationary

demand, that is to say, it is an *endogenously* determined magnitude. However, he subsequently remarks that

[i]t may be centuries before equilibrium is reached, or it may never be reached, but only approached asymptotically; and the question arises as to how, in the meantime, the rate of interest is determined. (Ramsey, 1928: 556)

Ramsey thus suggests determining the rate of interest that is to be observed on average “in the meantime”, i.e. at any given time, by finding an equilibrium between a “temporary” capital supply curve that represents the given quantity of capital, the single factor of variable ‘form’, available in the economy at a particular period in the process of capital accumulation, and hence assumed to be completely inelastic with respect to the rate of interest, and a demand schedule determined by the marginal productivity of capital (*id.*: 556-557). So, once again, owing to the need to determine an equilibrium rate of interest that is able to reflect the actual conditions ruling in the economy in any given period, we find Ramsey, as the bulk of traditional marginalist scholars, relying on the traditional conception of capital the single factor, necessarily an amount of value.

But I would like to point out a further aspect that has been little noticed: even assuming that the forces of supply and demand do tend to establish a secular equilibrium, this position *is not persistent* in the precise sense of persistency, that is, a speed of change of the data of a lower order of magnitude than the speed of convergence towards equilibrium, because the speed of adaptation of the endogenous variables towards a stationary position where there is no further incentive to accumulation is so slow that it is, no doubt, of lower order of magnitude (or at most of the same order of magnitude) than the speed of change of the determinants of the equilibrium (preferences, technical knowledge, population). It can be concluded therefore that, contrary to the vision expressed by some contemporary scholars (e.g. Zaghini, 1990) this solution to the supply side problem of capital must be rejected because it stands in contradiction with the concept of equilibrium as a centre of gravitation of empirically observed magnitudes. This lack of persistence of the data that determine a secular equilibrium is what seems to have prompted Knight (1930:

198) to wonder “whether accumulation is to be treated as an equilibrating process” or not, and to answer that the tendency towards a secular equilibrium

[i]s indefinitely remote in time, giving ‘other things’ indefinite scope for action (Knight, 1930: 200)

where the “other things” are the “given conditions” (*id.*) -or data- ruling at a given instant of time. Knight accordingly concludes with the following warning:

For very small changes it is admissible to assume that while any element or condition changes, the others in the same group remain fixed. But in discussing the trends over any considerable period of time this must not be done. The greatest caution needs to be exercised in determining and specifying the systems of constants or long-periods processes and of variables adjusting to them (and to each other), if the notion of tendency towards equilibrium is to yield sound results. (Knight, 1930: 200)

A final consideration is in order. While the different meanings of a static and a secular equilibrium seem to be clear enough for traditional neoclassical scholars, this distinction was obscured by subsequent literature. In effect, as we shall see in subsequent chapters, the developments in general equilibrium theory that start taking place during the 1930’s interpret the traditional neoclassical system (which includes a given value of capital among its data) as one aiming to describe *a secular equilibrium*; as Garegnani (1976, 2010a) has noticed, this misrepresentation of the traditional neoclassical system allowed the first generation of modern general equilibrium theorists, Hicks (1939) and Hayek (1941) in particular, to “kill two birds with one stone” (Petri, 2004: 121). In the first place, in a secular equilibrium the need to specify the capital endowment in value terms does not arise, hence this equilibrium can at least be *determined*^[30]. Second, the secular interpretation of the traditional neoclassical system allowed those authors (e.g. Hicks, Lindahl and Hayek) to *dismiss* the empirical relevance of this equilibrium for the same reasons pointed out by Robbins, Ramsey and Knight. Both considerations played important roles in subsequent developments of general equilibrium theory. On the one hand, the removal of the supply side problem of capital allowed preserving the internal

³⁰ Although under the contradictory assumption of current labour supply and technical knowledge, and the capital stock to be reached, say, after 100 years’ now.

consistency of traditional theory^[31] while, on the other hand, the new developments were defended on the grounds that they were more suitable to address a correspondence with observation than those constructions that allegedly attempted to explain the actual trends in relative prices by determining secular equilibrium states. For reasons to be discussed below (section 5), this last claim is in fact entirely disputable and liable to objection.

5. THE NOTION OF MARKET NASH EQUILIBRIUM.

5.1. Walras's inconsistency.

The last notion of equilibrium that will be examined in this chapter is the notion that currently dominates modern general equilibrium theory; this equilibrium concept finds its roots in Walras's (1954) work since, like the French author, modern versions of general equilibrium theory include the several endowments of each capital good as part of the data of the equilibrium. But Walras's specification of the capital endowment was criticised and rejected by the bulk of neoclassical scholars of that time (*cf.* e.g. Wicksell, 1934; also Hicks, chapter 4 below). The main analytical deficiencies of Walras's treatment of capital have been thoroughly examined by Garegnani (1960, 1990a, 2008 [1962]; *cf.* also Petri, 1978, 1989: 185-186 and De Vivo, 1976). Here I limit myself to point out that Garegnani has shown that Walras' aim to determine long-period or normal prices on the basis of a given endowment of capital goods is *inconsistent*: the endowments of capital goods must be endogenous variables if relative prices must yield a URRSP in equilibrium. I will argue in section 5.2 that modern versions of general equilibrium theory cannot solve this problem; they simply *ignore* it. But further difficulties seem to emerge within Walras's system. In his equilibrium equations, no less than in traditional neoclassical authors', it is assumed that the causes determining relative prices and distribution are sufficiently persistent so that it is legitimate to neglect price changes over time^[32]. However, this assumption is no longer justified within

³¹ This consistency is in any case difficult to argue given the other deficiencies of the secular equilibrium already explained.

³² Some influential scholars, e.g. Donzelli (1986) and Tosato (1997), have argued that in Walras's system price changes are not explicitly included in the definition of equilibrium because Walras allegedly assumed *stationary price expectations* "to avoid cumbersome formalizations", (Donzelli, 1986: 311, fn. 84, my translation). However, there is no textual evidence in Walras's work that confirms this view. On the other hand, Walras explicitly acknowledges that the expected prices of capital goods are not stationary; they can change over time and that is why

Walras's system. The arbitrary endowments of capital goods imply that, even assuming that the URRSP condition is satisfied, the changes in the composition of investment needed to bring about the required URRSP condition may be drastic, and hence the assumption that future prices will not change must be rejected. In section 5.2.1 below I critically assess the way in which modern general equilibrium theory incorporates price changes in the definition of equilibrium. Finally, Walras' system presents two further difficulties: the "substitutability problem" and the lack of persistence of the data relative to the capital goods' endowment. As it is argued in sections 5.2.2 and 5.2.3 below, these problems undermine modern versions of general equilibrium theory as well.

5.2. Neo-Walrasian equilibria.

The modern versions of general equilibrium theory have followed Walras in his attempt to determine prices and distribution by including a given vectorial endowment of capital goods among the data of the equilibrium. Owing to this similarity between the formal structure of these models and Walras's, current versions of general equilibrium are also known as *neo-Walrasian*. But neo-Walrasian theory does not –and cannot– solve Walras's inconsistency; the theory simply renounces to determine prices that yield a URRSP as equilibrium prices. For example, since in all likelihood the initial endowments of some kinds of capital goods will be so *abundant* relative to their demand that they will not be fully-employed, in equilibrium those capital goods will yield a zero rental price. And even if these capital goods yield positive rentals in equilibrium, nothing prevents these rentals from being very low owing to the high abundance of their initial endowments relative to demand, and hence it is highly likely that the demand price of these capital goods, that is, the price that savers are willing

'with regard to future changes, expectations differ from individual to individual' (Walras, 1954: 310–311). As argued in the main text, the reason why Walras ignores prices changes is because he aims to determine a sufficiently persistent position that could represent the average of actually observed variables produced by the repetitions of transactions. E.g. in his *Etudes d'Economie Politique Appliquee* (1896, pp. 24–25) when computing the rate of interest observed in actual economies, Walras argues that "A country that capitalizes 5 billion in ten years does not capitalize 500 million per year; this [amount of] capitalization will be the result of certain average years; some years it will be higher and may rise to 700 million or 800 million; 19 other years it will be lower and might go down to 200 million or 300 million. This movement is analogous with the sea's: it includes flood tides, high-tides, ebb tides and low-tides."

to pay in order to purchase them, and that are calculated by capitalising the value of its future rents at the current rate of interest, are lower than their minimum average costs. Therefore, like in Walras's system, it can well be the case that the rate of return on the supply prices of the capital goods is not uniform.

In the remainder of this section I wish to argue that the impossibility of neo-Walrasian theory to determine a URRSP as an equilibrium condition is only an *expression* of the fact that the given endowment of capital goods among the data prevents neo-Walrasian equilibria from being capable of having the role traditionally assigned to equilibrium, i.e. to be a centre of gravitation of actually observed magnitudes over a sufficient interval of time. My claim is based on three main reasons: i) by including the datum relative to the vector of capital goods, the most rigorous versions of the theory must assume the existence of *complete futures markets*, which do not and cannot exist; ii) the datum relative to the vectorial endowment of capital goods is impersistent, and this forces the theory to assume *instantaneous adjustments*, which by themselves cannot give any information as to whether a neo-Walrasian equilibrium is a position that the economy tends to realize; iii) as argued in sections 1.1.1 and 1.2.1, the marginalist approach asserts that the forces of supply and demand will push the economy towards a position of full-employment. But because of ii) neo-Walrasian disequilibrium theory is unable to justify this claim; the persistent tendency of the economy towards full employment must be argued on the basis of persistent forces, and these are currently found in neoclassical macroeconomic theory, i.e. in the *labour demand* curve and in the aggregate *investment function*. However, I will argue that the inclusion of the vectorial endowment of capital goods among the data prevents these schedules from plausibly justifying the neo-Walrasian assumption of full-employment, and more generally, the supply and demand explanation of distribution.

In section 5.4 it is finally argued that, unable to justify the conception of equilibrium as a centre of gravitation, neo-Walrasian theory has been forced to change the meaning attached to equilibrium prices and quantities: these magnitudes limit themselves to represent a *hypothetical* situation of market

clearing, namely, a position where individuals' optimal plans are mutually compatible and hence implementable. In other words, I will argue that equilibrium must be interpreted as a Nash equilibrium applied to the market phenomenon, an equilibrium concept that is sufficiently abstract that it *need not* address the issue of how this position may ever be realized in actual economies.

5.2.1. Neo-Walrasian equilibrium cannot be a position that the economy tends to realize I: the hypothesis of complete markets.

A first reason why the given endowment of capital goods seems to prevent neo-Walrasian equilibrium from being susceptible of having the role traditionally assigned to equilibria, i.e. a position that the economy tends to realize over sufficient time, is the following: neo-Walrasian scholars do not neglect that owing to future changes in the arbitrarily given composition of the capital stock, many relative prices will relevantly change in the not-very-far future and economic agents cannot ignore this fact; therefore, modern theory must attempt to explain how future price changes affect agents' current decisions. However, the point I wish to make is that the way in which the most *rigorous* versions of neo-Walrasian theory, i.e. the theory of intertemporal equilibrium developed by Debreu (1959), incorporate future price changes in the definition of equilibrium is highly unsatisfactory: intertemporal equilibrium theory assumes the existence of complete future markets. Given, as I argue immediately below, that these markets do not exist and cannot logically exist, the hypothesis of complete markets gives us a first reason to question the plausibility that the neo-Walrasian equilibrium path can describe actual paths with sufficient approximation.

Neo-Walrasian theory incorporates price changes in the definition of equilibrium in two different ways. As anticipated in the previous paragraph, the first one is the *intertemporal general equilibrium*, popularized by Debreu (1959). According to the intertemporal equilibrium model, future prices are simultaneously determined with current prices, and the assumption is introduced that *complete futures markets* exist, that is to say, that there are markets for each kind of commodity at each instant of time. The assumption of

complete futures markets implicitly means that agents have correct foresight about future conditions, which may include their ability to predict technological changes and temporary (and non-temporary) shifts in tastes^[33]. The assumption of complete future markets and of correct foresight of future conditions is even harder to digest in those formulations that contemplate the existence of *complete contingent markets*. In these formulations, each market is distinguished by the date of delivery, but also by the delivery being conditional on any sort of event, such as atmospheric conditions or natural disasters. Agents are thus assumed to contemplate the occurrence of all these possible contingencies ('states of nature') (*cf.* Ciccone, 1999). The necessary foresight needed to assume complete (and of course, contingent) future markets should lead us to conclude that it is *logically impossible* for this kind of market organization to exist in actual economies. An intertemporal general equilibrium is characterised by a set of prices and quantities such that all -current and future- markets simultaneously clear.

The second way, popularized by Hicks (1939), is the *temporary general equilibrium*, where the unpalatable assumption of complete markets is dropped, i.e. only spot markets exists for most commodities, and agents are assumed to have expectations (which can be mistaken) about future prices. A temporary general equilibrium is a set of prices and quantities such that only spot markets clear, that is to say, consistency of plans is only achieved for current markets. At first sight, the assumption of possibly mistaken expectations might appear to be more satisfactory than Debreu's hypothesis of complete futures markets; however (apart from the fact that a temporary equilibrium might easily not exist, *cf.* Ravagnani, 2010), the inclusion of expectations as part of the data creates serious problems that no doubt jeopardize the relevance of this equilibrium concept, and this explains why this notion is hardly discussed in modern general equilibrium textbooks (if at all). First of all, price-expectations functions may generally change due to disequilibrium transactions and productions and therefore, even assuming a tâtonnement adjustment process, there is no basis to assume that the equilibrium position determined on the

³³ One then has the right to wonder why, e.g., if agents know that there will be a technological improvement in the future that allows reducing minimum average costs they do not implement in the present.

basis of the initial expectation functions will be eventually reached; the changes in the shape of expectation functions during the process of adjustment can likely cause the path dependence of the equilibrium, and hence introduce a serious element of indeterminacy into the theory^[34]. Second, being entirely subjective, expectations are affected by a myriad of events whose effect in the production, consumption and investment decisions of the individuals cannot be asserted with any degree of approximation; and this adds a further cause of indeterminacy^[35]. This last issue has been brought back to the general attention by Garegnani (1976)^[36] and labelled by Petri (2004) as the “indefiniteness problem”. Finally, it must be noticed that in the temporary equilibrium formulations of the theory, there still is a savings-investment market, and hence the stability of that market still requires the rate of interest to bring into equilibrium savings and investment decisions. However, modern theory is currently unable to justify why a decrease in the rate of interest may increase investment^[37].

³⁴ It is worth highlighting at this juncture that, in the traditional versions of the theory, this problem does not arise because the persistence of the data relative to the capital endowment, measured in value terms, legitimately allows assuming that possibly mistaken expectations will be revised in the light of experience and, assuming that there is a tendency towards equilibrium, these expectations will be *endogenously determined* as correct expectations, hence no need arises to take them as exogenously given.

³⁵ In the theory of intertemporal equilibrium the indefiniteness problem is only *formally* overcome because the assumption of contingent commodities allegedly allows the theory to isolate all the possible causes that affect prices and distribution at any given time. However, as notably Ciccone (1999) has pointed out, this creates further problems, because the possible factors that affect prices and distribution at any given time cannot be reduced to any finite list of events. And this implies that the assumption of contingent markets “merely resolves into an endless multiplication of markets and prices, with the collapse of the theory as such” (*id.*: 83-84), because, e.g., it becomes impossible to describe the equilibrium conditions by a finite number of equations. The theory is then forced to select a finite number of possible events only, and this choice cannot but be *arbitrary*, it would seem. Paradoxically, that it is actually impossible to consider all the possible causes that affect prices and distribution at any given time is implicitly admitted by the theory *itself* in its very attempt to determine ‘equilibrium’ relative prices: as noticed in the first section, if it were actually possible to isolate all the possible causes that affect prices and distribution at every instant of time, and determine their effects with sufficient approximation, the whole notion of equilibrium would be useless.

³⁶ As we will see in the following chapters (chapters 4, 5 and 6) some neoclassical scholars, e.g. Hicks Patinkin and Allais, were well aware of this problem.

³⁷ *Cf.* Arrow and Hahn (1971), Donzelli (1989) and specially Ravagnani (1989, 2000, 2010) for a detailed analysis on the formal features of temporary equilibria. *Cf.* also Petri (1997, 2004: chapter 7) for a critique of the current attempts of marginalist scholars to determine a negative interest elasticity of investment. As we shall see in chapter 4, the negative elasticity of investment demand with respect to the rate of interest has been sometimes argued by implicitly and *illegitimately* assuming that expected future prices are independent of the rate of interest.

I conclude that in the most rigorous versions of contemporary neoclassical theory, i.e. the theory of intertemporal equilibrium, the highly unrealistic assumption of complete markets -or perfect foresight- makes it problematic to envisage an intertemporal equilibrium as a position around which actual magnitudes gravitate over sufficient time, because one must be clear on what is necessary to have an Arrow Debreu equilibrium, and this seems to deprive this equilibrium from being susceptible of having the role of a position that the economy tends to realize.

5.2.2. Neo-Walrasian equilibrium cannot be a position that the economy tends to realize II: insufficient persistence.

But neo-Walrasian theory cannot determine an equilibrium position conceived as a centre of gravitation of actually observed magnitudes for what appears to be a more fundamental reason: the datum relative to the capital goods endowment is not sufficiently persistent relative to the adjustment process. While the overall value of the means of production at a given moment of time might be presumed to be only slowly changed with capital accumulation, the individual quantities of existing capital goods can be changing along the equilibrium path at the same order of speed as do prices and distribution, which can change very rapidly precisely owing to the possible quick changes of the quantities of individual capital goods, which may also entail –owing to the little substitutability (*cf.* section 2.1.2) – quick changes in the marginal product of labour. So, even assuming that the equilibrium path is unique and stable^[38], the theoretically determined prices and quantities can be no longer envisaged as the outcome of time-consuming adjustments. Indeed, given that, like in traditional theory, the conditions that are assumed to be satisfied in equilibrium still imply a uniform price for the same productive service whichever the industry in which this service is employed, it is also implied that “all productive services should be in condition to move in and out of firms and industries” (Garegnani, 1990a: 50), and hence “it seems evident that the time required to realize this degree of mobility of all productive services is such that the composition of the capital stock has to change considerably” (*id.*). Therefore, under time-consuming adjustments, before those accidental deviations from equilibrium can correct

³⁸ Immediately below I discuss how stability can be studied.

and compensate each other and equilibrium can emerge as a sort of average of actually observed magnitudes, the initial composition of the capital stock, and hence the equilibrium position determined on the basis of this datum, may have changed considerably. The theory cannot exclude the possibility of cumulative deviations of the actual path from the equilibrium path either. Therefore, the equilibrium path would give little if any guidance to the actual (disequilibrium) path followed by the economy. The lack of persistence of the arbitrarily given endowment of capital goods has been firstly stressed by Garegnani (1976) and labelled by Petri (2004) as the “impermanence problem”.

The theory’s recourse to the unrealistic hypothesis of the *auctioneer*, who prevents actual productions and transactions from taking place before equilibrium has been actually established, a hypothesis that amounts to assuming *instantaneous equilibration*, is in fact a very unfortunate attempt to overcome the lack of persistence of the data, because otherwise time-consuming adjustments will cause path dependence, and hence the indeterminacy of the equilibrium finally reached (if at all). The understandable dissatisfaction with the auctioneer-hypothesis seem to explain why some contemporary general equilibrium theorists (e.g. Bliss, 1975, *cf.* chapter 8) simply *assume* from the start that equilibrium has been established, and directly focus on the dynamic sequence of equilibrium prices and quantities.

But the sequence of prices and quantities determined by neo-Walrasian equilibrium theory cannot be argued to describe a position that the economy tends to realize unless sufficient reasons are given to justify that the divergence between actual and neo-Walrasian paths is negligible. For this a theory of disequilibrium is needed, but this theory must be different from neo-Walrasian theory: as said, the given endowments of capital goods force modern theory to assume instantaneous equilibration, and hence the stability results, positive or negative, are devoid of any implication for real economies^[39]. In other words,

³⁹ As Fisher (1983: 14) has declared: “In a real economy, however, trading, as well as production and consumption, goes on out of equilibrium. It follows that, in the course of convergence to equilibrium (assuming that occurs), endowments change. In turn this changes the set of equilibria. Put more succinctly, the set of equilibria is path dependent ... [This path dependence] makes the calculation of equilibria corresponding to the initial state of the system essentially irrelevant. What matters is the equilibrium that the economy will reach from given initial

the fact that the equilibrium position itself would be changing during the process of equilibration under time-consuming adjustments implies that one no longer knows where the disequilibrium process is heading. So, if the theory can only conceive auctioneer-based adjustments, it remains silent on how actual economies evolve, and therefore, it is not legitimate to conclude that the dominant forces that neoclassical theory argues to be at work in real economies are the correct ones. In particular, even the claim that, in any market, a supply different from demand will cause price changes becomes open to questioning, and indeed as argued in the following section the claim appears devoid of legitimacy for labour markets.

5.2.3. Neo-Walrasian equilibrium cannot be a position that the economy tends to realize III: the illegitimacy of the assumption of full-employment^[40].

A very important outcome of the fact that modern general equilibrium theory cannot deal with time-consuming adjustments is that *no conclusion* can be derived on the basis of auctioneer-based adjustments as to whether neo-Walrasian equilibrium is a position around which market economies would gravitate over sufficient time. In particular, no conclusion can be reached as to whether the forces of supply and demand envisaged by neoclassical theory can push the economy towards a position of full-employment. Once time-consuming adjustments are admitted, the theory has no way of excluding, e.g., multiplier-accelerator interactions: e.g. assuming with neoclassical theory that an excess supply of labour causes the wage rate to decrease, it may well be the case that the decrease in wages reduces aggregate demand, hence aggregate production, hence labour demand in the following period, with the possibility of a further decrease in wages, and further decreases in aggregate demand, in production and in the level of employment^[41]. So, the conclusion that the total endowment of labour will eventually be fully utilized by suitable changes in the level of

endowments, not the equilibrium that it would have been in, given initial endowments, had prices happened to be just right” (Fisher, 1983: 14).

⁴⁰ I acknowledge in this section the influence of Petri (2003, 2004).

⁴¹ The possibility of multiplier-accelerator interactions suggests that even if workers are initially willing to bid down their wages when unemployed, wage reductions may not succeed in increasing employment. Therefore, as I conclude below, it is likely that in the light of experience, workers learn that wage competition is of no advantage to them, and develop different ways to prevent wages from falling indefinitely in the presence of unemployment.

wages, formalized in the general equilibrium equations by equalizing the labour endowment with labour demand, cannot be argued on the basis of neo-Walrasian disequilibrium theory, because this theory is *silent* about what happens under time-consuming adjustments.

In order to plausibly justify the actual tendency towards full-employment, neoclassical theory must find persistent tendencies at work, and these are currently found in neoclassical macroeconomic theory, i.e. in the *labour demand curve* (a negative function of the real wage rate) and the *aggregate investment function* (a decreasing function of the rate of interest). However, in the remainder of this section I wish to argue that these two schedules do not allow modern theory to conclude that actual economies tend to the full utilization of resources. As to the demand for labour, we observed in section 2.1.2. that when the different capital goods are treated as distinct factors of production, the possibilities of substitution among factors (labour, land and different capital goods) will in all likelihood be very limited, and hence the labour demand curve cannot be presumed to have the sufficient elasticity to prevent equilibrium outcomes that would deprive the theory of plausibility: e.g. a zero wage rate, or so high a wage rate to reduce all other factor rewards to zero; or a drastic change in the wage rate due to a slight change in the supply of labour.

Even if the labour demand curve determined on the basis of a given endowment of capital goods is sufficiently elastic with respect to the wage rate, implausible outcomes as a zero wage rate or an indefinitely high wage may emerge along the *sequence* of neo-Walrasian equilibria. One must in fact consider that, along with those technical choices made on the basis of a given endowment of capital goods, in any given period there must be also long-period choices being made by firms, and which will eventually dominate over the short-period choices. So, assuming that over the relevant period the equilibrium between investment and full-employment savings is preserved, it may well be the case that due to reswitching or reverse capital deepening, when the real wage falls, the existing capital goods are replaced by new ones that employ *less* labour per unit of output. Therefore, along the sequence of neo-Walrasian

equilibria, there will be a continuous shift of the ‘very-short-period’ labour demand curve^[42] to the left that will end up by reducing wages to zero^[43].

Finally we must consider that, even accepting a negative real-wage elasticity of labour demand, the absence of aggregate demand problems, i.e. Say’s Law, requires that, when wages decrease and production increases, the excess of production over consumption, i.e. savings, are absorbed by investment; and this in turn needs that variations in the rate of interest can accommodate investment and savings decisions. However, once the notion of capital as a single factor is shown to have no solid theoretical foundations, there is no basis to presume that a decrease in the rate of interest will sufficiently increase investment. In other words, contemporary neoclassical theory cannot provide any plausible mechanism that justifies why full-employment savings will be absorbed by investment via appropriate changes in the rate of interest. But then, if the excess supply of labour over the amount needed to produce the full employment demand for consumption goods can not be entirely absorbed by the capital goods’ industries because investment demand does not necessarily rise *pari passu* with savings, labour demand will not necessarily increase when the wage rate decreases because production will not necessarily increase, and hence the assumption of indefinite wage reductions in the face of unemployment would again lead to the implausible conclusion of zero wages in equilibrium, revealing the implausibility of the assumption itself.

A first important conclusion that emerges from the previous paragraphs is the following: the neo-Walrasian treatment of capital, which implies: i) the need to assume instantaneous equilibration, ii) insufficient substitutability among factors and iii) the lack of any solid justification for the negative inclination of the investment schedule, raises serious doubts about the legitimacy of the neoclassical claim of the tendency towards the full-employment of resources, a situation that the theory assumes to hold under general equilibrium conditions. But more generally, the lack of a plausible explanation of the working of the

⁴² That is, the labour demand curve determined for a given endowment of capital goods.

⁴³ A formal example of the possibility that wage reductions decrease labour employment by shifting the labour demand curve to the left along the sequence of neo-Walrasian equilibria can be found in Duffy (2006: chapter 6, sections 6.3.2 and 6.3.3).

supply-and-demand forces in factor markets raises serious doubts about the neoclassical explanation of income distribution in actual economies, the *distinctive* feature of the marginalist approach.

Let me insist on this important point: if neo-Walrasian equilibrium, or the sequence of these equilibria, is conceived as a position that the economy tends to realize over sufficient time it must be because, as assumed by traditional neoclassical theory, the forces of supply and demand are behind neo-Walrasian equilibria as well. E.g. that, within reasonable time, an excess supply of labour causes *moderate* real-wage reductions that eventually succeed in eliminating unemployment. But if wage reductions do not significantly improve the situation of the working class, it is plausible that workers learn from experience that wage competition must be avoided even from a strictly selfish point of view, and different ways of preventing wage reductions may gradually emerge in the form of social norms, etc (*cf.* Mongiovi, 1991; Petri, 1994, 2004: 318-320; Levrero, 2011). Notice then that, if customary norms, conventions and other institutions are accepted as factors capable of persistently influencing income distribution, and that are also seen as indispensable elements to allow for the normal functioning of market economies -for instance, by impeding indefinite wage reductions in the face of unemployment that may bring about unbearable social conflicts- then this seems to strongly indicate that the forces and mechanisms determining the distribution of income in capitalist economies are different from the supply and demand forces envisaged by marginalist theory.

5.2.4. Neo-Walrasian equilibria as Market Nash Equilibria.

In view of the serious problems that emerge when one attempts to conceive neo-Walrasian equilibrium (or the sequence of equilibria) as a centre of gravitation of actual variables, it is not surprising that the most attentive modern generally equilibrium theoreticians, e.g. F. Hahn (*cf.* chapter 8), have changed the meaning attached to neo-Walrasian equilibrium; neo-Walrasian theory has “simply changed the question”, as Eatwell (2012:4) has recently argued. In other words, the view, shared by all traditional scholars, that it is *necessary* realistically to assess the tendency towards the position where supply and demand coincide –i.e., Walras’s “practical solution”, “empirically” solved by

the market- in order for this position to have the theoretical status of equilibrium, and not to be considered, instead, as a “curious” position where the equality of supply and demand emerges as a purely “accidental” event (Wicksell, 1934: 59), is no longer shared by the most attentive contemporary general equilibrium theoreticians, who limit themselves to computing Walras’s “mathematical solution”, namely, to show that, on the basis of the given data (preferences, technology, factor endowments, different states of nature), agents optimal decisions can be simultaneously implementable.

To put it in a nutshell, according to neo-Walrasian theory, equilibrium relative prices and quantities determine a Nash Equilibrium applied to the market phenomenon, and hence I propose to refer to the set of equilibrium prices and quantities thus determined as a Market Nash Equilibrium (henceforth MNE)[⁴⁴] [⁴⁵]. In fact, this concept of equilibrium is sufficiently abstract so that it allows ‘disentangling’ ‘equilibrium theory’ from the ‘theory of adjustments’, as if they were, and had always been, two isolated issues that bear no relation to each other[⁴⁶][⁴⁷]. Ken Binmore, a well-known game-theorist, has

⁴⁴ Indeed, in their seminal contribution, Arrow and Debreu (1954: 273) explicitly have recourse to the notion of Nash Equilibrium to prove the existence of equilibrium in a competitive economy.

⁴⁵ Also Mark Blaug (2003) has noticed the nature of Nash equilibrium entailed by Neo-Walrasian prices. When assessing the theoretical status of neo-Walrasian equilibria, which for him are the result of a “Formalist Revolution”, he writes: No wonder then that they [Arrow and Debreu] made use of Nash’s relatively new concept of equilibrium to solve the game of ‘an abstract economy’. Because the justification for a Nash equilibrium is a negative one: a Nash equilibrium in a non-co-operative game is such that each player’s independent strategy is the best response to the strategies actually played by his or her rivals and this is true for each player in turn; in short, nothing other than a Nash equilibrium can be the equilibrium solution of such a game because in the end no player can improve on the outcome. Note that this says nothing about the process whereby the equilibrium is obtained... Equilibrium is simply imposed as a fixed point in which market adjustments have come to an end.” (Blaug, 2003: 147). Blaug however does not seem to realize that with the notion of MNE modern general equilibrium theory does not argue that “adjustments have come to an end”, since this claim already presupposes that there is some adjustment behind the notion of MNE. The notion of MNE implies that adjustments *are not needed* in order to define a neo-Walrasian equilibrium.

⁴⁶ Interestingly enough, Giocoli (2003, 2004) documents that in his Ph.D. dissertation, and against the use that modern neoclassical theory makes of the notion of Nash equilibrium, J. Nash does attempt to plausibly explain how a Nash Equilibrium can emerge as the outcome of agents’ interactions. Nash calls this explanation the “mass action interpretation” of the notion of Nash equilibrium. As Giocoli (2004: 647) explains, this “requires that many rounds of the same game be played. In this dynamic setup there is no need to postulate that players have full knowledge of the game structure or any special reasoning ability. Agents are instead assumed to be able to exploit their experiences in previous rounds of the game in order to accumulate information about the relative merits of the available strategies”. Note that this conception of equilibrium as a position that is reached through a process of trial and error and experimentation is fully traditional (recall, e.g. Kaldor’s (1934) and Schumpeter’s (1934)

precisely argued that the disentanglement between ‘equilibrium theory’ and ‘adjustment processes’ implied by the conception of equilibrium as a Nash Equilibrium “freed” contemporary (neoclassical) economists of the need

[t]hey had previously perceived to tie down the dynamics of the relevant equilibrating process before being able to talk about the equilibrium to which it will converge in the long run. (Binmore, 1996: xii)[⁴⁸]

However, closer inspection reveals that the ‘disentanglement’ is only a *camouflage* that cannot eliminate the theory’s main deficiencies: by not discussing how those equilibrium prices and quantities determined by the theory may plausibly emerge in market economies, the neo-Walrasian approach simply avoids facing the issue that, e.g., Walras (1954: 162) legitimately envisaged as the “*essential point*” economic theory must deal with[⁴⁹], or that e.g. Marx identified with *the* object of science when, in a letter to Kugelmann of 1968, he declared:

Science consists precisely in demonstrating *how* the law of value asserts itself (Marx, letter to Kugelmann, 1968, emphasis added)

6. SOME PRELIMINARY CONCLUSIONS.

In this first chapter I have argued that it is the unsatisfactory treatment of the data relative to the capital endowment as a set of physically heterogeneous capital goods what prevents modern neoclassical theory from determining an equilibrium outcome susceptible of having the role of a centre-of-gravitation of actually observed variables, and what forces it to determine, instead, a MNE, a

contributions examined above), and therefore it *presumes* persistent data. As argued in the main text, this interpretation is no longer defensible when the physical capital goods are included among the data.

⁴⁷ There is another independent reason why neo-Walrasian equilibrium cannot be conceived as a centre of gravitation of actually observed magnitudes that explains why modern theory is forced to disentangle ‘equilibrium theory’ from the ‘theory of adjustments’: the Sonnenschein-Mantel-Debreu results of the 1970’s have shown that the standard assumptions made on general equilibrium theory about individuals’ behaviour (i.e. that preferences are convex, monotone and continuous) do not impose enough structure on aggregate excess demand functions so as to ensure the stability of the equilibrium (*cf.* Kirman, 1989). Eatwell thus remarks, “The separation of the question of existence from that of stability of equilibrium has become a necessary part of neoclassical general equilibrium theory since the realisation that, in general, no equilibrium could be demonstrated to be stable.” (Eatwell, 1982: 225, fn 8).

⁴⁸ However, Binmore does not feel the need to discuss why modern neoclassical economists needed to be freed from explaining how equilibrium comes about.

⁴⁹ Considering that modern general equilibrium theory must necessarily avoid discussing what Walras saw as the “*essential point*” of economic theory, it seems worth wondering how accurate is the name ‘neo-Walrasian’ used to characterize modern neoclassical theory.

solution concept that allows *disentangling* ‘equilibrium’ from ‘adjustment’ theories, and treating them, as Hahn does, as two isolated issues that bear no relation to each other. The high cost paid is that this new notion of equilibrium is useless to explain and predict the average trend of actual prices and quantities.

Having said that, the following point seems worth stressing, since in my view it is a *key* element to understand why the notion of MNE was initially accepted by some influential neoclassical scholars since its introduction in economic theory during the 1930’s, and eventually became dominant among neoclassically oriented scholars in the 1960’s and 1970’s: unlike Hahn, all the influential neo-Walrasian scholars to be studied in this dissertation –from Hayek, Lindahl, Hicks and Lange, to Lucas, Malinvaud and Arrow- endow the MNE notion of equilibrium with explanatory-predictive power along traditional neoclassical lines, i.e. they argue that, over sufficient time, the forces of supply and demand will push actual economies to gravitate around a full-employment growth path. And to argue in this way they have recourse to *illegitimate* reasonings: despite the alleged abandonment of the traditional notion of capital –on which the plausibility of the supply-and-demand explanation of prices and distribution, is finally assumed to rest- and its formal replacement by a given set of capital goods in the general equilibrium equations, these scholars still rely on traditional neoclassical reasonings that, as a matter of logic, must presuppose the notion of capital as a single factor in value terms. On the one hand, we will see that, in one way or another, these scholars admit that the supply-and-demand forces *take time* to assert themselves, and this implies that the endowments of capital goods must be endogenously determined variables, because the data that determine the equilibrium must be sufficiently persistent so that equilibrium can emerge as the outcome of trial and error an experimentation of the agents that trade in markets. On the other hand, the traditional neoclassical tendencies are justified by envisaging a demand for capital as a *flow* –investment- to be in a negative relation with the rate of interest; therefore, we will see that, in their models, the latter variable cannot but perform the traditional role assigned to it, namely, to bring into equilibrium investment decisions with full-employment savings. In so arguing, these

scholars apparently overlook that, once the traditional notion of capital is erased from economic analysis, also the mechanisms traditionally envisaged by the theory to justify the gravitation of actual variables around a full-employment growth path are seriously at risk.

If the previous considerations are sound, the following conclusion will follow as a corollary: the marginalist approach is unable to satisfactorily incorporate capital goods within the supply and demand explanation of prices and distribution, and this seems to have condemned modern general equilibrium theory to face an *unpleasant dilemma*: either neo-Walrasian theoreticians –as e.g. Hayek, Lange, Malinvaud, Lucas or Arrow- attempt to argue a correspondence of the equilibrium path with observation, and in so doing, *inescapably* rely on the traditional and illegitimate notion of capital as a single scalar; or, alternatively, modern neoclassical theory and the notion of MNE entailed by it are taken at face value –as for instance Hahn does-, with the bitter consequence that the theory must unavoidably renounce to use the neoclassical approach for description and prediction of real world events. Either way modern general equilibrium theory runs into a blind alley; the implication is that, to predict with reasonable accuracy price, quantity and distributional trends as observed in actual economies, an explanation with different theoretical foundations is necessarily called for.

PART II

THE NOTION OF EQUILIBRIUM AMONG THE FIRST GENERATION OF NEO- WALRASIAN SCHOLARS

2.

HAYEK'S *THE PURE THEORY OF CAPITAL* (1941)

INTRODUCTION.

This second part of the dissertation examines some of the theoretical contributions that during the 1930's and 1940's set the foundations of neo-Walrasian theory. This chapter begins with the assessment of Hayek's *The pure theory of capital* (henceforth, PTC), and it is divided in two main parts. In the first part I discuss in some detail Hayek's main contentions on the relation between capital theory and equilibrium analysis. I show that Hayek rejects traditional neoclassical capital theory on two main grounds: its recourse to the concept of stationary equilibrium -that Hayek wrongly identifies with a secular equilibrium^[50]- and to the value conception of the factor capital -that Hayek correctly sees as a logically flawed concept. Hayek therefore develops a perfect foresight temporary equilibrium -a Market Nash Equilibrium (MNE) – in the attempt to avoid any reference both to stationariness and to the notion of capital in value terms^[51]. However, Hayek will prove unable to avoid the reference to those notions. As we shall see, the attempt to use the new concept of equilibrium to explain where the actual economy tends pushes Hayek to reintroduce in his framework both the notion of stationary equilibrium and the traditional conception of capital: first, given that Hayek accepts that an equilibrium that is determined on the basis of a given endowment of capital goods is essentially “transient”, in order to explain the actual tendencies of the economy, the sequence of MNE prices is argued to eventually reach a secular equilibrium state. The secular equilibrium, I submit, works as a centre of gravitation in PTC, since allows Hayek to reach plausible and well-determined long-run outcomes by providing the *direction of change* of the sequence of MNE

⁵⁰ For the distinction of these two notions of stationarity the reader is referred to the first chapter.

⁵¹ Is by now well known that the notion of MNE in Hayek's theoretical work does not appear for the first time in PTC. E.g. it is also possible to find this concept in Hayek (1928) and Hayek (1937). Neither does his rejection of the notion of a given quantity of capital (e.g. Hayek, 1936). However, due to reasons of space, I will mainly concentrate on PTC, also because it summarizes Hayek's research during the late 1920's and 1930's.

prices. I subsequently attempt to show in the second part of the chapter that, albeit implicitly, Hayek's analysis also takes for granted the traditional notion of capital as a single scalar, since the process of gravitation of MNE equilibrium prices to their secular equilibrium values is explained in terms of the traditional factor substitution mechanisms, that in Hayek's framework are assumed to be operative *over* the sequence of temporary equilibria. The overall conclusion of the chapter can be briefly summarised as follows: behind Hayek's reasonings there is a major logical 'jump', namely, an *illegitimate* attempt to present traditional results that, as a matter of logic, must presume the notion of capital as a single factor, as justified by a heterogeneous-capital-goods framework^[52].

1. EQUILIBRIUM ANALYSIS IN PTC.

1.1. Hayek's assessment of traditional theory: the assumption of stationarity and the endowment of capital.

In PTC, Hayek aims to provide new insights into capital theory; he argues however that there is a strong link between the theory of capital and equilibrium analysis, and hence "By far the greater part of the present investigation will be confined to that part of the subject which belongs to equilibrium analysis proper" (PTC: 4). Precisely, the author observes that most of the shortcomings of traditional neoclassical capital theory are related to two main topics, a) the assumption of a secular stationary equilibrium and b) the concept of capital as a single homogeneous magnitude. Hayek esteems that a) and b) are not independent issues, as the next paragraphs discusses.

⁵² It might be worth noting that the issue of the change in the notion of equilibrium in Hayek's work has been studied for the first time by Milgate (1979, 1982). Milgate claims however, that, due to the problem of capital as a value magnitude, Hayek abandoned the notion of equilibrium as a centre of gravitation. Also Donzelli (1986) has argued that in PTC Hayek abandoned the traditional notion of equilibrium; however, unlike Milgate, Donzelli claims that Hayek had good reasons to abandon the traditional method of analysis because the traditional conception of equilibrium would be an *irrelevant* notion to study actual economic states. For the reasons argued in the first chapter, I cannot agree with Donzelli's view. As to Milgate's view, while I share his contentions that the problem of capital is no doubt behind Hayek's attempt to replace the traditional notion of equilibrium with the concept of a MNE, I will however argue that, in Hayek's work, the notion of equilibrium as a position that the actual economy tends to realize after sufficient time still plays a central role in PTC. In this sense, I will bring further support to Gehrke's (2003) claim that the notion of perfect foresight MNE is used by Hayek to study transitional dynamics between stationary equilibria. To this claim, I will add in this connection that in the attempt to examine the direction of change of MNE along the intertemporal path, the traditional conception of capital as a single scalar manifests itself from its demand side: in PTC the traditional factor substitution mechanisms are assumed to work over the sequence of temporary equilibria.

Hayek's assessment of traditional capital theory begins by pointing out that "the problems arising of the dependence of production on the availability of 'capital' have hardly ever been studied for their own sake and importance" (*id.*: 4) and that these issues had been "entirely subordinate" (*id.*: 5) to the problem of explaining the rate of interest. The fact that capital theory was relegated as a mere "adjunct" (*id.*) of the theory of interest had "unfortunate" (*id.*) consequences: first, because "the general principles" that regulate the rate of interest were analysed by having recourse to "the simplest imaginable cases" (*id.*), i.e. the stationary state. Second, "and even more important" (*id.*), because the traditional neoclassical aim to explain interest "by analogy with wages and rent, as the price of the services of some definitely given 'factor' of production, has nearly always led to a tendency to regard capital as a homogeneous substance the 'quantity of which could be regarded as a 'datum'" (*id.*) that can substitute the specification of the vectorial endowment of physically heterogeneous capital goods. Hayek subsequently stresses that it is more than "doubtful" (*id.*: 6) that the specification of the capital endowment as a single quantity was a "fortunate" (*id.*) choice even to explain the phenomenon of interest; but more importantly, in the aim to understand the "dynamic process" of production, it was "disastrous" (*id.*), because the "stock of capital is not an amorphous mass but possesses a definite structure" (*id.*). Hayek thus observes that

The treatment of the capital problem in terms of the demand for and supply of one single magnitude is *only possible* on the assumption that the proportions just described stand in a certain equilibrium relationship to one another. (PTC: 7, emphasis added)

In the next two sections I discuss the stringent conditions that according to Hayek must be fulfilled in order for it to be possible to treat the capital problem in terms of the supply of and the demand for a single factor of production.

1.1.1. The problem of the capital endowment.

As to the supply of capital, Hayek argues that it is only under the assumptions of equilibrium i) between the demand for and the supply of the several capital goods, and ii) between their selling price and their cost of production -conditions that, according to Hayek, can only hold in a stationary state (a secular stationary state, see section 1.1.3 below)- that the supply of the

physically heterogeneous capital goods can be actually represented by “a quantity of capital in the abstract which could be set against a marginal productivity schedule of capital as such, and in this sense there would be a unique correlation between ‘the’ quantity of capital and the rate of interest.” (*id.*: 8). Hayek thus stresses that “as a first explanation of the rate of interest, the consideration of such an imaginary state of ultimate equilibrium may have certain advantages” (*id.*) because in long-run stationary equilibrium the selling prices and the cost of production of the different commodities coincide, and hence “the description of capital in terms of an aggregate of value is sufficient.” (*id.*). It seems then that, in Hayek’s view, as in long-run equilibrium selling prices and minimum unit costs coincide and, moreover, the form and quantity of the capital goods are fully adjusted to their forthcoming demand, then only under these two conditions it is legitimate to assume a *one-to-one correspondence* between the equilibrium physical vector of the capital stock and the quantity of value that this stock represents. Finally, Hayek adds that

Even for the purposes of what is sometimes called ‘comparative statics’, that is the comparison of alternative states of stationary equilibrium, *it is still possible* to assume that the two magnitudes [i.e. selling prices and unit costs] move in step with each other from one position of equilibrium to another, so that it *never* becomes necessary to distinguish between them” (PTC: 8, emphasis added).

While Hayek thus believes that under the realms of the stationary state -and also for the purpose of comparing different stationary equilibria- the assumption of a given quantity of capital measured as an amount of value may be accepted as a first rough approximation to explain the rate of interest on the same footing as wages and rents, he claims on the other hand that “The problem takes on a different complexion, however, as soon as we ask how a state of stationary equilibrium can ever be brought about, or what will be the reaction of a given system to an unforeseen change.” (*id.*), because when stationarity is not assumed,

It is then no longer possible to treat the different aspects of capital as one, and it becomes *evident* that the ‘quantity of capital’ as a value magnitude is not a datum, *but only a result*, of the equilibrating process. (PTC: 8, emphasis added)

In other words, Hayek maintains that, outside stationary states, i.e. when prices and cost are not necessarily equal, and hence when the forms and quantities of

the capital goods are not yet adjusted to the conditions of stationary equilibrium, the assumption of a given quantity of capital in value terms *is no longer valid*. It becomes “evident”, Hayek maintains, that the equilibrium quantity of capital in value terms can be determined only *after*, but never before, equilibrium prices and distribution are known.

1.1.2. The demand for capital.

As to the negative interest-elasticity of the demand for capital, Hayek appears to believe that it is the attempt to model an economy that repeats itself identically period by period (or that finds itself in the equivalently unrealistic situation of steady growth) that seems to have prompted his predecessors to treat the capital stock as a *single composite commodity*, i.e. to reason *as if* “additions to the stock of capital always mean additions of new items similar to those already in existence, or that an increase of capital normally takes the form of a simple multiplication of the instruments used before, and that consequently every addition is complete in itself and independent of what existed previously.” (*id.*: 10). So, Hayek’s argument goes, by treating the entire capital stock as a single composite commodity whose quantity increases with accumulation it was ‘natural’, so to speak, that traditional neoclassical scholars reached the conclusion that the demand for this composite commodity decreases with its price, i.e. the rate of interest.

This treatment of capital *as if* it consisted of a single sort of instrument or a collection of certain kinds of instruments in fixed proportions – a treatment which has won favour from the fact that it has sometimes been used explicitly as a supposed simplification - is perhaps more than anything else responsible for the idea that capital may be regarded as a simple, physically determined quantity, and that the rate of interest may be explained as a simple (decreasing) function of this quantity. It would of course follow from these assumptions that the rate of interest must steadily and continuously fall in the course of economic progress since every addition to the stock of capital would tend to lower it. (PTC: 10-11, emphasis added)

1.1.3. Static or secular stationary equilibria? Hayek’s assessment of the stationary assumption in traditional neoclassical theory.

While Hayek affirms that within the realms of the stationary equilibrium the treatment of capital as a single factor can be somehow accepted as a legitimate simplification, in any case, Hayek continues, stationary conditions are irrelevant

to understand the working of actual economies, which are far from being stationary.

Perhaps the irrelevance of the stationary equilibrium construction for the treatment of capital problems comes out most clearly when we remember that this fictitious state could not conceivably be brought about at any given moment in society as it exists, but could be reached only after the lapse of a very long time. (PTC: 16)

Hayek thus dismisses the importance of this “fictitious” state because conditions of strict stationariness can give no indication of the prices and the distribution of income ruling in actual economies, which are far from being stationary. Now, although in PTC Hayek never explicitly distinguishes between static and secular equilibria, the fact that in his perusal of traditional theory Hayek charges his predecessors with taking as given a value-quantity of capital to explain interest under stationary conditions, could at first sight suggest that he actually refers to a static stationary state. This presumption is misleading though. Indeed, when Hayek studies the long-run determinants of the rate of interest (*cf.* chapters XVII and XVIII of PTC), he says that in stationary equilibrium the quantity of capital will correspond to the one that will *induce* the constancy of savings, and therefore, he explicitly refers to a secular stationary state^[53]. It is precisely because Hayek misidentifies static with secular equilibria that we find him arguing that a stationary state will be reached after a very long-period of time only and, hence, that it is an “irrelevant” concept to understand the working of actual economies^[54]. Moreover, it is no doubt due to this misunderstanding that Hayek argues that only in a secular equilibrium are selling prices equal to minimum-average costs.

⁵³ E.g. in chapter XVII he asks, “What is the relative importance of the productivity element and the psychological attitude respectively in determining the rate of interest while the process of saving continues? The second is: At what point will that process come to an end, and on what will the rate of interest depend in that final stationary state ?” (PTC: 227).

⁵⁴ Two things are worth stressing on this issue. First that it is quite odd that Hayek, just like Hicks in *Value and Capital* (*cf.* chapter 4), does not take into account the distinction between secular and static stationary state put forward by Robbins (1930). Second, that in the secular stationary state there is no need to take as given a quantity of capital in value, since this magnitude also becomes an endogenous variable. However, it would seem that Hayek reasons as follows: suppose the economy is in secular stationary equilibrium, to this situation there corresponds a certain amount of value capital, assuming this magnitude to be measurable; then, given that the economy is in a secular equilibrium, if we replace this condition by the given quantity of capital that would result in this secular equilibrium, the equilibrium thus determined will be the same (*Cf.* Petri, 2004: 120, fn. 4)

1.2. Market Nash Equilibrium in PTC.

Hayek proceeds to develop an equilibrium concept that does not rely on the “useless” (*id.*: 22) assumption of the stationary state, and therefore, that can be used to analyse non-stationary conditions as well. He explains:

What we need is a theory which helps us to explain the interrelations between the actions of different members of the community during the period... before the material structure of productive equipment has been brought to a state which will make an unchanging, self-repeating process possible. (PTC: 17)

No doubt, a MNE, i.e. a situation such that the plans of the individuals are fully adjusted to one another and hence mutually implementable, satisfies these requisites. Hayek thus declares:

The general idea of equilibrium, of which the stationary state is merely a particular instance, refers to a certain type of relationship between the plans of different members of a society. It refers, that is, to the case where these plans are fully adjusted to one another, so that it is possible for all of them to be carried out because the plans of anyone member are based on the expectation of such actions on the part of the other members as are contained in the plans which those others are making at the same time. (PTC: 18)

Hayek subsequently shows how the notion of MNE may be also applied to *non-stationary* conditions: in a sequence economy, the mutual consistency of plans is to be fulfilled “not because people just continue to do what they have been doing in the past, but because they correctly foresee what changes will occur in the actions of others” (*id.*: 18). Hence, to be fully adjusted over several periods, agents must be able to accurately predict future economic conditions; Hayek thus develops the notion of *perfect foresight temporary equilibrium*^[55], and he argues that, although this concept describes a purely “fictitious” (*id.*) state, it is anyway an “indispensable” notion (*id.*) to study economic phenomena outside the realms of stationariness: in his view this equilibrium notion can be interpreted as a “kind of foil” (*id.*: 23) or reference point to be compared against actual economic states.

It seems natural to begin by constructing, as an intellectual tool, a fictitious state under which these plans are in complete correspondence without, however, asking whether this state will ever, or can ever, come about. For it is only by contrast with this imaginary state, which serves as a *kind of foil*... It serves as a *kind of standard case* by reference to which we are able to judge what to expect in any concrete situation. (PTC: 22-24, emphasis added)

⁵⁵ The concept of equilibrium developed by Hayek is not strictly an intertemporal equilibrium because, given that the author does not assume the existence of complete future markets, future prices are only expected prices.

In principle therefore, this equilibrium notion would have a purely *negative* usefulness: an actual economic situation will *not be* an equilibrium state as long as the ideal conditions under which perfect foresight equilibria exist are not satisfied in reality^[56].

However, we may at this juncture note a first slip in Hayek's attempt to defend the new concept of equilibrium against the old, traditional, one: while on the one hand Hayek adduces that the traditional notion of equilibrium is an unrealistic construction, and that is why he attempts to develop a new notion of equilibrium, on the other hand we now find him openly admitting that the new notion is at least as unrealistic as the old one: like secular equilibrium conditions, a perfect foresight equilibrium describes a purely "fictitious" and "imaginary" state (*id.*) that may never be realized under real economic conditions. Anyway, at this stage of his exposition Hayek betrays a second and more relevant contradiction. He in fact declares that the "justification" for the new notion of equilibrium

[i]s not that it allows us to explain why real conditions should ever in any degree approximate towards a state of equilibrium, but that *observation shows that they do to some extent so approximate* (PTC:27-28, emphasis added)

In short, Hayek suddenly appears to endow the notion of MNE with explanatory-descriptive value^[57]. (We will provide further evidence of this in section 1.4. below). But his claims are liable to objection: Hayek fails to grasp that some of the data that determine the perfect foresight temporary equilibrium, the vectorial endowment of capital goods, lack of the necessary persistence to plausibly justify that the equilibrium determined on its basis can

⁵⁶ As we shall see in the final chapter (chapter 8), this position is also shared and by F. Hahn.

⁵⁷ Actually, this attempt is not surprising since Hayek appears to be well aware that the usefulness of a notion of equilibrium that may never be actually realised is very limited in scopes. He for instance accepts that the justification of the tendency towards equilibrium "is of course the ultimate goal of all economic analysis, and equilibrium analysis is *significant* only in so far as it is preparatory to this main task" (PTC: 17, emphasis added). And as he observes several years later, "The problem [of determining equilibrium prices] is thus in no way solved if we can show that all the facts, if they were known to a single mind (as we hypothetically assume them to be given to the observing economist), would uniquely determine the solution; instead we must show how a solution is produced by the interactions of people each of whom possesses only partial knowledge" (Hayek, 1945: 530). In this sense, in her review of PTC Colonna (1990: 64) has argued: "Hayek's theory is strongly based on the assumption that, whatever the disturbing factors may be, in a free market economy the inherent tendency towards equilibrium finally will prevail, or at least is always at work".

be “approximately observed” in actual economies. As argued in the first chapter, the capital stock of the economy at any given period is bound to change considerably within a very-short interval of time; hence, since actual productions and transactions decisions are time-consuming and take place through a process of experimentation and trial and error, much before equilibrium has been established, the position of equilibrium itself would have changed considerably. Nothing thus excludes the possibility of mistakes that accumulate period by period and that therefore push actual magnitudes further away from the trend predicted by the theory. So, when the correction of possible ‘coordination failures’ does not operate instantaneously as the notion of perfect foresight temporary equilibrium requires, the pretention to endow this concept with explanatory-predictive value must be abandoned. And in fact, Hayek’s contentions are surprising, if for no other reason, because in one of his interventions in the Socialist Calculation Debate he himself admits that the process of equilibration may take considerable time; e.g. Hayek (1940: 139) explains that the cost minimizing methods of production are not known in advance by the entrepreneur, but must be *discovered* by him through a trial and error process of experimentation in the market.

1.3. The secular equilibrium in PTC

We must now turn to consider that, at any rate, Hayek does not deny the “transient” feature of the equilibrium that is to be established on the basis of a given endowment vector of capital goods. He writes:

[a]t any moment much of the capital equipment of society exists in a form which cannot or will not be reproduced. And any ‘equilibrium’ that will ever be reached is necessarily transient and limited to the life of the ‘wasting assets’ which constitute part of the ‘data’ of that equilibrium. (PTC: 265)

Note that in this passage Hayek openly admits that the mutual consistency of plans cannot prevent profit-maximizing firms from changing their initial capital stock, evidently because the persistent force of competition ensures that capital goods will eventually adjust to their demand forthcoming, and this adjustment process will make these capital goods yield a uniform return on their supply price in the long-run. This is why a few pages earlier Hayek noticed that, after some exogenous change in the underlying conditions, “sooner or later”

[s]ome entrepreneur will find that in consequence of this change in circumstances it is no longer profitable either to invest the same amount in equipment of the kind he already possesses or to invest anything at all in equipment of that same kind. Wherever under the changed conditions the return obtainable from a given type of equipment, which he can buy at a price which only covers the prime cost of its production, is lower than the returns which could be obtained elsewhere, the owner of the existing equipment of this sort will find it in his interest to reinvest in capital goods of a different sort. And even where at first the existence in an earlier stage of the process of equipment which will be used, provided mere operating costs are covered, enables him to replace his own equipment at a price which covers little more than these operating costs, the situation will change as soon as that equipment in the earlier stage is worn out. Finally, the successive reinvestment, in different and to some extent new types of capital goods, of funds which it is no longer profitable to use for the replacement of equipment similar to that from the amortisation of which these funds have been obtained, will gradually lead to the building up of a new and different investment structure. (PTC: 263)

And he explains that

The process by which such a system will tend towards a final equilibrium after any sort of change, will evidently be very slow and gradual. For at any stage part of the data, namely, the character and composition of the equipment in existence will be the result of an historical process which, from the point of view of present decisions, must be regarded as an historical accident which will never be repeated in identical form. And since the equipment which will exist at the end of any period will to some extent be influenced by the composition of the equipment which happened to exist at the beginning of the period, what we really have to deal with is a process of continuous change. Although the change may, if the external data (tastes, knowledge, and supply of permanent resources) remained the same, continuously decrease in magnitude, it will probably never cease entirely, so that the system will make only an asymptotic approach to the position of an ideal final equilibrium. (PTC: 263-264)

These passages reveal that Hayek is forced to accept that, since the datum relative to the physical capital goods will change from period to period, the equilibrium that is determined on the basis of this datum -“an historical accident”, in his own words- will change considerably until new stationary conditions are eventually reached. In other words, in Hayek’s view, if not disturbed, the sequence of temporary equilibrium prices will *asymptotically* reach a position where capital goods’ supply prices yield a uniform return, a situation which Hayek wrongly identifies with a secular equilibrium. Yet, the claim is not surprising, since in a previous article, Hayek (1937: 49) already argued that “*experience shows*” that there is a tendency of prices to correspond to their costs of production, and also that this empirical observation “was the beginning of our science” (*id.*).

So, Hayek’s admission that “any ‘equilibrium’ that will ever be reached is necessarily transient and limited to the life of the ‘wasting assets’ ” (*id.*:265), forces him, so to speak, to reintroduce into the analysis the idea of a position of

rest where the endowments of capital goods are fully adjusted to their demand forthcoming and the rate of return of the capital stock is uniform. Moreover, considering that Hayek claims that there is a process by which such a “system would tend towards a final equilibrium”, it might be additionally noticed that Hayek envisages it as a kind of centre of gravitation of actual economic states.

In view of the previous considerations, it seems to be sufficiently clear that both MNE and secular equilibrium *coexist* in PTC. And behind this coexistence there seems to emerge the following reason. By resorting to the notion of MNE, Hayek, like Walras, avoids the value specification of the factor capital in the general equilibrium equations by including a vector of heterogeneous capital goods among the data. But given that Hayek is clearer than Walras on the fact that a uniform return on the capital goods’ supply prices requires an endogenous determination of the capital stock, he acknowledges that MNE prices will generally have a *transitory* character. In other words, we have seen that Hayek does not deny that the arbitrarily given initial composition of the capital stock will change until the return on the supply price of the capital goods becomes uniform, a situation that Hayek wrongly identifies with a secular equilibrium. Then, the coexistence of both Market Nash and secular equilibria allows Hayek to ‘circumvent’ the problem of the ‘quantity of capital’ from its supply side, while simultaneously keeping the traditional role assigned to equilibrium within the analysis, i.e. a position that the economy tends to realize over sufficient time. However, an obvious flaw of this framework is that the position where the economy tends is identified with a secular equilibrium, whose relevance for the studying of actual economies is highly questionable, as Hayek himself recognizes.

In order better to grasp the importance of the notion of secular equilibrium in Hayek’s construction we must consider that in PTC, considered by *itself*, the sequence of MNE prices does not contemplate any notion of *normal* price that is determined *independently* of the sequence itself, and around which this sequence will gravitate in the long-run. The assumption that MNE prices will eventually reach a secular equilibrium attempts to fill this gap: the secular equilibrium allows Hayek to reach plausible results and to avoid indeterminate

outcomes, since this equilibrium provides the *direction of change* of MNE prices. This suggests that, in PTC, the role assigned to the new MNE concept of equilibrium is only to approximately describe the actual behaviour of the economy between secular equilibrium states, what Gehrke (2003: 104) calls “transitional dynamics” between secular stationary states. Inspection of part III of PTC seems to support this view, since Hayek argues that

The significance of the conditions of stationary equilibrium of capitalistic production which we have discussed in the last chapter will become clearer only as we apply the same considerations to the study of the effects of different types of changes. In the present and the succeeding chapters we shall therefore discuss the readjustments that will be made necessary if, after various types of changes in the data, equilibrium is to be re-established. (PTC: 268)[⁵⁸]

Incidentally, note that here ‘equilibrium’ and secular equilibrium states are identified as one and the same thing, i.e. the temporary equilibrium that is assumed to come about within the single analytical period is not really considered by Hayek as a *proper* equilibrium state.

We may summarize the foregoing discussion in the following way: although Hayek harshly criticises the concept of stationary equilibrium, it would seem that in PTC this equilibrium concept still bears a higher theoretical hierarchical position than the new, MNE notion: given that Hayek admits that perfect-foresight-temporary-equilibrium relative prices are determined on the basis of “accidental data” (i.e. the endowments of capital goods), and are “transient” in nature, their absolute level is essentially *irrelevant*, and their direction of change is provided by secular equilibrium prices, i.e. those prices that in PTC are assumed to yield a uniform return on the supply price of capital goods [⁵⁹][⁶⁰].

⁵⁸ In section 2.2 below I examine in some detail how in chapters XX and XXI Hayek deals with one of these changes that are originated in the ‘supply of capital’: the effect of accumulation –i.e. of positive net savings- on relative prices.

⁵⁹ As we shall see in part of the chapter, the assumed tendency towards stationariness is not without problems: Hayek attempts to justify it by relying on traditional neoclassical premises that stand or fall with the notion of capital as a single factor of production –i.e. the higher the amount of investment, the lower its marginal product – and that Hayek so firmly rejects.

⁶⁰ It is therefore not surprising that Lutz (1967: 56-67), in his documentation of the neoclassical theories of the rate of interest, decided to include Hayek’s PTC along with both Wicksell’s and Böhm Bawerk’s works in the group of theories that dealt with stationary economies. And that is why I can not share Steedman’s view, who in his perusal of Hayek’s PTC has argued that Hayek

1.4. The notion of MNE as a tool for description of actual economic states: an example.

To conclude this first part, a brief excursion to part IV of PTC may bring further support to the claim (advanced in section 1.2) that Hayek endows the sequence of Market Nash Equilibria with explanatory-descriptive value^[61]. There, the author first explains how money can be introduced in his equilibrium framework to subsequently study the negative effects caused by the ‘misguided’ intervention of the monetary authority. To introduce money in the new equilibrium framework, Hayek notes that a “slight” modification is necessary because individuals’ desire for money holdings are incompatible with the notion of perfect-foresight equilibrium developed in the previous parts of PTC. He writes:

[t]he desire of people to hold money cannot readily be fitted into the rigid definition of equilibrium we have used up to this point. At least, in an economy in which people were absolutely certain about the future, there would be no need to hold any money beyond the comparatively small quantities necessitated by the discontinuity of transactions and the inconvenience and cost of investing such small amounts for very-short periods (PTC: 357).

Then, to get a closer approximation to reality, Hayek abandons the assumption of perfect foresight equilibria over the whole economy’s life-span and assumes, instead, that agents may have mistaken expectations about future events. In other words, Hayek seems to *de facto* switch to a mixed intertemporal-temporary equilibrium framework. He explains that, under the new assumptions, “the plans of the various individuals may be compatible to the extent to which they are definite, and yet the individuals may at the same time be uncertain about what will happen after a certain date, and may wish to keep some general reserve against whatever may happen in that more uncertain future” (*id.*). Uncertainty and mistaken expectations provide some justification for the introduction of money into equilibrium analysis; this in turn allows Hayek to tackle, under a more realistic setting, the *concrete* issue of the Central Bank’s monetary policy. For our purposes, it is important to note that, in any case, Hayek stresses that

“of course, is not much concerned with the stationary equilibrium, at least in any positive sense” (Steedman, 1994: 521).

⁶¹ Cf. Gehrke (1996) for a more detailed and comprehensive analysis of the monetary analysis in PTC.

[t]he assumption of certainty about the more distant future, although we have so far based our argument on it, *is not really essential for our concept of equilibrium* (PTC: 357, emphasis added).

In short, in part IV Hayek definitely contradicts his original claims: he relaxes the assumption of perfect foresight - which as now Hayek himself stresses, “*is not really essential for our concept of equilibrium*”- and uses the new MNE concept as a tool for dealing with actual economic problems. For instance, he examines both the short and long-run effects of a decrease in the rate of interest on money loans. Hayek argues that, by changing the monetary rate of interest, the Central Bank can alter the structure of production only *temporarily*, since

[t]hese monetary influences on the rates of interest will set up forces which will work in a direction opposite to their immediate effect through interest rates. Thus in the short run money may prevent real changes from showing their effect, and may even cause real changes for which there is no justification in the underlying real position. In the long run, however, it will always merely accentuate the change it has at first prevented, or will bring about changes which are the opposite of the impact effects. (PTC: 407)

Hayek reaches the traditional neoclassical conclusion that monetary authorities should not try to ‘artificially’ stimulate economic activity by decreasing actual rates, because this will only increase the price level in the long run^[62] ^[63]. These long-run effects, he additionally observes, may take considerable time to assert themselves fully (PTC: 377); in fact, Hayek eventually admits in this connection the possibility of a “temporary equilibrium” with unused resources (*id.*: 360). However, he is careful to stress that full-employment is the unique possible outcome over sufficient time. In this sense, he argues that, while unused resources may “occasionally prevail in the depths of a depression” (*id.*: 373), i.e. in the short-run, he adds that “it is certainly not a *normal* position on which a theory claiming general applicability could be based” (*id.*, emphasis added) since situations such that unused resources exist are only “transitory” (*id.*:377)^[64]. So, in part IV of PTC Hayek accommodates the framework developed in the first three parts of the book to admit both uncertainty about

⁶² This “long run” notion of equilibrium will be commented below, like the normal position to appear shortly.

⁶³ Hayek’s description of the inflationary pressures and the “forced savings” triggered by the Central Bank’s decision to lower the rate on money loans is summarized e.g. in Roncaglia (2005: 319-320), Garrison (1994) and Colonna (1990: 57-65); a critical account of Hayek’s explanation is to be found in Milgate (1988) and Kurz (2000).

⁶⁴ The reason, to be explored in section 2 below, is that in PTC the interest rate still plays its traditional role, namely, that of accommodating investment decisions with full-employment savings.

future events, and also a transitory situation of unemployment. If, Hayek seems to argue, with respect to the perfect foresight equilibrium, the imperfect foresight temporary equilibrium gives a closer approximation to reality *without* substantially modifying the core notion of equilibrium developed in the first parts of PTC (recall once more that he explains that the assumption of correct foresight *is not* essential for the new notion of equilibrium developed in PTC), then, for short-run situations, the temporary equilibrium with unused resources is a further approximation of that approximation.

To put it in a nutshell, behind the claim that “observation shows that [actual conditions] do to some extent approximate” to a perfect-foresight-temporary equilibrium state (PTC: 27-28), there seems to emerge the following reason: in spite of the fact that, contrarily to what this notion of equilibrium presupposes, individuals do not have correct foresight over the indefinite future and, moreover, in actual economies resources can be temporarily unemployed, given that the supply-and-demand forces would be constantly at work, and would push the economy towards full-employment after sufficient time anyway, the perfect-foresight-equilibrium path, so Hayek seems to argue, does reasonably capture the action of these supply-and-demand forces, namely, it can approximately describe the direction of change of actual magnitudes over time, as for instance occurs along the ‘traverse’, i.e. the transition stage between stationary equilibria^[65].

Little wonder then that Hayek finishes PTC by charging Keynes with exclusively focusing on those short-run circumstances^[66], “the surface phenomena” as Hayek calls them, and hence with disregarding the main problem, namely the studying of the consequences of the monetary policy under ‘normal’ full-employment conditions.

⁶⁵ In the second part of the chapter we shall see that Hayek explains the direction of change of temporary equilibrium prices and quantities by assuming that techniques can be ordered according to their degree of ‘roundaboutness’ independently of distribution, and hence illegitimately taking for granted the traditional notion of capital from its demand side.

⁶⁶ It is not here the place to discuss how accurate Hayek’s contentions about Keynes are. On the issue of Keynes and the long-period method or centre-of-gravitation notion of equilibrium, however, the reader is referred to Milgate (1982). For a different position *cf.* Bhattacharjea (1987) and Cardim de Carvalho (1990)

I cannot help regarding the increasing concentration on short-run effects - which in this context amounts to the same thing as a concentration on purely monetary factors - not only as a serious and dangerous intellectual error, but as a betrayal of the main duty of the economist and a grave menace to our civilisation. To the understanding of the forces which determine the day-to-day changes of business, the economist has probably little to contribute that the man of affairs does not know better. It used, however, to be regarded as the duty and the privilege of the economist to study and to stress the long [run] effects which are apt to be hidden to the untrained eye, and to leave the concern about the more immediate effects to the practical man, who in any event would see only the latter and nothing else. (PTC: 409).

It is not surprising that Mr. Keynes finds his views anticipated by the mercantilist writers and gifted amateurs: concern with the surface phenomena has always marked the first stage of the scientific approach to our subject. But it is alarming to see that after we have once gone through the process of developing a systematic account of those forces which in the long run determine prices and production, we are now called upon to scrap it, in order to replace it by the short-sighted philosophy of the business man raised to the dignity of a science. (PTC: 410)

A final and important observation is worth making: with his distinction between “transitory” or “short-run” unemployment on the one hand, and “normal” or “long-run” positions of full-employment on the other hand, Hayek’s approach ends up being fully traditional. As e.g. Marshall argued before him, in these and the previous passages quoted in this section, the “long run” equilibrium or “normal position on which a theory claiming general applicability could be based” is *no longer* identified with a secular equilibrium state, but with a position that attempts to capture the action of those *persistent* forces that “in the long run determine prices and production”, and whose action emerges through the multitude of accidental circumstances that day-by-day affect the behaviour of the variables under study, manifesting their effects fully only after a sufficient interval of time.

2. THE ROUNDABOUTNESS OF PRODUCTION METHODS AND THE TRADITIONAL CONCEPTION OF CAPITAL

The second part of this chapter argues that Hayek still shares with his predecessors –i.e. Jevons, Böhm Bawerk and Wicksell- the idea that more capital-intensive or ‘roundabout’ methods of production are to be adopted if the interest rate decreases, and from this he derives the negative interest-elasticity of investment. Hayek seems to rely on the Austrian notion of roundaboutness for two main reasons. First, given that, as shown in the first part, he uses the notion of MNE to study transitional dynamics between secular equilibrium states, then the choice of techniques along Austrian lines provides the *direction*

of change of Market-Nash prices and quantities toward their very long-run position of rest. In other words, in Hayek's framework the neoclassical factor substitution mechanisms are assumed to work *over the sequence* of (perfect foresight) temporary equilibria. Second, in PTC the interest rate is still envisaged as the variable that must bring into equilibrium investment decisions and full-employment savings; Hayek must therefore face the issue of the stability of that market. I will conclude that, although Hayek attempts to eliminate the notion of capital as a single factor by treating each capital good as a distinct factor of production whose quantity is given when economic activity starts, his explanation of the adjustment process between secular equilibria relies on traditional neoclassical gravitational ways of reasoning that presume that techniques can be ordered according to their degree of 'roundaboutness', and hence (having criticized and abandoned the average period of production (*cf. e.g. PTC: 76, 142*)) it takes for granted the traditional conception of capital as a single scalar in its 'demand side' role.

2.1. The analysis of technical choices.

Hayek examines the determinants of optimal technical choices under several cases^[67] ^[68]. Hayek first analyses the "point input-point output" (Pi-Po) case: a non-produced input, e.g. labour, invested at a given date matures into a consumption good at a later date without in the meantime requiring any cooperation from other units of labour. The final output is produced under a well-behaved, constant-returns-to-scale production function $f(L,t)$, where L is the amount of input used and t is the period of time (endogenously determined) that elapses between the application of the non-produced input and the moment when the final output comes out. As Hayek (PTC: 151, 181) explains, this case represents the growing of trees or the maturing of wine.

⁶⁷ Hayek's analysis of technical choices begins with his more or less well known "input" and "output" functions. These curves however are not relevant for my purposes, and this is why they are not discussed here. The reader is referred to Hawtrey (1941), Lutz (1943) and Fabbrini (1951) for an analysis of Hayek's input and output curves.

⁶⁸ Along part II of the book, Hayek assumes the existence of a "dictator" that aims to maximize individuals' income stream. This assumption is subsequently relaxed in part III, and replaced by the hypothesis that there is a competitive economy such that each entrepreneur maximizes profits

A second representation of the production process is the “flow input-point output” (Fi-Po), according to which a flow of non-produced inputs over time produces a final consumption good at a later point in time. This is the case I intend to discuss here in some detail^[69]. Non-produced inputs, e.g. units of labour and land, are applied at all moments from the instant t_1 , when the first units of input are applied, to the instant t_n , when the last units of input are employed. We divide the production process into n different intervals or periods of equal length, i.e. t_1-t_2 (that means from t_1 to t_2), t_2-t_3 , ..., $t_{n-1}-t_n$, t_n-t_{n+1} , and assume that the final good comes out at the end of the interval t_n-t_{n+1} . Each interval is called a “stage”; in each stage -except the first one, where the capital goods (all circulating) that will be used in the following stage are produced by labour and land alone- non-produced units of input cooperate with the capital goods produced in the previous stage to elaborate the capital goods that will be used in the following stage until the final product comes out. In short, Hayek associates each stage with certain kinds of capital goods:

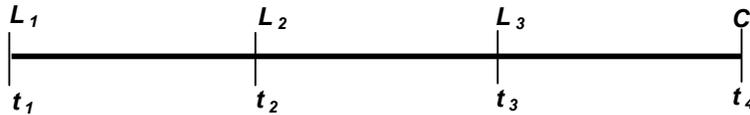
[t]he process leading up to anyone product is broken up into distinct ‘stages’...[which] can be conveniently used for a grouping of the various kinds of capital goods according to their remoteness from ultimate consumption. (PTC: 73-74)

Unlike the Pi-Po case, where “the intermediate products that emerge in different stages of the process are similar in character to the final product” (PTC: 181), e.g. wood produced by trees, in the Fi-Po representation of the production process the intermediate goods produced in each stage are qualitatively different from each other and also from the final output. Moreover, as we shall argue below (section 2.3), Hayek admits that each technique requires different capital goods.

Before proceeding with the analysis further, some qualifications are in order. The first qualification has been anticipated above: I will assume that the successive stages of production are separated by *discrete* intervals of time of *equal length*: t_i-t_{i+1} , $i=1, \dots, n$. At the beginning of the first $n-1$ intervals, i.e. t_1, t_2, \dots, t_{n-1} , different units of labour and land are applied to produce circulating

⁶⁹ Hayek also discusses the point input-flow output and the flow input- flow output cases. The discussion of these cases will be omitted here because they are not relevant for my purposes, namely to show that for long period technical choices, Hayek still relies on the traditional conception of capital from its demand side.

capital goods that will come out at the end of the interval, and are employed in the following stage. At the beginning of the last interval, i.e. instant t_n , labour and land are employed jointly with the capital goods produced in the previous stage to produce the final good C . For instance, for a three-period process that requires only labour as a non-produced input, we have:



Here, labour units are applied at instants t_1 , t_2 and t_3 . At instant t_1 the quantity L_1 is used to produce the capital goods that will come out at the end of the first interval, t_1-t_2 . At instant t_2 , L_2 units of labour are employed; these units cooperate with the capital goods elaborated in the previous stage to produce the capital goods that, in the following stage (interval t_3-t_4), will cooperate with the L_3 units of labour, applied at the beginning of this last interval (instant t_3) to produce the consumption good C . The consumption good comes out at the end of the interval t_3-t_4 . Following Hayek (PTC: 69, *cf.* also Lutz, 1951: 91-92), we define the “investment period” of each unit of input as the interval of time that elapses between the application of that unit of input and the emergence of the final output C [⁷⁰]. I.e., the investment period of the L_1 units of labour is equal to three periods, from t_1 to t_4 , the investment period of the L_2 units of labour is equal to the interval t_2-t_4 , and so on. In short, the investment period of those units of inputs applied at the earlier stages of production is higher than the investment period of those units of inputs applied at the later stages.

This first qualification appears necessary because Hayek sometimes assumes that labour is applied *without* discontinuities along the production process (PTC: 117). However, for practical purposes Hayek assumes discrete stages by dividing the process in intervals (PCT: 122); this is particularly clear when he discusses the effects of accumulation on relative prices (part III of PTC, chapters

⁷⁰ “The fundamental fact with which we are concerned is the change in the periods for which particular units of input are invested, that is, in the interval between the application of a unit of input and the maturing of the quantity of output [i.e. the final consumption good] due to that input. This interval of time we shall describe as the *investment period* of that unit of input.” (PTC: 69).

XX and XXI): as it is examined in section 2.2. below, when Hayek analyses how changes in the rate of interest affect the distribution of inputs employed in the different stages of production, he assumes that there is a finite number of stages of equal length, and that the non-produced inputs are applied discontinuously at precise instants of time, i.e. at the beginning of each stage (PTC: 288). And anyway, Hayek does not seem to view the assumption of labour units applied at precise instants for discrete intervals of time as altering his conclusions (relative to the case of continuous input flow).

The second qualification is that the Fi-Po representation of the production process can in turn be divided into two sub-cases (*cf.* Lutz, 1951, 87-91): the first case assumes that the various techniques among which the firm can choose to produce the final output cover the same sequence of intervals: $t_i - t_{i+1}$ $i=1, \dots, n$ are exogenously given, and hence the ‘absolute investment period’, i.e. the whole interval $T=t_1 - t_{n+1}$, is common to all alternative techniques: only the amounts of the different kinds of non-produced inputs to be employed at each stage change from technique to technique, with –Hayek generally assumes– smooth substitutability. Under the second case it is instead assumed that the entrepreneur can also decide the optimal T . While Hayek deals with both sub-cases, when he comes to part III of PTC (in particular when he proceeds to examine how changes in the interest rate affect the direction of change of optimal technical choices) he assumes that the absolute investment period is exogenously fixed^[71]. So here I will discuss this first case only.

The third remark is that Hayek eventually points out that in the Fi-Po case, each process uses different capital goods, i.e. that techniques are capital-good specific^[72] (As I argue in section 2.3 below, an important implication of this is that Hayek’s analysis of long-period technical choices operates over the sequence of temporary equilibria, and that the traditional notion of capital as a single factor emerges as a flow, in the savings-investment market). Having said that, it is also true that Hayek believes that, when the capital goods are ‘there’,

⁷¹ In any case, as Lutz (1951: 91) remarks, according to this framework, the effect of changes in the interest rate on the time distribution of dated-inputs is the same under both sub-cases: as we shall see, a decrease in the interest rate induces firms to increase the investment periods of some dated-inputs by transferring them from the later to the earlier stages of production.

⁷² Citations that confirm this claim will be provided in section 2.3 below.

so to speak, there are always possibilities of substitution among labour and capital goods 'ex post'. He writes for instance that:

Most kinds of input will however, be not specific, but more or less 'versatile', in the sense that they can be used in a great many if not in nearly all stages of production. (PTC: 251)

And he also adds later on that:

[c]omplete specificity of the old equipment... will occur only under very special and rather unlikely conditions (id.: 314).

This may explain why, although Hayek is clear that when cost-minimizing techniques change, the form of the existing capital goods will accordingly change, he usually seems to reason in ways that *only* make sense in a framework in which only those permanent resources (labour and land) are exogenously given, i.e. he usually assumes that the entrepreneur is free to use more labour of a certain date and less labour of another date denying or neglecting the fact that, because in his framework the initial endowments of capital goods are exogenously given, in the first periods of the economy's life-span there is nearly *no substitutability*; proceeding in this way, Hayek usually ignores the 'substitutability problem' (*cf.* chapter 1) and reasons *as if* there were always possibilities of substitution 'ex ante' among dated inputs, hence variable coefficients, even when there is a given vectorial endowment of capital goods, and he intends to develop Austrian analyses of these choices^[73]. In short, Hayek assumes variability of non-produced inputs even for half-way production processes, i.e. the fact that at a certain moment the three-period process described above has already reached t_3 –so a certain amount of L_1 and L_2 have already been chosen in the past and have produced certain specific capital goods- does not prevent a variability of L_3 . While this is in fact implicit in the

⁷³ This weakness of Hayek's analysis is for instance remarked by Hawtrey in his review of PTC: "But it is only in certain quite exceptional cases that it is possible to vary one constituent of the productive resources of an enterprise in isolation from the rest, and to cause a corresponding variation in the output. One of these exceptional cases (which Professor Hayek mentions -pp. 151 and 181) is the growth of timber left to itself for an additional year. Another would be an appliance introduced into a productive concern to reduce waste, and so to increase the effective output without modifying any other part of the productive organisation. But, apart from such special cases, that is not possible. In general all the different instruments used in a productive concern are employed on a succession of processes, each of which contributes to the final output, and the contribution of each must be just what is required for the final output, no more and no less. The outturn of any process cannot be varied independently of the rest, for, if it were, its contribution would merely become excessive or deficient" (Hawtrey, 1943: 286)

assumption of a smooth production function, i.e. variability of all input proportions, it seems relevant to point out a rather debatable implication of that assumption.

The final qualification I wish to make is that, given that in the initial period the vectorial endowment of capital goods is included among the data of the equilibrium, Hayek is aware that in the first periods of the economy's life-span, there will be price changes from one period to the next. He writes:

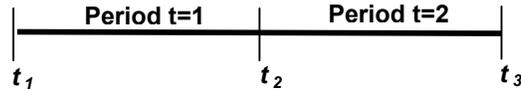
During the initial rearrangement of the resources certain substitutions of one kind of commodity for another at successive dates will be part of the plan. And the same will apply, although to a lesser and rapidly diminishing degree, to all later stages of the process of change. The reason is, of course, that we start out with an assortment of non-permanent resources, which is the result of a particular historical development, and which will consist in large part of items which it is either impossible or else unprofitable to reproduce. Since the form in which these resources exist at the beginning, and at every subsequent stage, will exert an influence on the kind of resources by which they will be replaced, and since the resources existing at every moment are determined by past conditions, we have to deal with a process of continuous change. Even if we could assume that at the initial moment, when [the entrepreneur] makes his new plan, he is in possession of complete knowledge of all future conditions, this plan would have to envisage an infinite series of changes. These changes would, it is true, rapidly decrease in magnitude as time went on, and would after a while become insignificant, but in principle they would continue in some small degree for ever. This means that we shall have to deal with a stream of different goods whose relative value will be constantly changing, at first, perhaps, quite considerable and even later to some extent. (PTC: 166-167)

(Note however that, given that Hayek thinks of an indefinite future that tends to stationariness, hence prices tend to long-period prices, he also argues that the change in relative prices will *rapidly* decrease in magnitude, and will become *insignificant* as time goes on. As I will argue in section 2.2. below, this suggests that Hayek must therefore explain the direction of change of technical choices conceding, above all, the tendency of prices to long-period prices).

In view of the previous remarks, a possible formalization of Hayek's view of the process of technical choices in a perfect-foresight, temporary-equilibrium framework may be the following^[74]: consider an entrepreneur who makes a two-period investment plan (for now we assume that the economy ends after the second period, but given that Hayek believes in a indefinite future that tends to

⁷⁴ I say "possible" because, alas, Hayek does not offer an explicit formalization of his results. A similar formalization of the Austrian process in an intertemporal framework is found in Fratini (2001)

stationariness, this assumption will be relaxed in the next section); the first period ($t=1$) starts at the instant t_1 and the second period ($t=2$) starts at the instant t_2 . (Note that the symbol t refers to a *period* of time, while the symbols t_i $i=1,2$ refer to an *instant* of time).



A certain quantity of consumption good C comes out at the end of each period t , and is produced by means of labour and land available in the current period (in what follows I will adopt Wicksell's jargon (*cf.* Wicksell, (1934: 150) and label these units of input as 'current labour' and 'current land'), and also by labour and land that have been 'saved', so to speak, in the previous period (from now on, 'saved-up labour' and 'saved-up land'), i.e. have been employed in period $t-1$ in the production of the capital goods that, in period t , cooperate with the units of current labour and current land to produce C . Below, the first index represents the period, the second index is 0 if the input is a 'current' one in that period, it is 1 if the input is a 'saved' one for one period, that is, was used in the previous period to produce capital goods to be then utilized in the given period (the Wicksellian representation is maintained because afterwards it will be used to study long-period choices). So, when economic activity starts at t_1 , the economy has the following endowments: $\bar{L}_{1,0}$, $\bar{T}_{1,0}$, $\bar{L}_{1,1}$, $\bar{T}_{1,1}$, $\bar{L}_{2,0}$, $\bar{T}_{2,0}$. $\bar{L}_{1,0}$ and $\bar{T}_{1,0}$ represent the quantities of current labour and land available at the beginning of period $t=1$, $\bar{L}_{2,0}$ and $\bar{T}_{2,0}$ represent the quantities of current labour and land available at the beginning of period $t=2$, and, finally, $\bar{L}_{1,1}$ and $\bar{T}_{1,1}$ are the quantities of labour and land saved in period $t=0$ (one period before the economy starts) and available at the beginning of the first period in the form of *specific* capital goods.

In each period t the consumption good C is produced under a well-behaved, constant-returns-to-scale production function: $C_t = f(L_{t,0}^d, L_{t,1}^d, T_{t,0}^d, T_{t,1}^d)$ [75]. C_t is

⁷⁵ As argued above, unless the period t of C_t is so located in the time horizon considered as to allow a free choice of all the saved inputs (i.e., in the present case, unless $t=2$), the assumption of differentiability of the production function is misleading because it hides the 'substitutability

the quantity of consumption good that comes out at the end of period t , $L_{t,0}^d$ is the amount of labour employed at the beginning of each period t (i.e. at instants t_1 and t_2) and saved for zero periods (i.e. current labour) and $L_{t,1}^d$ is the amount of saved-up labour employed at the beginning of each period t , i.e. what is actually employed is the capital goods resulting from its use in period $t-1$. The land coefficients, $T_{t,0}^d, T_{t,1}^d$, have the same interpretation.

Note that, given that besides the ‘current’ units of input, in each period firms also demand ‘saved-up’ units of labour and land, then period-1 ‘current’ units of labour and land can be employed in *two alternative ways*: first, they can be directly employed in the production of the consumption good that will come out at the end of the first period (C_1). Second, these units of non-produced inputs can be also saved, i.e. can be used to produce capital goods that will cooperate in the second period with $L_{2,0}^d$ and $T_{2,0}^d$ to produce the consumption good the will come out at the end of that period, i.e. C_2 . The implication is that in the first period the total demand for current labour is equal to $L_{1,0}^d + L_{2,1}^d$, and the total demand for current land is equal to $T_{1,0}^d + T_{2,1}^d$. On the contrary, if we provisionally assume that in the second period the economy ends, then the ‘current’ inputs available in the second period, i.e. $\bar{L}_{2,0}$ and $\bar{T}_{2,0}$, can be only employed in the production of C_2 (in other words, in the second period there is no production of capital goods).

The representative firm decides its production plan by maximizing the following profit function:

$$\begin{aligned}
 & p_1 f(L_{1,0}^d, L_{1,1}^d, T_{1,0}^d, T_{1,1}^d) + p_2 f(L_{2,0}^d, L_{2,1}^d, T_{2,0}^d, T_{2,1}^d) - w_{1,0} L_{1,0}^d - w_{1,1} L_{1,1}^d - r_{1,0} T_{1,0}^d - r_{1,1} T_{1,1}^d \\
 & - w_{2,0} L_{2,0}^d - w_{2,1} L_{2,1}^d - w_{2,0} \bar{L}_{2,0} - w_{2,1} \bar{L}_{2,1} - r_{2,0} T_{2,0}^d - r_{2,1} T_{2,1}^d - r_{2,0} \bar{T}_{2,0} - r_{2,1} \bar{T}_{2,1}
 \end{aligned} \tag{1}$$

problem’; in the present example the given $\bar{L}_{1,1}$ and $\bar{T}_{1,1}$ indicate in fact given endowments of capital goods which can no longer be altered, and hence, if one realistically admits that each technique requires specific capital goods (and not different proportions of the same capital goods), these impose a specific production method and hence rather rigid quantities of labour and land per unit of output, so there will be generally nearly no variability of the demand for current labour and land for the production of C_1 .

where p_1 is the price of C ruling in the first period, p_2 stands for the price of C ruling in the second period; $w_{1,0}$ and $r_{1,0}$ stand, respectively, for the wage rate and for the rate of land rent paid to current units of labour and land applied at t_1 ; $w_{1,1}$ and $r_{1,1}$ represent the wage rate and the rate of land rent paid to those units of labour and land that have been saved one period before the economy starts ($t=0$), and are employed at t_1 in the production of C_1 ; $w_{2,0}$ and $r_{2,0}$ represent the rate of wages and land rent paid to current units of labour and land employed at t_2 ; finally, $w_{2,1}$ and $r_{2,1}$ stand for the rate of wages and the rate of land rent paid to those units of labour and land that have been saved during the first period and are employed in the second period to produce C_2 .

All prices and rentals are *discounted* relative prices to the beginning of the initial date; as explained immediately below, the rate of discount is the *own rate of interest* of the numéraire. E.g. if the price of C in the first period is set as the numéraire, i.e. $p_1=1$, then p_2 indicates how much units of C an agent must pay at instant t_1 to obtain one unit of numéraire at the end of the second period. But this transaction is equivalent to a *loan operation*. In other words, if an agent lends p_2 units of C to a second individual, the latter will deliver his debt (one unit of C) to the former in the second period. Note then that in this operation a rate of interest (i) in terms of C is implied, and it can be determined by the following condition: $p_1 \equiv 1 = p_2(1+i)$. The rate of interest thus results of the ratio of two homogeneous quantities (commodity C) and is called the own-rate of interest of commodity C .

It follows from the previous remark that if, hypothetically, we consider a second commodity A , and P_2^A stands for the spot price of A in terms of C in the second period (i.e. A 's period-2 *undiscounted* relative price in terms of C), then the discounted price of A in the second period (p_2^A), i.e. the quantity of C that an agent must disburse in the first period to obtain one unit of A at the end of the second period, is equal to $\frac{P_2^A}{1+i}$. This is so because, in equilibrium, there must be *non-arbitrage* opportunities for savers. Therefore, for an agent it must be

equally convenient to lend one unit of C in the first period, get in return $1+i$ units of C in the second period and, finally, buy $\frac{1+i}{P_2^A}$ units of A that in the second period; or, alternatively, to directly exchange her unit of C in the first period for $\frac{1}{P_2^A}$ units of A to be delivered at the end of the second period. So

equilibrium requires that $\frac{1}{P_2^A} = \frac{1+i}{P_2^A}$, i.e. $P_2^A = \frac{P_2^A}{1+i}$. (As Hayek (PTC: 167-168)

notes (see fn. 76 below), the rate of interest will generally depend on the choice of the numéraire. If we define the own rate of interest of commodity A as $\frac{P_1^A}{P_2^A} \equiv 1+i^A$, then in a context of changing relative prices it will generally be the

case that $1+i = \frac{P_1}{P_2} \neq \frac{P_1^A}{P_2^A} = 1+i^A$. Only under stationary conditions (or steady growth) relative prices will not change from one period to the next, i.e., $\frac{P_1}{P_2} = \frac{P_1^A}{P_2^A}$, and hence $i = i^A$. Under changing relative prices, intertemporal

equilibrium only implies that there must be non-arbitrage opportunities for savers, and, as explained above, this condition is satisfied if all possible indirect exchanges bring the same result [76]).

⁷⁶ Hayek explains the dependence of the rate of interest on the numéraire by means of the following example. He says, "Let us consider two points of time of which the earlier one represents the date at which numerous investments are made and the later one the date when these investments mature. Let us assume further that the relative values of the different commodities are different at the two dates. If we now take anyone commodity in technical terms which is used at both dates and of which the quantity available at the later date can be increased at the expense of the quantity available at the earlier date, we shall have a definite quantitative rate of increase due to the 'investment' of that commodity. For another commodity we shall probably find a different rate of increase. Equilibrium (*i.e.* the most advantageous disposition over the resources) requires that these two rates of increase between the two dates shall stand in a definite relationship to the relative values of the two commodities at the two dates." (PTC: 167-168). So, he continues, "If we take such quantities of the two commodities as are of equal value at the first date, and make them increase by investment at their different individual rates [*i.e.* the own rates of interest], the quantities of the two commodities obtained at the second date must again be of equal value. This, of course, amounts to [*saying*] that in terms of anyone commodity (any 'numéraire') the rate of increase must be equal for all commodities. Although the quantitative ratio between the physical amount invested at the earlier date and the physical amount obtained at the later date may be different for different commodities, the value equivalents in terms of the 'numéraire' at the two dates must bear the same ratio to one another for all commodities". (*id.*: 168)

The first order conditions of the profit-maximizing problem of the firm are given by:

$$\begin{cases} p_1 f_{L_{1,0}} = w_{1,0} \\ p_1 f_{L_{1,1}} = w_{1,1} \\ p_1 f_{T_{1,0}} = r_{1,0} \\ p_1 f_{T_{1,1}} = r_{1,1} \end{cases} \quad \begin{cases} p_2 f_{L_{2,0}} = w_{2,0} \\ p_2 f_{L_{2,1}} = w_{2,1} \\ p_2 f_{T_{2,0}} = r_{2,0} \\ p_2 f_{T_{2,1}} = w_{2,1} \end{cases} \quad (2) \text{ [77]}$$

Where $f_{X_{i,q}}$ stands for the marginal product of input X employed at the beginning of the i^{th} period and saved-up for q periods (i.e. zero or one) of time. Conditions (2) state that, in equilibrium, the discounted value of the marginal product of labour and land (both ‘current’ and ‘saved-up’) should be equal to their respective rentals. But these conditions do not suffice to determine the optimal choices on the part of firms. Given that the endowments of period-1 current labour and land can be employed in two different ways, optimal choices on the part of firms require that:

$$\begin{cases} p_1 f_{L_{1,0}} = p_2 f_{L_{2,1}} \\ p_1 f_{T_{1,0}} = p_2 f_{T_{2,1}} \end{cases} \quad (3)$$

That is, the present value of the marginal product of those units of period-1 current labour that at t_1 are employed in the production of C_1 (i.e. $L_{1,0}^d$) must be equal to the present value of the marginal product of those units of period-1 current labour that in the first period are saved-up, and then employed at t_2 in the production of C_2 (i.e. $L_{2,1}^d$). The same condition must hold for the case of period-1 land. In the next section we will rely on this construction –adapted to study the problem of technical choices under long-period prices- to analyse the direction of change of technical choices when the interest rate changes.

⁷⁷ Actually, the partial derivatives with respect to factors $L_{1,1}^d$ and $T_{1,1}^d$ make little sense from an economic point of view since, when economic activity starts, these factors are already crystallized in specific capital goods, i.e. they have already *jointly* produced a single capital good. The procedure followed in the main text is done for analytical convenience only, because for the reasons addressed in section 2.2., the analysis will be subsequently applied to long-period technical choices. However, to help intuition, the reader may think of $L_{1,1}^d$ and $T_{1,1}^d$ as factors that have separately produced different capital goods, so that in fact they represent different amount of two capital goods.

2.2. Long-period technical choices.

Now, so far we have assumed that the economy starts at an arbitrarily defined initial period, and ends at the end of the second period. However, we have seen in the first part of this chapter that Hayek thinks of an indefinite future that tends to stationariness; hence Hayek openly admits that after sufficient time the composition of the capital stock will adapt to its forthcoming demand, and that prices will tend to long-period prices. Furthermore, we have noticed in the previous section that Hayek (PTC: 166) believes that along the sequence of MNE, price-changes will “rapidly decrease in magnitude” as time goes on; and although he argues that these changes will continue “to some degree for ever”, given the gradual adaptability of production to its forthcoming demand he also points out that these price-changes will become “*insignificant*” after some period of time. The implication of this is that Hayek must show the direction of change of technical choices as the rate of interest decreases conceding, *above all*, the tendency of choices to long-period choices under constant relative prices.

Little wonder then that, as noticed in the first part of this chapter, in part III of PTC Hayek analyses the effects of foreseen (and unforeseen) changes in the data by assuming that the economy is stationary before the change takes place, and becomes stationary several periods after that change. Recall that Hayek writes:

The significance of the conditions of stationary equilibrium of capitalistic production... will become clearer only as we apply the same considerations to the study of the effects of different types of changes... we shall therefore discuss the readjustments that will be made necessary if, after various types of changes in the data, equilibrium is to be re-established. (PTC: 268)

In other words, in part III of PTC Hayek seems to reason as if firms were confronted with long-period technical choices under constant relative prices. As an example of this, consider chapter XXI. There Hayek examines the direction of change of technical choices –i.e. the change in the distribution of dated inputs- when, in an otherwise stationary economy, positive net savings causes the rate of interest to decrease. It seems useful to anticipate Hayek’s conclusion, that at this stage of his exposition is fully Wicksellian: the decrease in the

interest rate will induce firms to increase the ‘investment periods’ of the non-permanent resources by transferring them from the later to the earlier stages of production; in other words, firms will adopt new cost-minimizing techniques that are more ‘roundabout’.

Since in equilibrium the discounted marginal product of the factor in question must be the same in all stages, the redistribution of a given supply of the factor will evidently be determinate for any given rate of interest... Then if we assume that the rate of interest is lowered... the given supply of the factor in question will evidently have to be redistributed between the different stages so that more will go to the earlier and less to the later stages. (PTC: 290) [78]

Now, before showing how Hayek reaches this conclusion, some modifications must be made to equations (1)-(3). Indeed, if we assume that technical choices are long-period technical choices, and we further assume that relative prices are constant over time because the economy is stationary before some of the data vary, and becomes stationary some periods after the change, then the total amount of C produced within each period can be regarded as a function $f(.)$ of the following variables: $L_0^d, L_1^d, T_0^d, T_1^d$; where L_0^d and T_0^d represent the employment on the part of firms of current labour and land, i.e. inputs saved for zero periods, and L_1^d and T_1^d represent the quantities of labour and land saved for one period, namely, inputs used in the previous period to produce the capital goods that, in the current period, are employed in the production of C . (Alternatively, as Hayek himself points out (PTC: chapter IX), the stationary assumption allows us to interpret L_1^d and T_1^d as the units of labour and land employed in the current period in the production of capital goods, and that will

⁷⁸ The Austrian idea that changes in distribution change the degree of roundaboutness of the production methods appears several times in Hayek’s work. Consider for instance what Hayek writes in chapter XXV of part III when he attempts to analyse the effect of a rise in real wages, and he concludes that “in so far as labour succeeds in securing for itself a larger share of the output and in raising real wages it will tend to bring about a substitution of capital for labour or a transition to more capitalistic methods of production. The net effect would probably be that fewer workmen would be employed with more capital per head...” (*id.*: 347). Consider also what he writes in chapter XXVI of part IV: Hayek (PTC: 364-366) assesses the influence of “real factors” in the demand for money by depicting a “marginal efficiency of capital curve” that is “simply what Mr Keynes calls the investment demand schedule, or the schedule of the marginal efficiency of capital” (*id.*: 364). The curve is in a negative relationship with respect to the rate of interest, because decreases in the interest rate “lead to a transition from more to less capitalistic methods of production” (*id.*: 386).

ripen into consumption goods in the next period)[79]. So, in each period, the total demand for labour is equal to $L_0^d + L_1^d$, while the total demand for land is equal to $T_0^d + T_1^d$. (Note that, differently from the example developed in the previous section, in this context it makes no sense to speak of a ‘first’ and a ‘final’ periods; also, note that the endowments of capital goods that are to be reproduced in (secular) equilibrium are endogenous variables to be determined by the conditions of equilibrium).

Under long-period choices with constant relative prices, the profit-maximizing problem of the representative firm is the following:

$$p f(L_0^d, L_1^d, T_0^d, T_1^d) - w_0 L_0^d - w_1 L_1^d - r_0 T_0^d - r_1 T_1^d \quad (4)$$

where p stand for the price of C ; w_0 , r_0 , w_1 and r_1 stand for the rentals, respectively, of current labour, current land, saved-up labour, and saved-up land ruling in each period, and assumed constant over time. Given the assumption of stationariness, in each period there are \bar{L} units of labour and \bar{T} units of land, and full-employment in every period requires:

$$\begin{cases} \bar{L} = L_0^d + L_1^d \\ \bar{T} = T_0^d + T_1^d \end{cases} \quad (5)$$

The first order conditions of the profit-maximizing problem are given by:

$$\begin{cases} p f_{L_0} = w_0 \\ p f_{L_1} = w_1 \\ p f_{T_0} = r_0 \\ p f_{T_1} = r_1 \end{cases} \quad (6)$$

⁷⁹ Note that the subscript that in the original problem -represented by equation (1)- indicates the specific period in which the inputs are employed, has now disappeared owing to the assumption of stationarity. The only subscript that remains is the one that indicates the possible employments of each non-produced input in any period: they can be employed in the production of consumption goods or they can be saved, and employed in the production of capital goods. Given that the economy is stationary, the amounts of labour and land ‘saved’ in the current period are the same as the amounts of labour and land saved in the previous period.

Equations (6) are equivalent to conditions (2): they show that in equilibrium the value of the marginal product of current and saved-up units of labour and land should be equal to their respective rentals. But analogous conditions to equations (3) must still be added to determine the optimal choices on the part of firms, because the available units of input in a generic period t can be employed in two alternative ways: they can be directly employed in the production of the consumption that will come out at the end of the same period, or they can be instead saved, and employed in the production of capital goods, which will ripen into consumption goods only at the end of the next period. Equilibrium thus requires that the values of the marginal products of current labour and land are equal to the discounted value of the marginal products of saved-up labour and land. As Hayek writes:

The actual share of the product to be attributed to a unit of 'labour' or the demand price which entrepreneurs will be willing to pay for a unit of 'labour' if employed at that stage, will be equal to the value of the marginal product discounted at the current rate of interest over the relevant investment period. (PTC: 289)

So, we have: $pf_{L_t} = \frac{pf_{L_0}}{(1+i_L)}$ and $pf_{T_t} = \frac{pf_{T_0}}{(1+i_T)}$, where i_L and i_T stand, respectively,

for the interest rate obtained by capitalists over the value of wages per worker and rent per unit of land paid to produce capital goods. Replacing these equations in conditions (6), we obtain:

$$\begin{cases} w_1 = w_0(1+i_L) \\ r_1 = r_0(1+i_T) \end{cases} \quad (7)$$

Finally, the condition of uniform profitability over capital requires that these rates of return are the same:

$$i_L = i_T \equiv i \quad (8)$$

We can replace (8) and (7) into (6) to obtain:

$$\left\{ \begin{array}{l} \frac{f_{L_1}}{f_{L_0}} \\ \frac{f_{T_1}}{f_{T_0}} \end{array} \right. = 1+i \quad (6')$$

Equations (6') give justification to Hayek's conclusion advanced at the beginning of this section, namely: "if we assume that the rate of interest is lowered... the given supply of the factor in question will evidently have to be redistributed between the different stages so that more will go to the earlier and less to the later stages." (PTC: 290); in other words, profit-maximizing entrepreneurs will adopt more 'roundabout' methods of production. The gist of Hayek's argument is the following: when the interest rate decreases, the left hand side of equations (6') becomes greater than the right hand side. So, the argument goes, entrepreneurs will be induced to increase the investment periods of dated-inputs by transferring them from the later to the earlier stages of production. In turn, this will have two effects: on the one hand, the marginal products of saved-up labour and land, f_{L_1} and f_{T_1} , will decrease, and, on the other hand, the marginal products of current labour and land, f_{L_0} and f_{T_0} , will increase. Both effects, it is argued, will work in the direction to re-establish the equilibrium conditions (6'). Schematically, for the case of current and saved-up labour, we have:

$$\downarrow i \rightarrow \uparrow \frac{L_1^d}{L_0^d} \rightarrow \downarrow \frac{f_{L_1}}{f_{L_0}}$$

(The same thing happens with current and saved-up inputs of land).

Closer inspection, however, reveals an important deficiency in Hayek's argument. In fact, suppose we increase the employment of L_1^d and keep constant the employment of L_0^d , T_0^d and T_1^d . Due to the principle of decreasing marginal productivity, we know that f_{L_1} will decrease. However, given that the inputs included in the production function are more than two, it may well happen that the marginal productivity of L_0^d decreases with the employment of L_1^d , namely

that $f_{L_1 L_0} < 0$ [80]. If the marginal productivity of L_0^d decreases enough with the employment of L_1^d , it may well be the case that an increase in the ratio $\frac{L_1^d}{L_0^d}$ is accompanied by an *increase* in the ratio $\frac{f_{L_1}}{f_{L_0}}$, rather than a decrease, as the adjustment process suggested by Hayek requires. (This effect need not be counterbalanced when, by means of this first equation of conditions (5), we consider, in a second stage of the analysis, the decrease in L_0^d that will take place when L_1^d increases[81]).

If this happens to be the case, then if in an otherwise stationary economy we admit with Hayek that the increase in savings causes the rate of interest to decrease, then we must conclude that the decrease in the ratio $\frac{f_{L_1}}{f_{L_0}}$ implies that in order to re-establish the equilibrium conditions (6), along the process of adjustment towards a new secular equilibrium entrepreneurs will find it optimal

⁸⁰ Suppose instead that only L_0^d and L_1^d are used as inputs, hence $C = f(L_0^d, L_1^d)$. Due to constant returns to scale, we have that $C = f_{L_0} L_0^d + f_{L_1} L_1^d$. Then, given that $f_{L_0} = \frac{C}{L_0^d} - f_{L_1} \frac{L_1^d}{L_0^d}$, we obtain that $f_{L_0 L_1} = \frac{-f_{L_1} * L_1^d}{L_0^d} > 0$, which is positive due to the assumption of decreasing marginal productivity of L_1^d . This last result implies that, as when for instance L_0^d increases, f_{L_0} must decrease and f_{L_1} must increase. Therefore, in the two-inputs case, an increase in the rate of interest –i.e. a decrease in the ratio of marginal productivities $\frac{f_{L_0}}{f_{L_1}}$ - must be associated with an increase in the ratio $\frac{L_0^d}{L_1^d}$ of present to past labour, as Hayek argued.

⁸¹ Consider a second stage of the analysis: due to equation (5), we now allow L_0^d to decrease (while T_0^d and T_1^d are still constant) by the same amount as L_1^d has been originally increased; then while the decrease in L_0^d may increase f_{L_0} , there is no reason why the ratio $\frac{f_{L_1}}{f_{L_0}}$ may not end up raising anyway. The reason is that the decrease in L_0^d causes an increase in f_{L_1} , which, moreover, may be more than proportional than the increase in f_{L_0} . So, the ratio $\frac{f_{L_1}}{f_{L_0}}$ may end up rising even more in this second stage (Cf. Garegnani, 1985).

to *decrease* the employment of L_1^d relative to L_0^d , contrarily to what Hayek argues (i.e. aggregate investment will decrease with a decrease in the rate of interest). At a more general level, we have argued in the first chapter that the phenomenon of reswitching has shown that it is not generally possible to order techniques independently of distribution, e.g. according to their degree of 'roundaboutness', because the same vector of capital goods -and hence the same distribution of labour and land units of input in the production process- can be associated with very different levels of the interest rate.

The conclusion that emerges is that, while Hayek formally takes as given the several endowments of capital goods, he assumes that temporary-equilibrium prices evolve in the direction of long-period prices (this is the really important assumption for his conclusions, not that eventually a secular equilibrium will be reached) and he bases his conclusions, on the effect of changes in policy or in some data, on the result of this tendency after a sufficient number of periods, so in fact he does traditional long-period comparative-statics analysis, and his reasoning in this respect presupposes that long-period techniques can be ordered according to their degree of roundaboutness, and this, albeit implicitly, presupposes the traditional notion of capital as a single scalar from its demand side. This traditional demand-side role of capital plays a crucial role in Hayek's framework, since i) it justifies the negative-interest elasticity of investment demand and ii) it justifies the traditional role assigned to the rate of interest (that of bringing into equilibrium investment decisions with full-employment savings), hence the stability of the savings-investment market. These features will be examined in the next section.

But before, I wish to notice that, interestingly enough, Hayek eventually *admits* that nothing can be said *in general* about the effect of changes in the rate of interest on the direction of change of particular input units.

[i]f we were to start from a complete statement of the substitution relationships between all the different resources concerned, all kinds of peculiarities and apparent anomalies in the behaviour of individual factors would appear to be quite consistent with the general tendencies. It is, for instance, quite possible that while a fall in the rate of interest will create a tendency for the services of most of the permanent factors to be invested for longer periods and for their prices to rise, in the case of some individual

factor the effect may well be that it will be invested for shorter periods, or that its price will be lowered, or both. (PTC: 292)

However, without any solid justification Hayek pays no attention to these “apparent anomalies”; he limits himself to argue that these issues belong “to more complicated aspects of the subject which it is not proposed to treat here *in extenso* (*id.*: 292)”^[82]. Hayek thus concludes that the “general tendencies” that can be deduced from the analysis are those predicted by Austrian neoclassical theory, a conclusion that, on the contrary, should have been reached by carefully studying the nature of those “apparent anomalies”. In other words, if the cases that contradict the theory are simply disregarded as “anomalies”, the only possible conclusions will be those that are in accordance with what the theory predicts.

2.3. The traditional notion of capital as a *flow* of savings.

As noticed in the previous section, Hayek’s analysis of long-period technical choices presupposes the traditional conception of capital from its ‘demand side’, since it takes for granted that techniques can be ordered according to their degree of roundaboutness *independently* of distribution; and we have shown the limitations of this analysis. In this final section I wish to discuss in some detail why Hayek seems to believe that the Austrian analysis developed in the previous section can be still applied in a neo-Walrasian framework, where the endowment of capital goods is formally included among the data of the equilibrium, and even though he admits that when distribution changes and long-period cost-minimizing techniques change, in the generality of cases the capital goods produced will change.

The little noticed point I wish to make is that, owing to the specificity of capital goods in production, Hayek envisages the factor substitution mechanisms as working *over* the sequence of temporary equilibria. In other words, for long-period technical choices, the rate of interest regulates the ‘degree of roundaboutness’ of production methods in new plants; the implication, to be discussed below, is that the traditional notion of capital as a

⁸² These self-indulgent conclusions explain why more than 25 years later Hayek still argued that a decrease in the rate of interest would induce the adoption of more capital intensive techniques of production (see the conclusions of this chapter below).

single factor capable of changing form will re-appear as a *flow* in the savings-investment market, and also that the rate of interest still plays the role of bringing into equilibrium savings and investment decisions.

Consider first Hayek's attempt to explain how a decrease in the rate of interest affects the composition of the capital stock in the extreme case where there is only one method to produce each final good. Hayek argues that

An increase in the relative amount of consumers' goods offered for capital goods, or a fall in the rate of interest, will cause an expansion of those industries which use more capital in proportion to labour than others do, while the inverse case will favour the industries using relatively little capital. More or less capital will be used in industry as a whole, not because the proportion between capital and labour (and consequently the technique of production) has changed in anyone industry, but only because the relative size of the groups of industries using comparatively much and comparatively little capital respectively has changed. The technique of production may have changed in none of the industries. All the different products may still be produced in the same manner as before. And yet the investment periods of the individual units of input which have been transferred to the expanding industry will have increased. What has happened is simply that the industries whose costs of production have been reduced more than those of others by the fall in the rate of interest have expanded at the expense of the second group (PTC: 286-287).

And he concludes

Wherever the coefficients in which the different factors of production can be combined in individual industries are relatively or absolutely rigid, a change in the relative scarcity of the different factors can be met only by an expansion of the scale of output of those industries which use relatively less of the factors that have become more scarce, and a corresponding contraction on the part of those industries that use more of those factors. (PTC: 287)

This passage gives initial support to the claim that, when the production of different commodities calls for different capital goods into use, Hayek sees the factor substitution mechanisms as working rather *slowly* –i.e. in Hayek's framework, over the different periods of a sequence of temporary equilibria. It is clear that Hayek considers here the physical endowments of capital goods as *endogenous* variables determined by the conditions of equilibrium, no doubt embodiments of the single factor 'capital', whose form can change only *gradually*; i.e. only after the labour units set free in those industries that use "little capital" are shifted to those industries that use "more capital". In fact, we have seen in section 1 that Hayek accepts that the tendency towards a situation in which the capital stock is completely adapted to its forthcoming demand after any sort of change will be "very slow and gradual" (PTC: 263). The overall result, so Hayek's argument goes, is that the economy as a whole becomes more

capitalistic because the investment periods of the units of labour that are transferred from those industries that use relatively less capital to those that use more capital will increase.

Consider now the following passages of chapter XX. Hayek here assumes that there are several techniques to produce the final good; however, given that capital goods are technique-specific, the more roundabout methods of production that are adopted when the rate of interest decreases can be initially introduced in new plants only, since

[t]he investments in the later stages of the new process can obviously be made only after the corresponding investments in the earlier stages have already been made, the whole process of lengthening the investment structure will be diffused over a period of time. When the new investment first begins to take place, only the input applied at the beginning of the various processes will be invested for longer periods; but people will do this with the intention of changing (and in the expectation that it will be possible to change) in succeeding periods the investment periods of units of input in the later stages of the same process. The transition from one sort of investment structure to another will therefore make it necessary during a considerable period of time for input to be transferred from one 'stage' of the process to another. (PTC: 279)

And a few pages later Hayek again stresses that, when techniques change:

[w]hat can be re-invested in the new and different form will be only the current pure input [i.e. labour] and the more versatile non-permanent resources. Such capital, on the other hand, as was irrevocably sunk, before the new saving was foreseen, in very durable and highly specific equipment, cannot, of course, be promptly or wholly shifted to a different use... and in some cases it will be found that even in the course of time only part of the capital originally invested can be recovered and re-invested in a different form (PTC: 283)

So, on the one hand Hayek freely admits that, when conditions change, the complete adaptation of the composition of the capital stock to the new conditions can take a *considerable* time owing to the specificity of the capital goods in existence. On the other hand however, he argues that the effects of a change in the rate of interest start asserting themselves *immediately* after these conditions change, by regulating those portions of the factors of production that are set free by the scrapping of old plants. It is only gradually -i.e. in the "course of time"- that the original capital invested will be "re-invested in a *different form*".

These passages bring strong support to the view that Hayek's description of the traditional factor substitution mechanisms operate *over* the sequence of

temporary equilibria: owing to the high specificity of the initial equipment, when the rate of interest decreases, the new cost-minimizing and more roundabout methods of production will be initially adopted in *new* investments only, i.e. only those labour units that have been released by the closure of old plants will be immediately employed under the now longer methods of production, while those labour units that still co-operate with the old equipment will be gradually re-employed under the new forms; in the meantime, the old equipment will yield residual quasi rents in Marshall's sense. In short, while Hayek avoids referring to the notion of capital as a stock by taking as datum the vectorial endowment of capital goods, the fact that the rate of interest is assumed to regulate the investment periods of the labour units that are set free in the old plants, implies that the increased roundaboutness of the production methods when the interest decreases concretely manifests itself in the investment of new plants, where the *flow* of free capital meets the flow of labour set free by the closure of old plants. And as seen in the first part of this chapter, a new position of secular equilibrium will gradually emerge as the result of "the successive reinvestment, in different and to some extent new types of capital goods, of funds which it is no longer profitable to use for the replacement of equipment similar to that from the amortisation of which these funds have been obtained, [which] will gradually lead to the building up of a new and different investment structure." (PTC: 263).

The previous passages are therefore particularly relevant because they seem to indicate why Hayek mistakenly thinks that traditional conclusions reached on the basis of a given endowment of value capital could be still supported by taking, instead, the vectorial endowment of capital goods as part of the data of the equilibrium: in his framework, the notion of capital as a single factor that changes form appears *as a flow* in the savings-investment market, and the rate of interest brings into equilibrium investment decisions with full-employment savings^[83].

⁸³ This same idea also emerges in Hayek's early work, *Prices and Production* (henceforth, P&P). There, Hayek argues that the value of the capital stock "it is not an imaginary, but a real and important magnitude, since the value of this total is a magnitude which continually rests within our power to determine. It probably stands in close relation to what is commonly called free capital, and it is certainly the supply of this factor which—together with new saving—determines the rate of interest; the capital which remains invested in durable instruments affects the

This role of the rate of interest is explicit in chapters XVII and XVIII and then in part IV of PTC. As e.g. Hayek explains in part IV, when the demand for present consumption increases relative to the demand for future consumption, i.e. the aggregate supply of savings is shifted to the left and the rate of interest increases, aggregate investment demand will decrease *pari passu*, and this restores the equilibrium between savings and investment. It is the explanation of the factor substitution mechanisms in terms of the roundaboutness of the production methods what justifies the sufficiently-negative interest elasticity of investment demand. As Hayek argues, when the interest rate increases:

During the transition from more to less capitalistic methods of production, the amount of input that will be demanded for investment purposes will fall. (PTC: 386)

(*cf.* also fn. 78). While Hayek admits that the savings function can have in principle any shape, this negative elasticity of investment demand with respect to the rate of interest is behind the conclusion that, in any event, capital accumulation, i.e. positive net savings, will be accompanied by “the fall in the rate of interest” (*id.*: 242). And again, in chapter XXV we find Hayek asserting that with positive net savings

[o]ne of the most important cost elements..., the rate of interest, will be reduced in consequence of this very increase in saving. (PTC: 344-345).

In other words, the previous passages show that in Hayek’s framework the rate of interest still plays its traditional role, i.e. it is the variable that must bring into equilibrium savings and investments decisions ensuring the stability of the savings-investment market, as if things continued working in the way justified by the traditional conception of capital in value terms. Little wonder then that Hayek concludes:

In long-run equilibrium the rate of profit and interest will depend on how much of their resources people want to use to satisfy their current needs, and how much they are willing to save and invest (PTC: 396)^[84].

interest rate from the demand side only, i.e., by influencing opportunities for new investment” (Hayek, 1967[1931]:66).

⁸⁴ Incidentally, as noticed in the first part, here “long run” must be interpreted in its traditional neoclassical way, namely, not as a secular equilibrium but as a persistent position that is to be observed after sufficient time.

I wish to finish my assessment of Hayek by noticing that, actually, it is not only in the savings-investment market that the traditional notion of capital manifests itself in PTC. Consider the following passage of chapter XX of part III:

[e]very increase in capital will mean an increase of capital relative to the quantity of other factors of production... It is only when the quantity of capital increases *ceteris paribus* [i.e. relative to the quantity of other factors of production] that it will lead to the peculiar consequences usually connected with an increase of capital, such as a fall in the rate of interest, a change in the technique of production, or the adoption of longer or more roundabout methods of production. (PTC: 269)

Hayek here simply decides to ignore his previous concerns regarding the illegitimacy of measuring the endowment of capital in value terms (concerns that, no doubt, prompted the writing of PTC), and he unproblematically speaks of a “quantity of capital” as a factor of production, which is clearly measured as an amount of value, and of the rate of interest as the price of this factor. Hayek therefore forgets what he had insistently pointed out in part I of his book, namely that this ‘quantity of capital’ would simply change with changes in income distribution, and hence that it cannot be a datum, *but only a result*, of the equilibrating process.

3. CONCLUDING REMARKS.

Our examination of PTC has shown that Hayek envisages the sequence of MNE prices, which he essentially sees as a tool to describe transitional dynamics between secular equilibria, as a good approximation of actual paths. A first implication is that, while Hayek attempts to do without the notion of secular equilibrium, he fails in this task: the secular equilibrium, which in PTC is assumed to be the position where capital goods yield a uniform return on their supply price, gives the direction of change of MNE, which considered by itself is otherwise indeterminate. Moreover, while Hayek attempts to develop the analysis without relying on the traditional notion of capital as a given amount of value, he fails in this task too. And the main reason is that the sequence of temporary equilibria is believed to bring about prices and technical choices that are the traditional long-period ones, determined according to Austrian theory, and that must presume the traditional conception of capital in value terms: so the sequence of temporary equilibria does nothing more than bring about traditional long-period positions; the a priori faith in the traditionally argued neoclassical tendencies then makes the approximation of

the sequence to actual paths look plausible. Therefore, the general conclusion that comes out from my assessment of PTC is that the inclusion of the set of capital goods among the data of the equilibrium is only a “cosmetic operation” (Garegnani, 2000: 443): Hayek’s analysis remains very traditional, i.e. the traditional factor substitution mechanisms in terms of capital –the single factor- and labour are still there, but they are now assumed to work over the sequence of (perfect foresight) temporary equilibria. Hayek’s heavily resorts to these mechanisms to justify with some plausibility the claim that the perfect-foresight-equilibrium (full-employment) growth-path can indicate actual paths with sufficient approximation. It must be also stressed that whenever Hayek must analyse the effects of policies or of changes in the data, he *does not* analyse how the first temporary equilibrium changes, but takes for granted that the sequence will eventually converge to a long-period normal position, and does long-period comparative statics; so in fact behind the smokescreen of temporary equilibria, he does long-period analysis. The outcome of *illegitimately* introducing traditional ways of reasoning in modern general equilibrium theory is that the long-run tendencies envisaged by the founders of neoclassical theory -i.e. the tendency towards full employment- and that stand or fall with the notion of capital, the single quantity, are essentially preserved in Hayek’s framework.

I would like conclude this chapter by noting that, despite his awareness of the illegitimacy of treating the endowment of capital as a single scalar measured as an amount of value, Hayek’s resort to traditional neoclassical ways of reasoning –that must presume this notion of capital- will still remain along the years. For instance, during his exchange with Hicks on “the Ricardo Effect” in the late 60’s, Hayek (1969: 275) admits that the representation of the factor substitution mechanisms in terms of isoquants that measure labour and ‘capital’ in the axes is misleading: changes in the rate of interest “necessarily involve some changes” in the prices of the several capital goods, and hence a change in one of the givens, namely, the endowment of value capital. However, he defends himself by arguing:

It seems to me that this defect is of comparatively minor significance and does not seriously detract from the validity of the conclusions which can be derived in a comparatively simple manner by these methods. (Hayek, 1969: 275)

Where by the “validity of the conclusions” reached, he meant that an increase in the rate of interest should induce the adoption of less value-capital intensive techniques of production (id: 275-276). Notice finally the nature of the statement: Hayek limits himself to write “it seems to me”, without providing any solid proof as to why it may be the case that “this defect is of comparatively minor significance”. And as it was definitely established during the Cambridge capital debates of the 60’s, it is not.

3.

LINDAHL'S *STUDIES IN THE THEORY OF MONEY AND CAPITAL (1939)*

INTRODUCTION.

Lindahl's *Studies in the theory of money and capital* (henceforth, STMC) is the main object of this chapter, whose structure somehow mirrors the one adopted in the assessment of Hayek's work. In the first section it is argued that the insurmountable difficulty relative to the specification of the endowment of capital as a homogeneous magnitude is certainly an important reason behind the author's decision to reject the traditional approach to prices and distribution, and to describe the behaviour of the economy along a temporary equilibrium (TGE) framework instead. However, Lindahl is particularly clear about the drawbacks of the TGE method: first, he is especially concerned with the possible *indeterminacies* that may emerge in the TGE framework owing to the introduction of exogenous expectation functions as part of the data of the equilibrium. Moreover, he shows noticeable uneasiness with the temporary equilibrium method due to the "especially unrealistic" assumption that the economy is *always* in equilibrium. However, Lindahl feels authorized to disregard these problems because, he argues, over *sufficient* time the sequence of TGEs can describe the behaviour of actual paths with sufficient approximation anyway: in STMC the TGE method is in fact essentially adopted as a device to describe *transition paths* between different secular equilibria; like Hayek therefore, Lindahl argues that the sequence of TGEs prices eventually reaches a secular equilibrium state, a position where the production of capital goods is fully adjusted to forthcoming demand, and the return on the supply prices of the capital goods is uniform across sectors. Secular equilibrium prices thus give the *direction of change* of the sequence of TGEs, which considered by *itself* is otherwise indeterminate. I will conclude in this connection that, in Lindahl's framework, the secular equilibrium essentially plays the traditional role assigned to equilibrium: it is the centre of gravitation around which actually observed variables tend to gravitate over sufficient time. The second part of the

chapter attempts to establish that, to plausibly explain the way in which TGE prices and quantities eventually reach a secular equilibrium state Lindahl will rely on traditional Austrian reasonings, i.e. he will argue that a decrease in the rate of interest induces firms to shift, along the sequence of TGEs, to more roundabout methods of production. Accordingly, the main conclusion of the chapter will be that, despite Lindahl's attempt to eliminate the traditional conception of capital as a single scalar from pure theory, the validity of his analysis and conclusions about the long-run trends of the economy *still* rely on this notion^[85].

1. EQUILIBRIUM ANALYSIS IN STMC.

1.1. The stationary state and the endowment of capital.

At the outset of his work, Lindahl argues that the final aim of economic theory “is either to explain the economic phenomena of the past or to forecast the economic events that will, under given conditions, probably occur in the future” (STMC (1939)^[86]: 21). The approach to these problems, Lindahl continues, involves the “demonstration of causal connections between the phenomena studied. The first step... is to explain a certain development as a result of certain given conditions prevailing at the beginning of the period studied [and the second is to compare] hypothetical developments that might be the result of an assumed variation of these conditions” (*id.*). Lindahl develops the temporary equilibrium method with the aim to deal with these issues.

Lindahl goes on to declare that, in economics, a central organizing concept to understand these “causal connections between the phenomena studied” is the

⁸⁵ Before proceeding with my assessment of Lindahl's work, I should remark that STMC is divided in three main chapters. The first chapter “*The dynamic approach to economic theory*” deals with more methodological aspects of equilibrium analysis in economic theory; the second chapter, “*The rate of interest and the price level*”, attempts to integrate the monetary analysis within the theory of general equilibrium; finally, in the third and last chapter, “*The place of capital in the theory of price*”, Lindahl develops, for the first time, a formal model of perfect foresight temporary equilibrium. It is also worth saying that, while the second and third chapters were respectively published in 1930 and 1929 as independent articles, the first chapter appears for the first time in 1939, as the introductory part of STMC. In this regard, while in the preface of the book Lindahl acknowledges that “Passages which do not agree with the author's present views have been retained” he immediately adds, “with the addition either of new explanatory paragraphs, or of new notes” (STMC (1939): 10). Therefore, it seems legitimate to accept that the content of the book, including the explanatory paragraphs, does reflect Lindahl's view of the working of the economic system by the time he publishes STMC.

⁸⁶ Considering that the three chapters are in fact three independent articles written in different years, the number in parenthesis refers to the year in which the article quoted was written.

notion of equilibrium, which “can be defined only on the basis of a theory of forces” (*id.*: 33). Lindahl notices that the working of these forces can be fully observed by having recourse to a “*reiterating process*” and therefore, he implicitly admits the need to rely on the study of those variables that are sufficiently *persistent*. “As in mechanics”, Lindahl in fact argues,

[t]he concept of equilibrium can be defined only on the basis of a theory of forces, so the significance of a reiterating process which, as stated above, is the central concept of the static theory in economics, can be explained only on the basis of a more general theory of economic development. (STMC (1939): 33)

There are in fact several instances where Lindahl explicitly stresses the importance of persistent forces for pure theory. For instance, consider what Lindahl writes when he develops the methodological foundations of his “general theory of economic development”: he notices that the data of the equilibrium includes individuals’ and firms’ plans about their consumption and production patterns, which can extend themselves over relatively long periods in the future; Lindahl therefore wonders whether or not this can be accepted as a realistic representation of the real world. He first observes that “in certain cases our assumption that economic actions are the result of planning activity may seem more difficult to apply [since] it can hardly be pretended that every individual has a clear conception of the economic actions that he is going to perform in the future” (STMC (1939): 37). However, he then remarks that

In the greater number of cases it will certainly be found that underlying such actions there are *habits* and *persistent tendencies* which have a definite and calculable character. We may accordingly without danger proceed to generalize our notion of ‘plans’, so that they will include such actions (STCM (1939): 37, emphasis added)

Notice then how Lindahl stresses in this passage the importance of *persistent tendencies* – in this case, in the form of *habits* that exert a persistent influence on individuals’ decisions-, and which cannot but manifest themselves over rather long periods of time, to set the foundations of a realistic theoretical corpus (the importance of persistent tendencies for Lindahl is further discussed in section 1.2.2 below).

Now, within the TGE method, Lindahl regards the stationary state as a particular case of a dynamic process that repeats itself identically period by period. However, he adds, the main goal of theory consists of the explanation of

more general phenomena. Why, then, bother with the stationary state? In STMC we find three different answers to this question, of which the first one is particularly important to our present concerns. Lindahl observes that

We can often use static structures as approximations to the real phenomena... Even in the real world all factors do not alter continually. There is always a good deal of invariability. In some fields, fairly stationary conditions may prevail for a comparatively long time. (STMC (1939): 34)

The passage is particularly interesting because Lindahl's observation that "*fairly* stationary conditions may prevail for a comparatively long time", and therefore, that stationary conditions can be regarded as a first good approximation of real world phenomena seem to be in line with traditional neoclassical thought. We may recall from the first chapter in this connection that the traditional notion of equilibrium, namely, the *static* stationary state, does not actually imply invariability of the data; rather, the exogenous variables are provisionally assumed as given because they are sufficiently *persistent*, i.e., because their speed of change is of a lower order of magnitude than the speed with which endogenous magnitudes approach their equilibrium values. Accordingly, given that actual economic conditions change rather slowly over time, traditional authors regarded the hypothesis of stationariness as a valid abstraction to focus on those forces that persistently affect prices and distribution over relatively long periods, and they legitimately disregarded the action of those mere accidental events because, countless as they are, they are simply bound to disappear with time. However, given that the exogenous variables in the problem of prices and distribution are bound to change, the analysis of the evolution and change of those variables should -and would- be explicitly considered in a subsequent analytical stage under more particular hypotheses.

But then, considering Lindahl's previous observations regarding i) the importance of persistent tendencies; ii) the legitimacy of the stationary state as a realistic representation of economic phenomena and, finally, iii) bearing in mind that the traditional, static approach to equilibrium theory acknowledges that the evolution over time of the *given* variables in the specific problem of prices and distribution must be subsequently analysed, the reader might legitimately wonder, "why does Lindahl decide to abandon the traditional framework and to develop, instead, a new 'dynamic setting' from scratch?". The

answer is suggested by Lindahl himself who, in the same introductory chapter of STMC, explains that

He [Lindahl himself] has some experience of the difficulties with the static method as a first starting point in the theoretical analysis. (STMC (1939): 33 fn.*)

Now, which were in fact these “difficulties”? To find the answer I must refer the reader to the third part of the book –which corresponds to Lindahl’s 1929 article “*The price of capital in the theory of price*”- where the author develops as a first step of his dynamic method, the general equilibrium equations that correspond to the traditional static (long-period) framework. There, the Swedish author observes that, to determine this equilibrium,

[o]nly the amount of the circulating capital can be regarded as known, determined in one way or another as a sum of saved-up services. The distribution of this capital among different types of services of various ages belongs to the unknown factors of the problem, like the rate of interest. *Otherwise the problem would be over-determined.* (STMC (1929): 304, emphasis added)

The passage is clear: unless the equilibrium composition of the capital stock is endogenously determined “the problem would be over-determined”, Lindahl argues, as for example in Walras’ original system of equations (see chapter 1) [87]. Therefore, Lindahl is forced to ‘close’ his general equilibrium system by setting up “an equation in which we use what may be regarded as given in respect of the size of the circulating capital” (STMC (1929): 308); Lindahl here admits two possibilities. The first is to follow Böhm Bawerk (1891), or Wicksell (1954[1893]) in his early writings, and take the “wage fund” as given, i.e. the “amount of wages [that] has been paid by the capital” (STMC (1929): 307). But Lindahl objects that Böhm Bawerk’s approach assumes that “the invested values are added together without account being taken of the interest that has accrued from the time of investment” (*id.*: 314); in other words, it is assumed that the prices of the capital goods are calculated by the rule of simple interest, a very

⁸⁷ It is important to remark however that, despite Lindahl admits that in order to avoid the over-determinacy of the equilibrium solution, the composition of the capital goods must be endogenously determined, he does not notice that Walras’s equilibrium equations would precisely suffer from that drawback. Indeed, in regards of the analysis put forward by Walras in the *Elements*, Lindahl limits himself to point out that, contrarily to what the French author assumed, the economy Walras was analysing was not necessarily progressive, and “his formulae can be better applied to a stationary society” ((STMC (1929): 294, fn*); in his general assessment of Walras’ system, Lindahl claims that it “provides on the whole a solution of the problem of including capital and interest in a mathematical analysis of the determination of prices under certain simplifying assumptions” (*id.*) but he does not mention that, by taking the quantities of capital goods as given, his system would be generally over-determined.

restrictive assumption^[88]. The other approach, Lindahl proceeds, consists of measuring capital directly “by the sum of values which it represents”. This is the method pursued by Wicksell (1934[1901]) in his more mature work. According to Lindahl, this method –which is the one he himself adopts- is more appropriate because the prices of the capital goods are determined under the rule of compound interest.

But in any event, Lindahl shows *noticeable* uneasiness with both ways of specifying the capital endowment, since under either approach this magnitude is bound to change when noting but the rate of interest changes. He writes that both methods of measuring the capital endowment

[h]ave the disadvantage, that the measure of capital is made dependent on the prices of the services invested and on the rate of interest, which belong to the unknown factors of the problem. When unexpected events occur that bring about a change in these factors, the size of capital will usually be changed if it is measured by one of these two methods, that is, as an amount of exchange value or as a certain length of the average investment period of all original factors. Since no better method seems to be available we must, however, choose one of these two measures of capital. (STMC (1929): 317)

So, Lindahl somehow tries to justify his choice allegedly because “no better method seems to be available”. Anyway, here Lindahl points out the problem that, for instance, Wicksell (1934: 202) had to face when explaining prices and distribution in terms of supply and demand schedules; i.e. a sufficiently persistent position of rest requires an endogenous determination of the composition of the capital stock, and hence only the aggregate endowment of value capital can be considered as part of the data of the equilibrium. But then, the insurmountable problem of determining the size of this endowment before distribution becomes evident. Lindahl’s considerations are simply a honest admission of this unavoidable difficulty within neoclassical theory. Yet, in Lindahl’s view, the problem relative to the value specification of the endowment of capital has other negative consequences for the theory. Indeed, he also notices that this issue considerably limits the applicability of comparative static exercises, since the fact that the size of the capital endowment changes with distribution renders the comparison between two different stationary equilibria a nonsense problem. “It seems impossible”, Lindahl admits,

⁸⁸ A more detailed assessment of this first approach will be given in chapter 6.

[t]o determine any fixed points for a comparison between different stationary situations. What is for instance meant by saying that two separate communities with different populations, on different cultural levels and with different technique and consequently quite different price relations, have the same quantity of capital? To regard this as being the case if the circulating capital has the same value (estimated in some common money term) or if the weighted average investment period is equal in the two communities is evidently a purely conventional idea. For this reason the possibilities of an analysis of the pricing problem on these lines are somewhat limited. (STMC (1929): 317)

However, at this stage of his exposition, Lindahl's appraisal of the problem of capital from its supply side makes a surprising twist. "The difficulties here mentioned", Lindahl in fact goes on to observe, "are associated with the stationary setting of the problem" (STMC (1929): 317); he subsequently dismisses the severity of the issue for the explanatory power of the theory by affirming that

On account of its very artificial and very special assumptions the static problem has little or no connection with the phenomena determining prices in the real world. Therefore, the attempt must be made to build up on this foundation an improved analysis which will have more general validity. (STMC (1939): 317)

Thus, Lindahl concludes, owing to the very special conditions of stationariness, the problem of the endowment of capital from its supply side can be overcome by relaxing the hypothesis of stationarity *itself*. However, in line with what Milgate (1979), and more recently Gehrke (2003) and Petri (2004) have all argued, if we bear in mind Lindahl's previous observations regarding the stationary state as a realistic representation of the real world (STMC (1929): 34), it would seem that the Swedish author's discontent with respect to the stationary framework had more to do with the problem of the quantity of capital as such than with the adduced unrealistic character of the stationary state⁸⁹. Before pursuing the argument further, it must be noticed that this notion of stationariness that Lindahl objects to is no longer the traditional, static, concept of stationary equilibrium, but the notion of secular equilibrium. "All factors determining prices", Lindahl in fact argues, "are to the requisite degree adapted

⁸⁹ These "difficulties" addressed by Lindahl in determining an equilibrium on the basis of a given quantity of capital may help explain why in the introductory chapter of STMC –which, as I have noted (see footnote 85), was actually written much later than the second and third chapters- Lindahl decides to abandon his original aim of going from pure 'statics' to 'dynamics' –as he actually proceeds in the third part of STMC- and he directly starts his analysis by developing the basis of a pure dynamic framework of which the stationary state is considered as a "particular case". This tension between the different methods of exposition is, in fact, openly admitted by Lindahl in the Preface of the book when he argues that "Differences of method between the two later parts and the first are the inevitable consequence of the development of the author's thought" (STMC (1929): 10).

to stationary conditions' (STMC, (1929): 311–12), i.e. the quantity of capital is the one that induces zero net accumulation^[90].

How Lindahl proceeds is probably well known, he removes the hypothesis that the economy is stationary by developing, for the first time, a formal neo-Walrasian model in which the physical endowments of capital goods are part of the data^[91]. As Gehrke (2003) argues, by treating each capital good as a *distinct* factor of production, the TGE method allowed Lindahl to avoid a value specification of the capital endowment of the economy, and then to endogenously determine the quantities of capital goods that will be reproduced in the following periods until supply is fully adjusted to forthcoming demand, and the return of the supply price of the capital goods is uniform across industries, a position that Lindahl associates with the secular equilibrium state. In fact, as it is discussed in section 1.3 below, Lindahl declares that the sequence of TGEs will eventually reach a secular equilibrium; however, in order better to grasp the reasons that lead the Swedish author to argue that this is the case, we must first turn to consider some serious drawbacks admitted by Lindahl with respect to the TGE method.

1.2. Lindahl's uneasiness with respect to the temporary equilibrium method.

1.2.1. The indeterminacy of the temporary equilibrium path.

⁹⁰ A further indication that Lindahl associates the notion of stationary state with a secular equilibrium comes from the fact that Lindahl views the stationary state as a state that is actually reached by a dynamic process. For instance he writes that "A stationary state may possible arise in the course of time" (STMC (1929): 311), or that "if in a stationary situation some important factor were changed all the functions would have to be modified to a greater or less degree, before new stationary conditions could arise" (id.: 312). It must be noticed that at this stage of his exposition, Lindahl does not seem to be clear that in the secular stationary state there is no need to include the endowment of value capital among the data because it is an endogenous magnitude. He includes the quantity of capital among the data because he seems to reason in the following way: suppose that the economy is in stationary equilibrium, to this situation there corresponds a certain amount of value capital; then given that the economy is in secular equilibrium, if we formulate a static equilibrium with a given quantity of capital equal to the one that would result in this secular equilibrium, the equilibrium determined will be the same (*cf.* Petri, 2004: 120, fn. 4).

⁹¹ "During the initial period in the dynamic process under observation, all existing capital equipment in the community can be regarded as original, including any that has actually resulted from the production of earlier periods not covered by the analysis. For since at the starting-point a knowledge of the nature of all capital equipment must be presumed, the question of its actual origin is irrelevant. Produced capital goods have the same significance for price formation as true original sources of similar kinds" (STMC (1939): 320)

In STMC, Lindahl raises two objections with respect to the TGE method as a tool for explanation and description of actual paths, and which by themselves seriously undermine the capacity of the TGE method to have a correspondence with observation. First, due to the inclusion of expectation functions among the data, Lindahl declares that the description of the evolution of prices and quantities by means of the TGE method may reach indeterminate outcomes. Consider for instance what Lindahl writes with respect to the possible effects in TGE variables after there is some change in the data:

The effects of a change cannot be unambiguously determined, since they depend on the nature of the original state, and on the *reactions* of individuals in different situations. (STMC (1930): 159)

In the introductory chapter of STMC, Lindahl explains that to determine temporary equilibrium prices and quantities, and also the evolution of these magnitudes over subsequent periods, it is necessary to include among the data (STMC (1939): 37) i) the plans of the individuals at the initial point of time, ii) how these plans are likely to be changed in the future; iii) how “external” or “non-economic” factors affect individual’s plans and actions. And with respect to the third group of factors, he writes:

Concerning other (‘non-economic’) events during the period, enough must be known to make it possible to calculate... the conditioned actions that follow therefrom. Here we may have to deal with climatic or political conditions. For instance, if we know that the members of the group are prepared to act in a certain way on a sunny day and in a different way on a rainy day, the determination of the their actions presupposes knowledge of which alternative is realized. (STMC (1939): 57)

Clearly however, at a sufficiently general level of abstraction it is impossible to assert the *direction* in which those non-economic factors such as climatic conditions or political events may influence production and consumption plans. And Lindahl seems to be so much aware of this problem that he directly suggests eliminating it by *assumption*:

A theoretical treatment of the problem may of course be facilitated by the use of various simplifying assumptions. Thus... non-economic events are neglected... (STMC (1939): 57)

But the problems with given expectations functions do not stop there. Lindahl in fact openly admits that agents are likely to modify their plans, not only because expectations are usually mistaken and revised in the light of experience, but also

because individuals alter “their valuation attitude” (STMC (1939: 47), that is, agents generally change *the way* in which they form their expectations about future events. And with respect to these kinds of changes, Lindahl accepts that they are

[u]sually of a more irregular character, and hence they present a more difficult subject for theoretical treatment. (STMC (1939: 47)

Namely, Lindahl admits here that it is very difficult (impossible, actually) to predict how expectation functions are modified in the light of experience. Finally, Lindahl also acknowledges that individuals are heterogeneous, and hence “it will be probably necessary to distinguish between different categories of economic subjects and between different valuation attitudes” (*id.*: 50). In sum, the countless number of events that may affect individuals’ plans, the heterogeneous ways in which individuals form these plans and, last but certainly not least, the impossibility to account for the way in which these plans are subsequently revised and modified in the light of experience, lead Lindahl to accept that “it is of course *impossible* for the economist to follow [the development of the economy] in detail *from period to period*”. (STMC (1939): 59, emphasis added), and he concludes:

In general it is impossible for the economist to give a complete analysis of a complicated course of development in one and the same exposition. He must usually be content to discuss the total developments from some special point of view, i.e. he must make a selection among the variables entering into the development. He must then assume that the other variables connected with those selected are given in one form or another, and must leave their study to other branches of the science. (STMC (1939): 59)

So, Lindahl openly recognizes here that the theorist must select only some of the possible variables that may exert an influence on prices, quantities and distribution along the sequence of temporary equilibria. However, given that expectation functions may be influenced by any sort of accidents, e.g. climatic or political conditions, and expectations functions will generally change in unpredictable ways in the light of experience, as we have seen Lindahl openly admits, the choice of these variables, and the way in which they may affect expectations, cannot but be *arbitrary*, it would seem; and Lindahl indeed admits this when he writes, since he accepts that the theorist must “be content” to select the variables under examination under some “special” criterion. To put

it in a nutshell, the inclusion of expectation functions among the data of the equilibrium inevitably condemns the theory to *indefiniteness*. A “weakness” of the TGE, so Lindahl honestly concludes,

[...]lies in the fact that the dynamic element is not overtly present in the equilibrium equation in each period, unless the equation is made so complicated that it also includes anticipations referring to future periods... In other words, during each period there is present a latent disequilibrium, and that is the reason why equilibrium achieved during the period is found to be only temporary. The driving force in the dynamic process thus lies entirely in the sphere of expectations, and this curtails the usefulness of this method as a basis for the construction of exact model sequences. (STMC (1939): 69)^[92] [93]

1.2.2. An economy that is *always* in equilibrium.

A second objection raised by Lindahl to the TGE method is the following: the author of STMC accepts that by relying on the notion of TGE, the economy is assumed to ‘jump’ from one position of temporary equilibrium to another.

Especially unrealistic is the assumption that the dynamic development presents an abrupt transition from one equilibrium position to another. (STMC (1939): 69)

Lindahl thus shows particular uneasiness with the fact that the temporary equilibrium method “unrealistically” envisages the economy as essentially being *always* in equilibrium. Lindahl is in fact clear that real-world adjustments cannot be argued to be instantaneous, since they take sufficient time to assert their effects fully^[94]. For instance, when describing the effects of a drop in the

⁹² On this basis for instance, Lindahl criticized the concept of the *neutral* rate of interest since this concept should be defined according to the expectations of the “public”; accordingly, arbitrary hypothesis regarding expectations should be defined. “A neutral rate of interest does not necessarily imply an unchanged price level, but rather such a development of prices as is in accordance the expectations of the public, so far as this is possible... This definition of the concept of ‘normal rate of interest’ presents certain difficulties. Under actual conditions, expectations regarding future conditions do not everywhere coincide. It is therefore impossible to speak of a generally anticipated future course of prices. The price development to which the loan rate is to be related must be conceived as a sort of average of different individual expectations of future prices. Since a certain amount of arbitrariness can hardly be avoided in a construction of this kind, the concept ‘normal rate of interest’ has some corresponding arbitrariness. This brings to light a certain imperfection in the concept.” (STMC (1930): 252-253).

⁹³ The passage is also interesting because Lindahl notes that, as long as expectations are part of the data, during each period there is a “latent disequilibrium”, because the subjective “anticipations of the individuals referring to the future period” might well turn to be actually incorrect, hence expectations will eventually change and the equilibrium position will be subsequently modified; in other words, by pointing out the transient character of the temporary equilibrium, Lindahl came close to admitting that a TGE could not be considered, in fact, as a *proper equilibrium*, since it is a position from which the economy tends to *move away*.

⁹⁴ However, Lindahl does not explain why the theory must *necessarily* assume instantaneous equilibration or which would be the consequences of eliminating the assumption of an abrupt transition from one equilibrium position to another, i.e. the path dependence of the equilibrium.

money rate of interest, Lindahl openly acknowledges the long-period nature of the phenomenon under analysis.

A change in the interest rates affects offers of consumption goods directly by causing changes in a proportion of these goods held in a stock, and in the long run by giving rise to adjustments of production in respect of the balance between present and future output. A reduction in the short term rate soon leads to increased stocks, since it reduces interest charges on holding them. This gives rise to smaller offers of consumption goods. Conversely, a higher interest rate leads to a reduction of stocks and increased offers of goods. Adjustments in production are caused by the altered relation between the prices of capital and consumption goods brought about by the change in the long term interest rate. This changed price relation is an immediate consequence of the altered interest rate, but the adjustments in production to which it gives rise can only take effect by degrees, on account of frictions. (STMC (1930): 156-157)

And he clearly notices that the transfer of labour and other factors of production due to changes in income distribution requires rather long periods to assert itself fully. “In the real world”, he readily admits,

[i]t is more difficult to transfer factors from short to long term investments than to make adjustments between factors which are all invested for a short period. It is especially easy to adjust the date of the ripening of very short period investments by varying stocks. To transfer labour and other factors from consumption to capital goods industries on the hand is more difficult and requires a longer time. (STMC (1930): 192)

Moreover, given that the effects of changes in the variables under study will make themselves felt only after a significant interval of time, Lindahl accepts that it is actually impossible to determine the factors determining TGE variables.

It is impossible to make a satisfactory analysis of the factors influencing the price level if it is confined to a single period. For the most important effects of an alteration of the rate of interest, for instance, do not appear until after a comparatively long period of time. (STMC (1930): 158)

Where by “single period” Lindahl means here a single temporary equilibrium period⁹⁵. Lindahl accepts therefore that the absolute level of TGE prices is essentially indeterminate. All the difficulties documented in this and in the previous sections with the TGE approach as a tool for explanation and prediction of real world events pushed the Swedish author to declare that this

⁹⁵ That Lindahl here associates each “single period” with a TGE period is clear when he writes that “We assume further that in each of these short periods of time individuals have full knowledge of the prices ruling during the period, and that they allow their actions concerning supply and demand to be determined by these prices, which are therefore consistent with their actions. The price situations will then be equilibrium states in the sense that there will be equality between supply and demand during the period. (STMC (1930): 159)

method has a limited “usefulness” (STMC (1939): 69), and to openly accept that “In a real dynamic development such equilibria are probably found only sporadically” (STMC (1939): 69). These evident shortcomings of the TGE should have naturally made Lindahl reject the entire TGE approach as a proper scientific method to explain the persistent causes of prices and distribution. However, if we bear in mind Lindahl’s admitted “difficulties” with the traditional method of analysis and the ‘quantity of capital’ (*cf.* section 1.1), a complete rejection of the TGE approach would have left him in a *vacuum* that he was not willing to face, it would seem.

In view of this, little wonder that Lindahl adopts a different position: he defends the usefulness of the TGE method by concluding that “these disadvantages” of this approach, serious as they are, would become less important

[w]hen we aim at a description of the *main lines* of a dynamic development. It then seems to be justifiable to concentrate our study on such possible temporary equilibria in different phases of development. By comparing such equilibrium positions, as the analysis in Part II will show, we obtain a picture of the essential characteristic that is both simple and clear. (STMC (1939): 69).

Lindahl (STMC (1930): 59) already affirmed earlier indeed that, in spite of the fact that it is actually impossible for the economist to follow in detail the movement of TGE variables from “period to period”, the TGE method can capture in any case the “active tendencies at work” because it can describe the actual trends of prices and quantities over sufficient time. “It is quite feasible”, the author declared,

to give a picture of the course of events during a *fairly long* stretch of time. If by the analysis of certain selected typical periods, one can determine the *directions of movement* during these stretches, the character of the intervening periods may also be understood. It may sometimes be necessary to modify the actual character of the periods selected so that they may adequately represent the *active tendencies* during the phase in question. (STMC (1939): 59, emphasis added)

These considerations further support the view advanced in section 1.1^[96] that in STMC Lindahl is mostly, if not exclusively, concerned with those

⁹⁶ In effect, recall from the first section that Lindahl argues that economic theory needs relying on a “reiterating process” (STMC (1939): 33) to capture the persistent, long-lasting causes that may systematically affect the phenomenon under analysis over rather long periods.

persistent tendencies at work, and that would assert themselves rather slowly, over long periods. The fact that Lindahl admits that it is impossible to “satisfactorily” isolate the factors influencing each TGE, and therefore, that the absolute level of each set of TGE prices is essentially indeterminate and hence *irrelevant* to understand the trends in actual economies explains why, as the next section argues, “the *main* lines of development” that Lindahl attempts to describe by means of the TGE approach are transition paths between different secular equilibria; it is this equilibrium concept that gives “*the direction of movement*” of the sequence of TGEs over a “*fairly long* stretch of time”, with the implication that, within Lindahl’s construction, the secular equilibrium has the traditional role of a centre of gravitation of actually observed variables.

1.3. Convergence of MNE prices to a secular equilibrium.

Despite the fact that Lindahl declares that the secular equilibrium “has little or no connection with the phenomena determining prices in the real world”, still, I submit, in STMC this equilibrium concept has a higher hierarchical theoretical position than the notion of TGE: in part II Lindahl looks for the conditions under which a secular equilibrium is to be realized over sufficient time, and, in part III, he directly assumes that the sequence of temporary equilibrium prices eventually reaches a secular equilibrium state. In this way, like in Hayek’s *Pure theory of capital*, this latter equilibrium notion gives the direction of change of the sequence of temporary equilibria, which considered by *itself* is otherwise indeterminate^[97]; essentially, therefore, the secular equilibrium plays the role of a centre of gravitation, the position where the economy tends over sufficient time.

As to part III of the STMC, Lindahl argues that “A stationary state may possibly arise in the course of time” (STMC (1929): 311) and that “the community would reach in time stationary conditions. Population, capital and other factors affecting prices would gradually attain a relative size and nature such that at prevailing prices, conditions for a stationary state would come into existence” (STMC (1929): 331). And when Lindahl comes to formalize the

⁹⁷ In fact, considered by itself, the sequence of TGEs does not contemplate any notion of *normal* price that is determined independently of the sequence itself, and around which the sequence will gravitate over a sufficient interval of time.

temporary equilibrium under perfect foresight, he *assumes* that from period v onwards, the economy is in a position of secular equilibrium state.

Let us now turn to part II of the book. Although Lindahl does not consider perfect foresight “as entirely unrealistic, since people actually do anticipate correctly much of what takes place” (STMC (1939): 10), in STMC the hypothesis of perfect foresight is adopted only as a provisional assumption that is eventually relaxed in order better to represent real-world phenomena; hence along his book, especially in part II, Lindahl resorts to the temporary equilibrium method to represent actual economic conditions, that is to say, he includes exogenous expectation functions among the data of the equilibrium.

However, we have noticed in section 1.2.1 that Lindahl is particularly aware of the possible indeterminacies caused by exogenously given expectation functions. He is therefore clear that expectation functions –and hence, temporary equilibrium variables- can be influenced by a variety of transient exogenous events, which cannot be reduced to any general explanation; and this may prevent the theory from explaining how prices change from one temporary equilibrium to the next. So, because the evolution of temporary equilibrium prices depends on the evolution of expectations, which can in turn be subject to any sort of exogenous accidental influences, it is *not possible* to determine a priori the direction of change of temporary equilibrium prices from period to period, and this inability seriously impairs the usefulness of the temporary equilibrium method to explain and predict how the system of prices evolves over time. As Ciccone (1999: 81) argues in this connection, “for the sake of its own relevance the theory must not lose control, so to speak, of the sequence of temporary equilibria.”, and he continues:

Witness of this concern can be found in the effort the analysis has devoted to detecting the conditions under which the sequence converges to some particular state, often defined as ‘long-run’ equilibrium. (Ciccone, 1999: 81)

Precisely, Lindahl proceeds in this way in part II of STMC: he considers the “case of a stationary economy suddenly disrupted by a lowering of the interest rates, which is expected to endure” (STMC (1930): 162), and attempts to detect the conditions under which the sequence of TGEs converges to a new secular

equilibrium state. Clearly then, especially in part II of the book, the TGE method is used as a tool to study *transition paths* among secular equilibria. The decrease in the rate of money loans set by the Central Bank is argued to cause a typical Wicksellian cumulative process, which is “characterized by the transference of factors from short to longer term investments together with a general price rise” (*id.*: 180) “The problem is”, Lindahl eventually wonders:

[h]ow long will this process continue? Will a new stationary equilibrium gradually be established when capital resources have been so greatly increased that no more capital can be absorbed at the existing interest rates? (STMC (1930): 180)

According to the author, under the hypothesis of *static* price expectations, i.e. as long as “individuals in each period expect existing prices to be maintained in the future” (*id.* 180), the inflationary process will eventually come to a halt, and a new secular equilibrium will assert itself. Lindahl writes:

The price which is the effect of a lower rate of interest need not continue indefinitely, but comes to an end when the supply of capital has been increased until it corresponds to the new interest rate. (STMC (1930): 181)^[98]

⁹⁸ Notice incidentally that, in Lindahl’s view, monetary policy might well have long-lasting real effects in the economy: a decision to lower the level of the interest rate by the monetary authority is argued to trigger a redistribution of income in favour of those individuals (the capitalist borrowers, i.e. the entrepreneurs) who have a “strong disposition to save at the expense of those whose disposition to do so is relatively weak” (STMC: 174). As a result, “entrepreneurs find their incomes increased and have a strong incentive to apply it to further capital investment. The income that comes to the entrepreneurs at the cost of the rest of the community will thus tend to be saved to a greater degree that would otherwise be the case. The shift in the redistribution of income thus increases total saving to such an extent that it more than counteracts the decline in the propensity to save due to the lower loan rate of interest” (STMC (1930): 174). Therefore, as opposed to Wicksell, we find Lindahl asserting that it can well be the case that the cumulative process would come to a halt because, due to the higher level of capital accumulation, the rate of interest determined by the supply of and the demand for savings would accommodate to the money rate, which means that Lindahl accepts as a likely possibility that monetary policy can exert a long-lasting influence on aggregate demand. He concludes in this connection: “The statement commonly met with that the quantity of savings directly determines the amount of real investment is thus, strictly speaking, not quite correct, as long as monetary policy is autonomous. The causal connection should instead be described by saying that the rate of interest, by determining the price relation between capital and consumption goods, makes the production of the former remunerative to a certain extent. The required savings will take place, voluntarily in greater part, and the causal element will rather be the alteration in distribution of income due to the shift in the price level” (STMC (1930): 175). As noticed, these results are clearly opposed to Wicksell’s. While Wicksell does consider the possible real effects, i.e. capital accumulation, caused by the action of the monetary expansion, he seems however to disregard this issue as being of secondary importance because, due to the high elasticity of investment with respect to the rate of interest, the amount of capital in need of accumulation by lowering the rate of interest would have to be significant (Pivetti, 1991: 95; Garegnani, 1979: 66, fn. +).

At this juncture it must be noticed that, while on the one hand Lindahl affirms that “the increase in capital resources is regarded as desirable” (*id.*: 182), on the other hand he does not in the least suggest that monetary authorities should attempt to decrease the money rate of interest to stimulate investment demand. First, because it will generally “take a long time” for a new stationary equilibrium to assert itself, during which “prices are continually rising” (*id.*: 182). In view of this undesirable, long process of cumulative inflation, Lindahl claims that

[i]t may then probably be necessary to arrest the movement before the amount of capital appropriate to the lower rate of interest has been accumulated. This will mean that a certain part of the capital newly invested will not give adequate returns (as it has been based on a lower interest calculation) and must be regarded as misdirected, with corresponding losses for the entrepreneurs. (STMC: (1930): 182-183)

Second, because had price expectations been sufficiently *elastic* (something that cannot be excluded on theoretical basis, see immediately below), “the anticipation of higher prices will make longer investments” (*id.*: 182), and the “transfer of factors to longer processes will continue”, causing an increasing disproportion between supply and demand of consumption goods that will “be brought to an end by a crisis” (*id.*: 182).

As regards the elasticity of price expectations –on which, according to Lindahl, the convergence to a new secular equilibrium is ultimately assumed to rest - it must be noticed that, while in part II Lindahl suggests here and there that in actual economies expectations are elastic (e.g. STMC (1930): 186) – something that would jeopardize the convergence to a secular equilibrium state- in the third part of STMC, on the other hand, he clearly supports the view that “the most natural” assumption is either correct or static expectations:

In a theoretical case these assumptions [regarding expectations] should not be arbitrary in any way. The most natural assumption from the theoretical point of view is either that anticipations are such that they will be realized... or that in each period people are certain that the price situation then existing will be maintained in the future. (Lindahl (1929): 344)

But Lindahl’s analysis does not stop with the examination of the possible effects caused by a decrease in the money rate of interest. In part II he also examines the dynamic process triggered by, e.g., a sudden change in consumers’

tastes (STMC: 201-204), changes in the supply of savings (STMC (1930): 204-210), changes in the productivity in the capital goods' industries (STMC (1930): 210-218) and changes in the productivity in the consumers goods' industries (STMC (1930): 218-222), and goes on to indicate how the monetary authorities should proceed to keep the price level stable, and therefore, by avoiding an undesirable inflationary (or deflationary) process, to allow for a *smooth* transition from the old secular equilibrium to the new one. As an example, consider for instance what Lindahl writes about the case of an increase in the marginal productivity to save that is expected to be permanent: in order to avoid an downward spiral of money prices, he suggests that the Central Bank should sufficiently decrease the long term rate of interest so that “the fruition of real capital will be lengthened, and the consumer’s goods maturing annually will be reduced to the extent required as a consequence of the saving” (*id.*: 207)[⁹⁹].

In the light of the foregoing considerations, it seems to be sufficiently clear that the key elements of Lindahl’s dynamic analysis are the *reference point* -a stationary state- and whether or not, once started, the dynamic process eventually *comes to an end* –i.e. reaches another secular stationary position. As noticed in section 1.2.2, the assumption of temporary equilibria along the transition path, on the other hand, seems to be rather irrelevant for Lindahl’s purposes. Consider in this connection that, while when describing his ‘dynamic’ method, Lindahl first argues that the equilibrium prices established within each analytical period “will be then equilibrium states in the sense that there will be equality between supply and demand during the period” (STMC (1930): 159), later on, when he wonders whether or not a new stationary equilibrium is ever to be reached after some specific change in the data, he does not usually regard the sequence of TGE prices along the transition path as actual ‘equilibrium’ states. For instance, he observes that:

⁹⁹ As another example, consider the case of a shift in consumers’ demands. In this case, Lindahl (STMC (1930): 204) claims that, during the transition period, there will be higher investment demand and higher money prices. To avoid an upward pressure on the price level therefore, an increase in the short term rate of interest on the part of the Central Bank is necessary. As to the long term rate, Lindahl asserts that, if as a consequence of the shift in tastes, the consumption goods that are now in higher demand require “more capital” to be produced, the rate of interest under the new secular equilibrium conditions will be higher than the old one. Hence, to avoid a maladjustment between the rate of interest determined by the forces of supply and demand and the long run money rate, and that eventually triggers an upward Wicksellian inflationary process, the Central Bank should increase the latter as well.

[t]he question here is whether the final result will be a *new equilibrium* or whether the cumulative process will continue indefinitely. (STMC (1930): 180, emphasis added)

where for “new equilibrium” Lindahl means a new secular equilibrium. Similar considerations are made by the author e.g. in pages 182 and 186 of STMC (1930), where equilibrium and secular equilibrium are identified as one and the same thing and, consequently, there is no reference to other notions of ‘equilibrium’ prices, as for example temporary equilibrium prices. Furthermore, in accordance with the foregoing interpretation, earlier in that chapter of STMC Lindahl already argued that the results regarding the effects of monetary policy on prices and distribution he had reached thus far under the assumption that a temporary equilibrium is to be established in every period, are in fact “in full harmony with the results obtained with the aid of the more realistic method” (STMC (1930): 175-176), i.e. by regarding “the dynamic process as consisting of a series of disequilibria” (STMC (1930): 175); in this way Lindahl is close to admitting that the assumption that in every period a temporary equilibrium prevails is in fact of secondary importance for his analysis.

So, while Lindahl is clearly uneasy with the possible indeterminacies in TGE prices caused by exogenous expectations functions, and also objects to the TGE approach the assumption that the economy is always in equilibrium, he ends up accepting the usefulness of this method to study dynamics along transition paths between secular equilibria, which, by giving the direction of change of the sequence of TGEs, clearly retain the role traditionally assigned to equilibrium as a centre of gravitation. In fact, the assumption that these transition paths are characterized by a sequence of market clearing, TGE, prices is regarded by Lindahl as a secondary issue. To sum up, as long as the sequence of TGEs could be plausibly argued to describe actual paths with sufficient approximation, the drawbacks of the TGE approach, Lindahl believes, could be legitimately neglected as being of relatively minor importance. But Lindahl will meet deep troubles to use the TGE to account for the “main lines of development” of actual paths. As the second part shows, to plausibly describe the adjustment process between secular equilibria Lindahl heavily relies on Austrian gravitational reasonings, marred by the traditional conception of capital in its demand side role.

2. THE INTEREST RATE AND THE SAVINGS-INVESTMENT MARKET.

In this second and final part I shall attempt to show that Lindahl, despite acknowledging what we have called “the supply side problem of capital” (*cf.* chapter 1, section 3), fully embraces the traditional Austrian belief in a negative relationship between the rate of interest and the higher profitability of more capitalistic methods of production, a relation that presupposes the traditional notion of capital as single factor in its demand side role. This idea is already anticipated in the preface of the book: Lindahl (STMC (1939): 10) explains there that part III of STMC is an attempt to “fuse together” the pricing theories of Walras and Cassel and the capital theories of Böhm-Bawerk and Wicksell. As to part II, devoted to studying the relationship between “The price level and the interest rate”, he declares that, while the novelty of his analysis is that the economy is assumed to evolve along “a series of temporary equilibria... For the rest of the problems are dealt with on Wicksellian lines” (*id.*: 11). In short, the idea that a decrease in the rate of interest induces profit-maximizing firms to adopt more capital intensive methods that in turn increase aggregate investment appears in several parts of the second and third parts of STMC. For instance, in part three of STMC Lindahl argues that one should

[k]eep in mind that with an increased amount of capital and a falling rate of interest longer investments are on the whole increased in a greater degree than the short ones. (STMC (1929): 310)

And he adds in a footnote that this conclusion,

It may perhaps be said to constitute his [Wicksell's] *foremost achievement* in the field of capital theory (STMC (1929): 310, emphasis added)

Lindahl's concerns with respect to the validity of traditional neoclassical capital theory are undoubtedly restricted to the supply side only, i.e. to the specification of the capital endowment independently of the rate of interest, without suspecting that this problem also undermines the traditional explanation of the distribution of income in terms of elastic demands for factors of production. This explains why, already in the introductory chapter of the book, Lindahl forcefully affirms:

The simplified exposition in static theory of a higher level of wages due to an increase in the quantity of capital is found to retain important relevance under dynamic conditions (STMC (1939): 30).

Those “dynamic conditions” refer to the theory of temporary equilibrium, where the endowment of capital of the economy is specified as a vector of heterogeneous capital goods. However, contrarily to what Lindahl claims, once it is acknowledged that the “quantity of capital” depends on distribution, i.e. it is, paraphrasing Hayek, “a *resultant* of the adjustment process”, there is no reason to believe that the physical changes in the composition of the capital stock entailed by changes in distribution must be such that, when the wage rate increases, the “quantity of capital”, i.e. the value of the new composition of the capital stock, should increase as well, as shown by reverse capital deepening.

In order to clearly grasp the Wicksellian influence on Lindahl, we must turn to part II of STMC. As we have noticed in section 1.3 above, in that part of the book Lindahl studies the possible effects of a decrease in the rate of interest “that is expected to endure” (STMC (1930): 162), and asserts that this will cause an increase in investment demand. Let us further inspect how Lindahl reaches this conclusion. The *immediate* impact of a decrease in the rate of interest, so Lindahl argues (*id.*: 157, 162), is an increase in capital values, which initially prompts firms to accumulate stocks of consumption goods since “it reduces interest charges on holding them” (*id.*: 157). But this is only a “temporary” (*id.*: 170) phenomenon; “Of greater importance”, Lindahl goes on to explain,

[i]s the change in productive organization, consisting in a lengthening of the investment period which is now profitable owing to the lower rate of interest. Factors of production will be transferred from the direct production of consumption goods to the production of capital goods, the relative prices of which have increased. And in the capital goods industries the newly constructed equipment will be more durable than the old while the productive process will itself occupy a longer period. (STMC (1930): 170) [¹⁰⁰]

The strong Wicksellian flavour of this passage is evident: when the rate of interest decreases investment demand rises because firms are argued to adopt more roundabout methods of production; but notice that this claim presupposes the traditional notion of capital as a single homogenous factor in its demand

¹⁰⁰ Similar considerations are found in the third part of the book, where Lindahl for instance argues that “at a lower rate [of interest] there is usually a shift in favour of earlier periods” (STMC (1929): 296).

side role, because it takes for granted that techniques can be ordered *independently* of distribution. Lindahl also relies on this traditional, albeit illegitimate, notion of capital when he inspects the influence that changes in the rate of interest will exert on the relative prices of the different consumption goods sold in the market. And he declares that when the rate of interest decreases, those goods “requiring much capital in their production” will *decrease* in price relative to other goods (STMC (1930): 181). And then, when examining the possible effects of a shift in consumers’ demand (*id.*: 201-204), he goes on to assert that, after the transition period has been completed, the rate of interest corresponding to the new secular equilibrium will be respectively higher or lower than the one prevailing before the change depending

[f]undamentally on whether the expanding industries require *more or less capital* than those that are declining. (Lindahl (1930): 204, emphasis added)

However, Lindahl seems to ignore that industries cannot be ordered according to their needs of “capital”, because this ranking will itself depend on the ruling rate of interest, and will generally change with it.

To conclude, consider now Lindahl’s analysis of increases in the interest rate (STMC (1930): 183-184): not surprisingly, he affirms that “there will be a reorientation of production to shorter investments” (*id.*: 184); but in this case, Lindahl continues,

The transfer of factors is, however, more difficult than in the movement from short to long term investments, as it takes longer time for capital to be liberated from the longer investments and to become available for co-operation with other factors in short term investments. (STMC: (1930): 184)

Lindahl here openly admits that at each moment of time, most “capital” will have taken a specific physical form; it will therefore take sufficient time for it to “be liberated” as it gradually wears out and is replaced in a different form, i.e. as it is employed in the now more profitable short-term investments on account of the increased interest rate. The point I wish to make is that, like Hayek, we find Lindahl too admitting that the change in the rate of interest will alter relative prices and convenience of alternative techniques along a number of periods, and therefore determining technical choices in a way close to traditional neoclassical long-period choice of techniques; hence, like in Hayek (1941), we find in STMC

too the idea that the influence of income distribution on investment decisions can assert itself rather slowly and only by degrees; in other words, it can only manifest itself *over the sequence* of temporary equilibrium prices, and who are assumed to evolve in the direction of long-period prices. As in Hayek, this is the actually relevant element of his reasonings, and not so much that eventually a new secular equilibrium will be reached. In this connection, we have already seen in the first part of the chapter that, according to Lindahl, “the most important effects of an alteration of the rate of interest... do not appear until after a comparatively long period of time” (*id.* : 158), and later in the book we find him again stressing that, “On account of frictions”, the transfer of factors from the consumption goods industries to the capital goods sectors when the rate of interest decreases, “can only take place *to a limited extent in each period*” (*id.*: 179, emphasis added, *cf.* also *id.*: 192).

The implication is that, although Lindahl attempts to do without the traditional notion of capital as a stock, his analysis of the influence on investment decisions of changes in the interest rate is still fully traditional, (the novelty is that now the factor substitution mechanisms are assumed to work over the sequence of temporary equilibria, in each of which each capital good is formally considered as a distinct factor), and hence it still needs the traditional notion of capital as a single factor of variable form, in this case manifested as a *flow* demand for savings. Moreover, the interest rate is still assumed to perform its traditional neoclassical role, i.e. it brings the savings-investment market into equilibrium as if things continued working in the way justified by the traditional notion of capital, the single factor measured in value terms. Little wonder then that, in his analysis of the effects of an increase in aggregate savings (STMC (1930): 204-210), Lindahl concludes that by suitable decreases in the rate of interest “the period of fruition of real capital will be lengthened” (*id.*: 207), causing an “increased volume of real capital” (*id.*), namely, investment demand will increase.

3. CONCLUDING REMARKS.

Our assessment of Lindahl’s contributions on equilibrium and capital during the 1930’s seems to reinforce many of the conclusions reached in the previous

chapter. First, while Lindahl accepts that in many branches of the economy “fairly stationary conditions prevail”, conditions that would justify the assumption of zero net accumulation, and hence the value measurement of the capital endowment when determining prices and distribution, he shows evident uneasiness with the traditional method of neoclassical theory because it is illegitimate to specify a value magnitude before distribution is known. Therefore, besides his aim to extend the supply and demand approach to a ‘dynamic’ framework, the development of the temporary equilibrium method on the part of this author can be also interpreted as an attempt to overcome those capital-theoretic “difficulties” that Lindahl (STMC (1939): 33 fn.*) himself openly admits to arise when the factor capital is measured as a single factor of variable form. We have subsequently seen that Lindahl points out several essential drawbacks of the temporary equilibrium method: e.g. the assumption that the economy is *always* in equilibrium and, especially, the *indefinite* outcomes due to expectation functions being included among the data, and that by themselves undermine the capacity of the method to have a correspondence with observation. However, Lindahl believes, these problems lose importance when the TGE method is used to explain the main determinants of prices and distribution over sufficient time. This explains why he feels authorized to use the TGE method to study transitional dynamics between secular equilibria. Therefore, secular equilibrium prices (prices that in STMC yield a uniform return on the supply price of capital goods) by providing the long-run direction of change of the sequence of temporary equilibria, still bear a higher theoretical hierarchical position than the temporary equilibrium prices that are assumed to come about within the single analytical period, in each one of which the vectorial endowment of capital goods is included among the data. So, in STMC the sequence of TGE eventually brings about traditional outcomes, i.e. long-period or normal prices, along Austrian lines: changes in the rate of interest induce firms to change the degree of roundaboutness of the cost-minimizing methods of production, *as if* things continued working in the way justified by the traditional conception of capital, the single magnitude in value terms. The main difference with traditional neoclassical capital theory is that now the factor substitution mechanisms are assumed to be operative over the sequence of temporary equilibria. But as noticed above, the fact that there is a TGE in each

period is hardly important: Lindahl admits that a TGE is seldom established, so he accepts that most of the time the economy is in disequilibrium even relative temporary equilibria. Then, the belief in the tendency towards a final position of secular equilibrium is even more necessary to him, and can only rest on traditional beliefs about disequilibrium adjustments.

In a nutshell, like Hayek, the Swedish author makes a 'logical jump': in order to assert the long run tendencies about distribution and growth, Lindahl introduces traditional gravitational ways of reasoning into neo-Walrasian theory; but this is clearly illegitimate: those modes of explanation *presume* a conception of capital as single scalar measured in value, which is clearly incompatible with the neo-Walrasian approach, since it treats each capital good as a distinct factor of production. This jump allows Lindahl to preserve the essential features of traditional neoclassical economics within the temporary equilibrium framework, namely, to justify with some plausibility the negative interest-elasticity of investment demand; this ensures the stability of the savings-investment market, and therefore, it justifies the traditional neoclassical claim that a full-employment growth path is a position that the economy tends to realize over a sufficient interval of time.

4.

HICKS'S *VALUE AND CAPITAL* (1939)

INTRODUCTION.

This fourth chapter reviews Hicks's *Value and Capital* (hereafter, VC), no doubt the most influential contribution among the first generation of neo-Walrasian writings. The chapter is divided in three main sections. The first section attempts to establish that Hicks's writings during the early 1930's can be considered as fully-internal to traditional neoclassical economics. Hicks therefore appears to be clear that an equilibrium that is to be conceived as a centre of gravitation of actual variables must be a position that is reached only after sufficient time, and that this requires an endogenous determination of the capital stock. Moreover, Hicks's readings of Clark and Robbins suggest that he is also sufficiently clear on the difference between static (or long-period) and secular equilibria to understand that the founders of the marginalist approach argued the correspondence between theory and observation on the basis of the former and not of the latter. It emerges as a consequence that the explicit reasons adduced in VC to dismiss the traditional neoclassical method of analysis, namely, that this approach would "condemn" the theory to study secular equilibria, do not seem to stand close scrutiny. I will argue in this connection that the identification of the traditional notion of equilibrium with a secular equilibrium state allows Hicks i) to argue the *consistency* of that theory, in the sense that a secular equilibrium can be determined without mentioning the traditional notion of capital *at all* and, at the same time, ii) to argue the irrelevance of that theory, and hence the need to *extend* it in the direction of the temporary equilibrium method. In the second section I proceed to argue that, while on the one hand Hicks admits that the new TGE method essentially assumes instantaneous equilibration, on the other hand, the British author does not actually abandon the notion of equilibrium he has recourse to in his early writings, i.e. a position that the economy tends to realize after sufficient time. In fact, the main contribution of this chapter consists of addressing the thesis according to which in the fourth part of VC, Hicks recants of some of the

distinctive features of the TGE method, namely, the assumptions of instantaneous equilibration and of market clearing even in the single temporary equilibrium, and resumes the idea that the forces of supply and demand are able to assert themselves rather slowly, and hence that a position of full-employment equilibrium can plausibly emerge only over a sufficient interval of time, as if things continued working in the way justified by the traditional conception of capital. Little wonder that, as I show in the third part of the chapter, the traditional notion of capital in its demand side role is still behind the mechanisms that Hicks relies on to justify the tendency towards equilibrium.

1. EQUILIBRIUM AND THE SPECIFICATION OF THE ENDOWMENT OF CAPITAL IN HICKS'S WORKS.

1.1. Hicks early writings and the traditional conceptions of equilibrium and capital.

1.1.1. The plausibility of the factor substitution mechanisms.

In some of his early 1930's contributions, specially "*The principle of variation*" (Hicks, 1932a) "*A reply*" (Hicks, 1932b) and *The theory of wages* (Hicks 1932c), Hicks shows clear awareness that the conception of equilibrium as a position that the economy tends to realize over sufficient time *needs* treating the composition of the capital stock as an *outcome* of the adjustment process. Hicks's discussion involves two main issues that are rather intertwined in those writings. The first one entails the *plausibility* of the factor substitution mechanisms under a given stock of capital goods, while the second is related to the very possibility to *determine* an equilibrium position when the endowments of capital goods are included among the data of the equilibrium, as e.g. in Walras's system of equations. The first issue, which points out to what we have called the "substitutability problem" (*cf.* chapter 1) has been discussed by Garegnani (1976), Petri (1991) and more recently by Trabucchi (2011). In those early contributions Hicks seems to be sufficiently clear that, given that different techniques will generally call for *qualitatively* different capital goods into use, the plausibility that profit-maximizing firms can vary their demand for factors when income distribution changes, and on which the notion of downward-sloping factor demand curves, and hence the whole neoclassical edifice, are ultimately assumed to rest, requires the physically heterogeneous capital goods

to be determined by the conditions of the equilibrium. Speaking about the plausibility of the notion of marginal product, Hicks asserts:

In the extreme case where the quantities of *all* the factors required to give a unit of product –the coefficients of production as Walras calls them- are given by technical facts, the marginal productivity breaks down completely. (Hicks, 1932a: 81, emphasis in the original)

The reference to Walras here is not surprising, since as is well known the French scholar treats each capital good as a distinct factor of production. The point Hicks makes is that, given the specificity of capital goods in production, when each capital good is treated along Walrasian lines, the possibilities of substitution will be in fact very limited, and therefore, “an extra unit of a factor, unaccompanied by an increase in the other, will yield precisely no addition to the total product” (*id.*), namely, the marginal product of the factor would be zero, implying therefore a zero rental price in equilibrium. On the same footing, when one unit of the factor is subtracted, “its removal would put corresponding units of the other factors out of action” (*id.*). If the marginal product of the factor were calculated in this second manner, Hicks concludes, “their total pay would undoubtedly be far in excess of the goods they produced. *Which is absurd*” (*id.*, emphasis added). Hicks here points out the possible indeterminacies in the rate of wages predicted by the theory had the marginal product of labour been calculated by treating each capital good along Walrasian lines; these indeterminacies would no doubt undermine the usefulness of the supply-and-demand forces to account for the trends in income distribution. So Hicks writes in this respect:

For the lower limit to the wages which can be paid is zero; and the upper limit is the total incomings of the enterprise. And to know that the total wage-bill must lie between these limits we do not need to have recourse to elaborate analysis! (Hicks, 1932: 81)

Consider also the following example that is given in Hicks (1932b)

When it is a question of pricing the services of a typewriter, the rule ‘one typewriter - one typist’ is important. The possibility of variation is very limited, and one cannot say that the price for which a typewriter is hired will tend to equal its marginal product. (Hicks, 1932b: 298)

“But this is not the way in which most economists have desired to use the marginal productivity theory” (*id.*), Hicks goes on to argue, because “The theory

is generally applied to the pricing of the factors of production.” (*id.*); and by factors of production, Hicks means not only labour and land, but also

[c]apital; and capital is capable of transference from one form into another. (Hicks, 1932b: 298)

“The true factors of production”, Hicks in fact asserted earlier, are “land, labour and *money capital*” (Hicks, 1932a: 86). So, to justify how the physical composition of the capital stock is to be determined in equilibrium, Hicks unproblematically speaks of the traditional neoclassical notion of “capital”, the single homogeneous factor measured in value terms that can take *different forms* without significantly changing in quantity. It is this indispensable, albeit illegitimate, notion what gives plausibility, in capital-goods producing economies, not only to the mechanism of direct factor substitution, and hence to the concept of marginal product, but also to the working of the indirect factor substitution mechanism; and the action of both mechanisms plausibly justify the negatively-sloped demand for factors of production, on which the supply-and-demand explanation of distribution is ultimately assumed to rest. Hicks’s (1932a: 82-83) justification for the downward sloping demand for labour is in fact fully traditional: if wages rise, he argues, there will be a contraction of those industries which use more labour per unit of capital, and an expansion of those industries which use more capital. The net effect will be a decrease in total employment because

[i]n the more capitalistic industries the amount of labour needed to use a given quantity of capital is less than it is in the rest, the transferred capital in its new position absorbs into employment less labour than had been thrown out by its withdrawal. There is net unemployment. (Hicks, 1932a: 83)

The opposite effect will take place when wages decrease, leading therefore to a “rise in the demand for labour” (*id.*). Clearly, given the specificity of the capital goods used within each industrial sector, the amount of capital that is “transferred” and employed in its “new position” cannot but take a physically *different* form, implying therefore the endogenous nature of the endowments of capital goods that will be produced *after* the change in wages takes place. In sum, from Hicks’s discussion of the working of the substitution mechanisms two main conclusions can be drawn: the first one is that Hicks is clear that the

endowments of capital goods cannot but be endogenously determined variables. And the second conclusion is that this endogenously determined composition of the capital stock needs a single scalar endowment of the factor capital, which is capable of changing form without significantly changing in quantity.

1.1.2. Walras's inconsistency.

The other issue raised by Hicks in those early writings is perhaps less well known (*cf.* however Trabucchi, 2012 for a detailed discussion): Hicks explicitly accuses the theory of capital developed by Walras and Pareto of not being capable to determine a proper *equilibrium*, because the solution to the Walrasian equations of “capital formation” describes in Hicks's view a position from which the economy tends to *move away*. It is “perfectly possible”, so Hicks writes,

[t]o conceive of a community in which all Pareto's equations are satisfied, but which is *not in equilibrium*. (Hicks, 1932a: 85, emphasis added)

And he continues:

For so long as the n commodities which are being produced are *arbitrarily given* commodities, each individual may have reached his preferred position within that charmed circle, but he may still have the incentive to move outside it (Hicks, 1932a: 85, emphasis added)

While here Hicks speaks of ‘commodities’ in general (capital goods and consumption goods), at the heart of his critique to the theory of capital of Pareto and Walras there emerges the specific problem of the *arbitrarily given* endowment of capital goods. In fact, Hicks refers to the capital goods industries to illustrate “the incentive” that entrepreneurs would “have to move”. He writes:

It would be possible for the machine-makers, say, to modify slightly the character of the goods they produce, so as to meet the needs of their customers more exactly. By so doing, it would be possible for them to sell their new goods at a price which would yield them a greater profit than they had earned before; while on the other hand their customers would be better satisfied, for they in turn would also earn larger profits. (Hicks, 1932a: 86)

The main deficiency that Hicks seems to detect in the Walras-Pareto's approach to capital is that it *assumes*, but only “for mathematical convenience” (*id.*: 85), that those goods “with which the market need be concerned are those goods which are actually being produced” (*id.*), where by “goods being produced” Hicks clearly means those capital goods that are *arbitrarily* included among the

givens. According to this theory, Hicks objects, the entrepreneur is “*limited*” (*id.*) to choose “specialized articles, made for specialized use” (*id.*). But this assumption, he counters, has no “absolute economic significance” (*id.*): had the capital goods produced been qualitatively different from those that are initially included in the “arbitrarily restricted list of possible commodities”, “*larger profits*” would be obtained by entrepreneurs, revealing therefore the state of disequilibrium in which the economy initially finds itself^[101]. “A system will not be in equilibrium”, Hicks in fact wrote earlier,

If any advantage is to be gained by transferring resources between industries (Hicks, 1932a: 83).

And this is precisely what Walras’s equations seem to show according to Hicks: the convenience of producing different kinds of capital goods from those that are arbitrarily given, namely the convenience of “transferring resources between industries” ^[102]. It is no doubt true that in those early writings Hicks does not

¹⁰¹ It is important to notice that in Hicks’s view, Walras’s equations would not be able to determine an equilibrium position only due to the lack of adaptation of the *qualitative* composition of the capital stock to its forthcoming demand; he seems to disregard however that this same impossibility to determine an equilibrium would emerge even when the kinds of capital goods that must be produced in equilibrium are qualitatively identical to those initially available but, yet, the equilibrium composition of the capital stock defers from the quantities of capital goods that are initially given.

¹⁰² It would seem at first sight that the problem that Hicks detects in Walras’s theory of capital could be simply removed by incorporating, within that theory, the existence of different methods of production that allow which capital goods can be produced in equilibrium. Hicks himself suggests this possibility in his “Reply” to Schultz. As Hicks writes: “we should need to introduce, as well as the n commodities, intermediate and final, which are actually produced, a much larger set of possible commodities, which are not produced, because something else is preferred to them, at the lowest prices at which they could be offered.” (Hicks, 1932b: 299). This suggestion, however, does not appear to be satisfactory. In fact, the problem remains that, if the capital goods that are being produced are arbitrarily given, the inclusion of all the different methods of production can only show the convenience of producing *different* capital goods from the already existing ones, therefore confirming that the state of disequilibrium of the economy (*cf.* Trabucchi, 2012: 17-19). Moreover, Hicks seems to detect a second, more methodological, issue with the inclusion of all the possible methods of production, and hence the different kinds of capital goods, in the Walrasian equations. The issue is addressed in a subsequent contribution of Hicks (1979: 57-58), but it may suggest way in those early writings Hicks does not develop his own suggestion of including all the possible methods in the Walrasian equations in extense. In this subsequent contribution, Hicks argues that, in case of factor-price changes, it will generally take a considerable period of time for entrepreneurs to *discover* and *adopt* the new optimal techniques; he points out that it may even well be the case that the cost-minimizing technique that will be eventually adopted is not originally available. So, in Hicks view, the assumption that all possible methods of production are originally available and must simply be implemented by the entrepreneur seems to leave little room for the process of *discovery* that takes place in the market. Hicks writes: “The fixed coefficients... could be taken to be rather *firm*; the neo-classical Production Function’ is by no means so firm. It is supposed to be the expression of a given

identify the deficiencies behind Walras's treatment of capital with the clarity that we will later find in Garegnani (1960)^[103]; however, the previous passages strongly suggest in any case that, at that time, Hicks does appear to grasp with notable understanding the main inconsistency behind Walras's theory: the contradiction between the inclusion of an arbitrarily given set of capital goods among the *data*, and Walras's aim –common to all authors of his time- to determine a solution that is at the same time an equilibrium solution; i.e. a position where the composition of the capital stock has been so adjusted that the return on the supply prices of the capital goods actually produced is uniform across industries. This contradiction is in fact explicitly point out by Hicks when, he writes to Schultz:

Any particular set of prices of the factors of production, determines not merely the quantities of factors demanded, and the quantities of intermediate and final products produced, but also the *kinds* of intermediate and final products. (Hicks 1932b, 299, emphasis in the original)

1.1.3. The relative persistence of the data.

The previous discussion showed that Hicks criticizes Walras's theory of capital on the basis that it would not be capable to determine an equilibrium solution because, even if Walras's equations were satisfied, "entrepreneurs could still have an incentive to move". The point I wish to make in this section is that, in Hicks's view, the conditions that ensure that the incentive "to move" on the part of the entrepreneurs is absent must not be mistakenly confused with a situation where there is no incentive to move at all, namely a situation where

'technology', a set of techniques which *might be* employed, that actually employed being one of them. But *might be* in what sense? It may be granted that techniques do respond, sometimes at least, to changes in factor prices; but what usually happens is that a change in relative factor prices stimulates a search for a new technique which shall economize in that factor which has become relatively scarce. In the course of that search a new technique may well be discovered which it would have paid to adopt even though the factor price change had not occurred. Such a technique, we are taught, should not be regarded as belonging to the old technology; its introduction would mark an *invention*. But is not the new technique, introduction of which would not occur without the factor-price change, but which was not known before the factor price change, equally an invention? If we include such techniques in the original technology, are we not drawing its boundaries too wide? But if we exclude them, are we not drawing them too narrow? A Production Function, the technology of which includes no more than known techniques, leaves little scope for the adjustments which for marginal productivity are essential, even when that is taken in the strictest comparative statics sense."

¹⁰³ For instance, as argued in fn. 101, Hicks does not seem to grasp that even the capital goods that must be produced in equilibrium are qualitatively identical to those initially available, Walras's system will in any case be generally unable to determine a position where all capital goods yield the same return on their supply price.

nothing is changing. In other words, that Hicks is clear that the version of neoclassical he is eagerly defending does not assume *strict* constancy of the data, in particular of the quantity of capital, but only its persistence relative to the time-consuming adjustment processes. A clear indication that this is in fact the case emerges in the *Reply* to Schultz, where Hicks observes that the principle of marginal productivity under discussion “is an issue of economic *statics* in Clark’s sense”, and the equilibrium conditions thus determined

[a]re similar in form *whatever assumption* we make about saving. It is thus perfectly natural, when one is discussing marginal productivity, to *assume* constant *supplies of the factors*, as being the simplest assumption one can make on this essentially *irrelevant matter* (Hicks, 1932b: 297, emphasis added).

Clearly, the *static* version of the theory Hicks defends simply *assumes* the constancy of the factors of production when determining the equilibrium position of the economy, rather than determining these quantities as endogenous magnitudes. The reference to “Clark’s statics” therefore strongly suggests that in those early writings, Hicks’s method was fully traditional: in particular, capital accumulation is simply *assumed* to be zero, arguably because the speed of change of the total quantity of value capital observed over sufficient time is so slow relative to the process of adjustment of disequilibrium prices towards their equilibrium magnitudes that the problems connected with capital accumulation can be safely ignored when the aim is to explain the determinants of prices and distribution in its utmost generality. Little wonder then that Hicks writes that capital accumulation is “essentially an irrelevant matter” for the problem under discussion. The important implication to be drawn from this discussion is that, in the versions of neoclassical theory that Hicks is promptly defending here, which encompass time-consuming productions and transactions, the quantity of capital is not treated as an endogenously determined magnitude but it is rather considered as part of the givens of the theory, and therefore, the equilibrium determined is a static and not a secular stationary equilibrium state.

Further confirmation that in those writings Hicks speaks of the traditional, static notion of stationariness comes from a subsequent article of his, “*Equilibrium and the cycle*” (1933). In the first paragraphs of this article, Hicks

discusses in some detail the conditions necessary to ensure the determinacy of the equilibrium under realistic, time consuming adjustments in capital-goods producing economies. According to Hicks (*id.*: 31), the hypothesis of time-consuming adjustments could be plausibly argued under stationary conditions with not net saving. While Hicks asserts that this is a “significant limitation” of the analysis, the little noticed point I wish to make is that in footnote 6 of that article Hicks refers not only to the authority of Marshall, but also to Robbins’s (1930) article to argue that despite its “high degree of abstraction”, the notion of stationary equilibrium is in any case of “fundamental importance” (*id.*: 31, fn. 6). Now, if recall from the first chapter (section 4), the aim of Robbins’s article is precisely to distinguish between the notions of static and secular stationary state, and to defend the former concept against the later as a tool to explain the trends in actual economies. No doubt then, the notion of stationary equilibrium that Hicks considers to be of “fundamental importance” is the traditional notion of static, rather than secular, equilibrium. We shall return to this point in section 1.2 below, when we examine how Hicks reconstructs the traditional versions of neoclassical theory of capital in *Value and Capital*.^[104]

1.1.4. Adjustments take time.

We have seen in the previous paragraphs that Hicks is sufficiently clear that a strict constancy of the data is not actually necessary for the traditional method to be capable of indicating with sufficient approximation the position where the economy tends. The fact that real world adjustments are time consuming, and that equilibrium is reached only after sufficient market rounds by trial and error and experimentation only implies that the data of the equilibrium must be persistent. In this connection, it is worth documenting a little mentioned article, “*Edgeworth, Marshall and the indeterminateness of wages*” (1930), where

¹⁰⁴ It is important to stress that the fact that here and there Hicks shows some concerns with the notion of static stationary equilibrium that are independent of the problem of the value specification of the capital endowment of the economy, and that in those early writings he suggests that the dynamic method could be a possible way to “get around” with the notion of stationary equilibrium, does not in the least imply that, at that time Hicks was willing to abandon the traditional method of analysis as a general framework to explain the main determinants of prices and distribution. And in fact, despite showing some uneasiness with respect to the hypothesis of zero net savings in Hicks (1933), in the “*Leon Walras*” paper published one year later, Hicks claims that the stationary assumption is a *necessary* condition for equilibrium, since “it is only in a stationary state that we can get any sensible sort of equilibrium” (Hicks, 1934: 346), so long as prices are expected to remain fairly steady, as it will be the case when the change in the quantity of factors of production is slow enough to be neglected.

Hicks discusses the time-consuming nature of actual equilibration processes, hence the need of persistent data. In that contribution, Hicks attempts to refute an argument raised by Marshall about the possible indeterminateness of wages, according to which, owing to the higher bargaining power of the “buyers” of labour, wages may end up being low simply because they were initially low^[105]. “But if we suppose multiplicity of employers and workmen, free communication and a competitive market”, Hicks counters, under an “ongoing market” the

[t]erms which were fixed in the first week to the workmen's disadvantage will be subsequently modified by the employers' mutual competition, by some employers endeavouring to take on more men. Wages will thereby be bid up to the normal value of the labourers' marginal net product, and the first disadvantageous contract will have no effect in depressing them. (Hicks, 1930: 226)

Notice how Hicks stresses here that it is only through the *repetition* of transactions that, albeit implicitly, are clearly assumed to take place under essentially the *same* conditions- that the competition between buyers and sellers of labour will eventually correct the initial disadvantage of the buyers by bidding up wages to the “*normal* value of labourers' marginal net product” (*id.*) and, therefore, that the alleged indeterminacy of wages disappears. Hicks thus remarks the need for an “ongoing market” so that, only after sufficient time, competition can ensure that the marginal productivity principle will emerge as the law that regulates the normal value of wages. Only over sufficient market rounds, Hicks notes, “the first disadvantageous contract will have *no effect in depressing wages*”, because the causes that have keep wages low in the first periods –e.g., since competition takes time to operate, in the first periods there might be imperfect information that possibly justifies the higher bargaining power of the buyers- are essentially transitory causes that will disappear with time, and can thus be legitimately disregarded in long-run analysis; only the more persistent causes –i.e. those objective conditions that determine the marginal product of labour- will influence the average wage level in the long-

¹⁰⁵ Hicks summarizes Marshall's position in the following way: “Once we assume a continuous market, with production and consumption going all the time, with contracts being made afresh under certain conditions, then it is true that Marshall's statement would, under certain conditions, be formally justified. In a single contract (for a single week's work day, say) the lowest rate which the labourer will be prepared to take will be largely governed by the marginal utility of money to him; and since that marginal utility will be high, the rate will be low. When contracts take place in subsequent weeks, the low wages earned will have done no more than satisfy the labourer's most immediate wants and will have left him without any reserve on the basis of which to refute the bad terms offered. He will 'go on selling his labour at a low rate', for the marginal utility of money to him is no less than before” (Hicks, 1930: 226).

run. In a nutshell, the persistence of the data is therefore implicit all along Hicks's argument, because Hicks is aware that real adjustment processes take time to assert themselves and, therefore, only by a trial-and-error process of equilibration that takes place under essentially unmodified conditions, the marginal product of labour will be able to reflect, again, as an average, the normal or equilibrium value of wages^[106]^[107].

1.2. Hicks's misgivings about the traditional concept of equilibrium in Value and Capital.

While in subsequent writings Hicks will again acknowledge that the traditional concept of equilibrium is not a *secular equilibrium* ^[108], in the second half of the 1930's, Hicks holds a different position: we find him stressing that neoclassical theory is "incompetent to deal properly with capital or interest" (Hicks, 1939: 119) and would be allegedly "condemned" (1935: 435) to study secular equilibria. "How will the quantity of intermediate products –the quantity of capital- be determined?", Hicks wonders; according to the reconstruction of traditional theory we find in VC, the answer is that

It turns out to be determined by the rate of interest. A fall in the rate of interest would encourage the use (at any moment) of larger quantities of intermediate products. But since we are in a stationary state, there can be no tendency for the stock of capital to increase or diminish; constancy of the stock thus gives one relation between its size and the rate of interest. Also, if entrepreneurs do not desire to increase or diminish their stock, their net borrowing must be nil. If the demand and supply for loans are to be in equilibrium, net saving must therefore be nil. The rate of interest must therefore be fixed at a level which offers no incentive for net saving or dis-saving. What this level is depends partly upon the propensities to save of the individuals composing the community, partly upon their real incomes – and these depend again upon the size of the stock of intermediate products. We therefore have two equations to determine the size of the stock of capital goods and the rate of interest; consequently, both are determined.

¹⁰⁶ In Hicks (1934: 343) it is again stressed the need to assume persistent data in order to plausibly justify time-consuming adjustments (*cf.* Petri, 1991: 273-274).

¹⁰⁷ For completeness sake, it is worth noting that in those early writings Hicks also notes what we have called the 'indefinites problem' (chapter 1, section 5) relative to expectations. This problem is clearly addressed in his review of Keynes's *General theory* (e.g. Garegnani, 1976: 140; Petri, 1991: 274-275)

¹⁰⁸ Specifically, in *Causality and Economics*, Hicks (1979: 55-56, emphasis added) writes: "So it is not surprising to find that later economists (the so-called neo-classics) had a static equilibrium which was different [from Ricardo's secular stationary state]. Being less impressed than Ricardo by the population peril (and having learned by experience that growth in labour supply is a more complex matter than can be reduced to dependence upon a single variable) they found it wiser to *treat the supplies of the factors, not only labour but also capital, as exogenous variables*. That is to say, they would be changeless over time, for the model would still be a static model; they would vary from one state of the model to another (the hypothetical states still needed for analysis of causation) but why they varied would not by the model be explained."

The theory thus boldly summarized is a plausible theory of a stationary state; unfortunately, it is only a theory of a stationary state. It is only on very special conditions that savings and investment will both=0. (Hicks, 1939: 118-119)

However, in view of the evidence provided in the previous section, it seems legitimate to wonder about the reasons that led Hicks to argue in VC that traditional neoclassical theory would have envisaged the constancy of capital as a *resultant* of the process of equilibration, rather than a variable that should be included among the data of the equilibrium. In this respect, it has been originally suggested by Garegnani (1976), and then convincingly argued by Petri (2004: 121) and Trabucchi (2006), that this new (and mistaken) interpretation allows Hicks “to kill two birds with one stone”, so to speak. On the one hand, this reconstruction allows Hicks to dismiss the *relevance* of those versions of neoclassical theory on the basis that actual economies are far from being stationary, and that there is no actual tendency towards a secular equilibrium state (on this last point, see section 2.1 below). On the other hand, the adduced identification of Böhm Bawerk’s, Wicksell’s, etc, notion of equilibrium with a secular stationary state also *does away* with the illegitimate treatment of capital that emerges in those versions of the theory, and that is forcibly needed within the neoclassical approach to prices and distribution to determine the traditional, static, notion of equilibrium, i.e. a position that is not strictly stationary, but only persistent enough to be capable of having the role of a centre of gravitation of actual magnitudes. In other words, the reconstruction offered in VC also allows Hicks to *skip* the discussion about the problems derived from the inclusion, in traditional neoclassical theory, of a value endowment of capital among the givens (a problem of which Hicks is certainly aware owing to Shove’s harsh critique of *The theory of wages* in regards to the measurement of the endowment of capital^[109]), a discussion which would have

¹⁰⁹ Shove (1933: 470, emphasis added) summarises his strong critique of Hicks (1932a) in this way: “It must suffice to indicate what seem to be the main sources of obscurity. The central thesis is this: that if wage-rates generally are forced above ‘the competitive level’ (whatever exactly that may be), unemployment will be caused in two ways: (i) by ‘the tendency for capital to shift from the less capitalistic to the more capitalistic trades’ (and methods) (id: 187-8), i.e., to ‘those which use a relatively large proportion of capital to labour in making a unit of product’ (id: 187), from those which use a relatively small proportion[;] (ii) because ‘the total supply of capital’ will be diminished (id: 193), since capital will be ‘lost’ (id), ‘eaten into’ (id), ‘consumed’ (id: 199), ‘destroyed’ (id), ‘cut into’ (id: 200), ‘dissipated’ (id: 206) or ‘decumulated,’ and ‘savings’ therefore checked (id: 193). Unfortunately ‘capital’ is not defined and we are not told how quantities of it are to be measured, and similarly of ‘saving’. Presumably, these are ‘matters which properly belong to the theory of capital’ (id: 200).”

inescapably pointed to the specific problems of that theory, and not to the traditional neoclassical method of analysis based on the concept of static or long-period equilibrium.

To sum up, the mistaken reconstruction of the traditional neoclassical approach we find in VC seems to offer Hicks the possibility to argue the *consistency* of that theory (in the sense that a secular equilibrium can at least be determined), while at the same time allows him to argue the *need* for an *extension* of that theory, i.e. the need to develop a framework that covers more general and realistic cases than the rather irrelevant situation of a secular equilibrium state. As is well known, the temporary equilibrium method is the direction in which Hicks suggests that the extension of traditional theory should proceed.

2. THE REHABILITATION OF THE TRADITIONAL CONCEPTION OF EQUILIBRIUM IN VC.

2.1. Hicks's uneasiness with the notion of secular equilibrium.

Having wrongly identified the traditional neoclassical notion of equilibrium with a secular equilibrium state, Hicks proceeds to dismiss the relevance of this latter concept on the grounds that there would be no actual tendency towards stationariness.

Although it would always be recognized that the actual state of any real economy is never in fact stationary, nevertheless stationary-state theorists naturally regarded reality as 'tending' towards stationariness; though the existence of such a *tendency is more than questionable* (VC: 119, emphasis added)

On this point, Hicks's position differs from Hayek's (1941) and Lindahl's (1939); recall that in our excursion to these scholar's contributions we have shown that, even though they also misidentified static with secular equilibria, they do not essentially question, however, that the economy will eventually approach a secular-equilibrium state. Hicks on the contrary considers only two equilibrium concepts as theoretically relevant: the temporary equilibrium (TGE) and the notion of "equilibrium over time", i.e. a perfect foresight equilibrium, envisaged as a kind of benchmark to compare against actual economic states, and to measure "the extent to which expectations are cheated, and plans go astray" (VC: 132). In Hicks's view it will be highly unlikely to observe a situation of

equilibrium over time because anticipations of the future are usually incorrect – *except*, he adds, “when prices are fairly steady” (VC: 133). Indeed, only under fairly stationary conditions it is plausible to assume that price expectations will be approximately realized, and agent’s plans will be mutually compatible on average. However, given Hicks’s misgivings about the (secular) stationary state, he envisages this possibility as a rather irrelevant and uninteresting case, and hence goes on to argue that there is no need to pay special attention to it.

But if a situation of equilibrium over time is to be realized only when “prices are fairly steady”, but, however, the tendency towards such fairly stationary equilibrium is “more than questionable”, Hicks’s new TGE method would only be capable to determine, if anything, the prices and quantities that will prevail within the first temporary equilibrium period, since the myriad of accidental events that may affect the economy will likely disappoint current expectations about future conditions. As a result, those conditions prevailing in the initial period become irrelevant to predict the evolution of the economy in the longer run. But Hicks sees those long-run trends as the actually relevant object to be explained by the theory. Consider for instance what he writes in this respect in chapter XX, “Temporary equilibrium of the whole system”:

Even when we have mastered the ‘working’ of the temporary equilibrium system, we are even yet not in a position to give account of the process of price-change, nor to examine the ulterior consequences of changes in data. *These are the ultimate things we want to know.* (VC: 246, emphasis added)

Therefore, Hicks’s analysis can not easily abandon the traditional role assigned to equilibrium: a position that is sufficiently persistent to indicate where the economy is tending over sufficient time. Interestingly, as we argue below, this traditional notion of equilibrium seem to emerge in VC when the author attempts to justify the stability of the single temporary equilibrium, i.e., even before he proceeds to study those “ultimate things we want to know”. The remainder of this section will be devoted to analyse this issue, which seems to have been little noticed by the literature so far^[110].

¹¹⁰ Gehrke (2003: 100) is an exception: in the 2003 article the notion of “normal price” to be discussed below is very briefly mentioned.

2.2. The “not very convincing” hypothesis of instantaneous equilibration.

But in order better to understand the reasons behind Hick’s de facto rehabilitation of the traditional conception of equilibrium as a position that is reached only over sufficient time, we must first notice that from the outset of part IV of *Value and Capital*, where the TGE framework is presented and developed, Hicks shows clear awareness that the lack of persistence of the data of the TGE forces the method to assume an essentially *instantaneous* equilibration process. Although Hicks initially refuses to adopt Walras’s “not very convincing” (VC: 128) hypothesis of “non-false trading”, in any case he ends up assuming that markets will “proceed *quickly* and *smoothly* to a position of temporary equilibrium” (VC: 123, emphasis added)^[111], arguing that those simplifications “do not make very much difference to the sort of results we may expect to obtain by our analysis” (*id.*) and, therefore, he *de facto* adopts the hypothesis of *instantaneous* equilibration. Indeed, he continues,

Since we shall not pay much attention to the process of equilibration which must precede the formation of the equilibrium prices, our method seems to imply that we conceive of the economic system *as being always in equilibrium*. We work out the equilibrium prices of one week, and the equilibrium prices of another week, and leave it at that. (VC: 131, emphasis added)

¹¹¹ In *Note to chapter IX* he attempts to dismiss the problem of path dependence that may likely emerge under time-consuming adjustments by arguing that the effect of out-of-equilibrium transactions can be safely neglected because “very false prices are limited in volume”. As Petri (1991: 279-280) has noticed, Hicks appeared to see the income effect due to false trading as simply redistributions of a given total income, ignoring the fact that when production is allowed, there will also be deviations of the total income from its full-employment equilibrium level. In any case, Hicks himself is not at all convinced by his own assumption, as it is admitted in the *Additional Note C* of the second edition. There Hicks eventually acknowledges that the assumption of “‘an easy passage to temporary equilibrium’ required more justification when it is applied to my problem of many markets that it did when applied to Marshall’s case of the single market” (VC: 336). In this regard, also in the *Note of chapter IX* Hicks admits that there are substantial differences between Marshall’s partial system and his own that made his conclusions regarding the unimportance of perverse income effects much more implausible than Marshall’s. In fact, while Hicks deals with a general equilibrium framework, Marshall studied one single market at a time and, therefore, Marshall could assume *i*) a constant marginal utility of money and that *ii*) the individual’s “expenditure on the commodity is small”, which, according to Hicks (VC: 128-129), is tantamount to assuming a small income effect. As Hicks notes, these two assumptions are not available in a general equilibrium framework. Moreover, one should also notice that Marshall envisaged the process of convergence towards a temporary equilibrium under the assumption that individuals are more or less able to accurately anticipate the equilibrium price. As Petri (1991: 279) has noticed then, due to these restrictive hypotheses, Marshall was ready to concede that his conclusions were devoid of “general applicability”.

And in the *Additional Note C*, Hicks again remarks that his TGE analysis presupposes an “essentially instantaneous adjustment” (VC: 337). Hicks’s hypothesis of very-fast adjustments towards equilibrium is quite striking, if not for the absurdity of neglecting his own early concerns about the importance of trial and error and search in the process of equilibration –and hence of persistent data relative to the adjustment processes- simply because Hicks himself stresses in VC that

[a]djustments needed to bring about equilibrium take time. (VC: 116)

These claims notwithstanding, Hicks knows that he can hardly leave the issue of the tendency towards equilibrium “at that”: the lack of any consideration to the stability of the equilibrium would deprive the analysis of any plausibility. The author thus attempts to study the “laws of the working of the temporary equilibrium system” in chapters XX-XXII; the main conclusion that comes out of that discussion is that *elastic* price expectations can possibly jeopardize the stability of the system^[112]. The reason can be intuitively grasped in the following way: if, e.g. there is a general excess demand for commodities, the average price level will rise and demand will decrease, thus correcting the disequilibrium situation. If, however, people expect future prices to rise even further, the increase in money prices will prompt a further rise in current demand, rather than a decrease, as the tendency towards equilibrium requires^[113]. But along the discussion of the tendency towards a “static” equilibrium (i.e. an equilibrium without production of capital goods, VC: part II) Hicks already argued without any solid justification that, while “it cannot,

¹¹² “A system with elasticities of expectations greater than unity... is highly unstable” (VC: 255).

¹¹³ At this juncture, it might be also worth stressing that Hicks does not limit himself to point out the possible destabilizing role of expectations. He also comes close to admitting the *indefiniteness problem*. Indeed, Hicks (VC: 204) enumerates three types of factors that might influence price expectations. The first one is “entirely non economic” and consists of “the weather, the political news, people’s state of health, their psychology” (*id.*). As regards the second force, while it is an economic factor, it is not “still closely connected with actual price-movements; it will include mere market superstitions, at the one extreme, and news bearing on future movements of demand and supply (e.g. crops reports), at the other.” Finally, the third force “consists of actual experience of prices, experience in the past and experience in the present” (*id.*). Hicks concludes that “it is this last [force] about which we can find most to say” (*id.*) while with respect to the first two factors, Hicks admits that “changes in price-expectations which result from either of the first two sorts of influence have to be treated as autonomous changes” and hence, that the “*current economic situation may perhaps react along these channels in mysterious and indirect ways; but we cannot hope to do anything about it*” (*id.*: emphasis added)

indeed, be proved a priori that a system of multiple exchange is necessarily stable... the conditions of stability are *quite easy conditions*, so that it is quite reasonable to assume that they will be satisfied in almost any system which we are likely to be concerned” (*id.*: 72-73, emphasis added). Little wonder then that this faith in the tendency towards a supply-and-demand equilibrium prompts Hicks to find, in this case within the TGE method, “sufficient factors of stability to enable [the TGE system] to work” (*id.*: 271). Moreover, Hicks argues, these modifications would also make the original TGE model more “realistic” (*id.*: 258).

2.3. The “factors of stability”: normal prices and wage rigidities

2.3.1. Normal prices.

Hicks proceeds to suggest some possible ‘stabilizers’, of which two will be particularly important to us: the notions of “*normal price*” and of “*price rigidities*”. As it is argued in this and the following sections, these concepts however are quite in conflict with the TGE framework. As regards the notion of normal price, Hicks notes that, as long as individuals have a relatively accurate idea about the ‘normal’ price of a certain commodity, price expectations will remain fairly steady and, therefore, a sudden change in prices will not exert a destabilizing influence on the economy: people will simply expect prices to return to their old, *normal*, level. Hicks indeed argues that “people’s expectations do usually seem to be fairly steady” (VC: 271). The reason is that

It takes a very neurotic community indeed to show much sensitivity in total over a very short period of actual time. People do not usually expect to be able to foresee the actual prices ruling on any particular day with complete accuracy, so that an appreciable variation from what they had thought to be the most probable price may fail to disturb their expectations at all. (VC: 272)

Notice that behind Hicks’s claim there is clearly the presumption that individuals can accurately distinguish between those purely *accidental* and those more *permanent* causes that affect prices even within a single temporary equilibrium. And this implicitly takes for granted that –because of past experience– individuals know fairly well the underlying conditions of the markets in which they interact. Little reflection however shows that there are no solid grounds to argue as Hicks does within a TGE framework. The plausibility of Hicks’s argument can be reasonably accepted under the hypothesis that

equilibrium relative prices, determined by *persistent* data, will be *slowly* reached after several market rounds, so that, granting stability, by trial and error agents may eventually come to know the equilibrium quantities that must be brought to the market, and their respective ‘normal’ or equilibrium expected prices. The assumption, therefore, that an accidental circumstance will not exert a long-lasting destabilizing effect on expected prices would be plausibly, it would seem, since this purely fortuitous event would have itself disappeared within a short interval of time^[114]. But in a TGE framework, where the data relative to the capital goods endowments can be very quickly altered if so decided, before the repetitions of transactions can allow those accidental events to vanish and equilibrium can emerge a sort of average of actually observed prices, the equilibrium itself can be considerably changed, and hence the presumption that individuals will be eventually come to know the “most probable” price of a commodity seems to be devoid of legitimacy.

Moreover, the following point deserves to be stressed: in a TGE framework there is no such thing as a “most probable” or “normal” price that is determined *independently* of the exogenous expectation functions assumed by the theorist; therefore, in the above-quoted passage, Hicks came close to admitting that the determinants of the normal price, that must no doubt represent a kind of average around which market prices tend to gravitate over sufficient time, must be actually independent of these subjective expectation functions. But then, in Hicks’s setting, these determinants remain largely unexplained. A reasonable explanation is to assume that this “most probable” price will more or less coincide with minimum average costs. This is the notion of normal price we find in Marshall and other traditional neoclassical scholars, from whom, in all likelihood, Hicks must have borrowed the term; however, in the traditional supply and demand approach, the concept of normal price needs, as Hicks himself freely admits in his early writings, and endogenous determination of the capital stock, which is clearly incompatible with the notion of TGE.

¹¹⁴ Recall in this connection from chapter 1, Kaldor’s 1934, and Schumpeter’s, 1934 considerations regarding the role of persistent data in the equilibration process. As seen in section 1.4., this is, essentially, the reasoning that Hicks’s uses to counter Marshall’s argument regarding the possible indeterminacy of wages.

So, within a TGE framework, it would seem that the idea of “normal” prices can be only justified by assuming that the economy, even in the very first periods of the sequence of TGE, has already been working under normal, fairly stationary conditions, so that equilibrium prices must be *preserved* but not actually *reached*. Indeed, because the data that determine a TGE may change drastically from period to period, it is only under these fairly stationary conditions that past equilibrium prices can be reasonably argued to indicate the normal price of a commodity –incidentally, we may recall (*cf.* section 1.2) that although Hicks originally dismisses the idea of a stationary economy, this is however the only *realistic* setting he can envisage to justify how an equilibrium with correct expectations is to be ever observed in actual economies. However, a moment’s reflection reveals the weakness of assuming that equilibrium must be only preserved; indeed the question of how these normal –fairly stationary-conditions might come about in the first place remains largely *unexplained*. The economy is in stationary equilibrium with steady –persistent- price expectations only because it has already been in such a situation for a long time. So Hicks’s assumption is untenable whenever some of the data change, e.g. labour immigration, or technical progress, and hence equilibrium must be found^[115] [116].

2.3.2. Price rigidities.

According to Hicks, subjective price expectations may be also anchored as a consequence of “frictions” or “rigidities” that prevent current prices from fully adjusting to supply and demand imbalances. However, Hicks is aware that price rigidities conflict with the notion of TGE, since its distinctive feature is market

¹¹⁵ In fact, there are other instances where Hicks seems to reason as if the economy has already been in equilibrium, or at least very close to it. He argues “this difficulty does not seem to matter very much. In application, *we shall always start from a position of equilibrium*, i.e. from a set of consistent quantities. It is not necessary to suppose any more than that some variation from this position is possible. That I think, will be granted” (VC: 85-85, fn. 2, emphasis added). On the other hand, Hicks does not justify why it will be plausible to assume that the starting point of the analysis is “a position of equilibrium”. The only plausible justification will be the stability of the equilibrium under plausible adjustment processes, but this is precisely what is in dispute.

¹¹⁶ Incidentally, notice that the introduction of the concept of normal price within a TGE framework also takes care of the *indefiniteness* problem regarding expectations, an issue that Hicks pointed out both before (in his perusal of Keynes General Theory) and after (Hicks, 1956: 225-226, *cf.* Petri 1991: 283) the publication of VC, and, as I have observed in fn.113, he came close to admitting in VC itself.

clearing, hence flexible prices, even in the *very-short* period. But Hicks notes that:

So far we have been assuming that prices are perfectly flexible... This assumption too must now be dropped, for it is of course *highly unrealistic*. (VC: 265, emphasis added)

In particular, given the importance of wages in unitary costs of production, Hicks envisages the rigidity of wages and hence unemployment as possible anchors of price expectations, i.e. as possible “stabilizers”. “The existence of unemployment”, so Hicks writes, “almost necessarily makes for stability” (VC: 269). And as a plausible explanation for the rigidity of wages we find Hicks arguing:

In order to explain the rigidity of wages, we have to assume in the parties to the wage-bargain some sense of normal prices, hardly distinguished (perhaps) from ‘just’ prices. The rigidity of wages extends over precisely that time –*it may be quite a long time*– during which the parties concerned persuade themselves that changes in related prices (whether prices of the products of labour, or of the things labour buys) are temporary changes. Once they become convinced that these changes are permanent changes, there is a tendency for wages to change. (VC: 270, emphasis added)

Notice then that Hicks explicitly remarks here that the hypothesis of fully flexible prices within the very short period –the extension of the single TGE– is “highly unrealistic”, and he admits that the rigidity of wages, hence the possibility of unemployment, can in fact last “for a long time”. So, while it is important to stress that Hicks does not deny the action of the supply and demand forces in the labour market, he does argue that the excess supply of labour will exert its effects only after sufficient time, when individuals “become convinced” that the ruling conditions “have permanently changed”.

It may be worth adding in this connection that, actually, Hicks (VC: 206; 211-212) already admitted the possibility of unemployment within the TGE framework, but in this case owing to the “specific character of the initial equipment” that causes substitution among factors in the very-short period to “be limited”; so, as a result of the existence of “technical rigidities” or “input lags” (*id.*) in production, when e.g. the price of factor of production changes and a new equilibrium must be found, “it will take some time before the entrepreneur has a really free hand to deal with the new situation” (VC: 211). The conclusion that Hicks reaches is that input lags are “closely connected

with... unemployment and the intractability of unemployment... [and hence] a theory which leaves out the probability of input lags is likely to be gravely misleading” (VC: 212). In so arguing, he seems to resume the traditional neoclassical idea that the assumption of full employment is only plausible as a long-period outcome.

Hicks concerns seem to be well justified, although he does not seem to grasp that the argument that in the face of unemployment wages will tend to decrease only gradually, and hence that, if any, a position of full-employment will be effectively reached only over sufficient time is clearly inconsistent with the TGE method; since, granted stability, the claim presupposes sufficiently persistent, slowly changing data. But in any event, the little noticed point I wish to make is that Hicks’s admissions that the idea of instantaneous equilibration is “highly unrealistic” and that unemployment can in fact last “for a long time” strongly suggests that even in the years that he was putting forward the notion of TGE, he still considers a supply-and-demand equilibrium as an outcome that can plausible emerge only over a sufficient interval of time, *as if* things continued working in the way justified by the traditional conception of capital, the single factor measured in value terms.

In other words, Hicks seems to believe that it is along the sequence of periods of the economy’s life-span that full-employment equilibrium will eventually assert itself^[117]. It thus seems that Hicks *de facto* abandons the hypothesis of instantaneous equilibration and envisages, instead, an economy with a fairly rigid real wage, determined by individuals’ sense of “fair” wages, and owing to specificity of the initial equipment and the existence of lags in production, a rather inelastic demand for labour. While on the one hand those factors will most likely cause short-run unemployment, the notion of normal prices and, in particular, of fair wages, may on the other hand provide a plausible explanation as to why wages do not abruptly fall to zero in case of any temporary excess of supply over demand for labour—as will be the case, e.g., in a

¹¹⁷ Precisely, Hicks’s discussion of the trade cycle (VC: 294-302) as a fluctuation around a position of full-employment, would be difficult to understand without considering that, behind his assertions, there is the traditional neoclassical notion of full-employment equilibrium as a position that is to be reached in the long-run.

recession- as on the contrary, one would be forced to conclude under a TGE framework with fully flexible wages and prices. As Hicks remarks at the end of the chapter devoted to the trade cycle,

If all prices were equally flexible, and all price-expectations equally flexible, mere cessation of accumulation would be sufficient to produce a slump without a bottom -the instability of capitalism declaring itself in complete breakdown. That this does not happen is due to price-rigidities, and ultimately, beyond price-rigidities, to people's sense of normal prices... The importance of this service can hardly be over-estimated. (VC: 297-298)

Yet, Hicks must still justify why, accepted that unemployment may last for rather long periods, full-employment is still a plausible outcome over sufficient time anyway. Otherwise, the assumption, implicit in the definition of TGE, that the economy follows on average a full-employment growth path, would be devoid of legitimacy. As we shall discuss in the following section, behind Hicks's claim that wage reductions will eventually bring about a position of full-employment there is the traditional neoclassical idea that investment is a negative function of the rate of interest; this latter variable still plays its traditional role, i.e. it accommodates investment decisions with the full-employment level of savings^[118], confirming our claim that, essentially, Hicks still reasons in ways that can be plausibly justified only by the traditional, albeit illegitimate, notion of capital^[119]. Little wonder then that, in those explanations, this traditional notion of capital is still there in its demand side role.

3. INVESTMENT AND THE RATE OF INTEREST IN VALUE AND CAPITAL.

The justification of the negative interest elasticity of investment demand one finds in VC relies on two different but related mechanisms, since both presuppose that a decrease in the rate of interest induces firms to adopt longer

¹¹⁸ In this connection it might be recalled that, as we have argued in the first chapter (section 5.2.3), a decreasing demand for labour is not enough to ensure the tendency towards a full-employment level of output. If the wage rate falls and employment increases, but investment does not adjust to savings, the level of national income will rise and savings will exceed investment. Effective aggregate demand will be thus *insufficient* to absorb the increased production (*cf.* also Garegnani, 1978:343; 1979: 71).

¹¹⁹ Moreover, if the negative influence of the rate of interest on investment decisions were plausibly justified, Hicks would have good reasons to disregard aggregate demand problems (to assume Say's Law) since, whatever the level of employment and output in the short-run, this output could be effectively sold.

processes of production, increasing somehow the average time lag between the application of capital goods and the moment that the outputs arrive to the market. The first mechanism is fully traditional: in some passages of VC Hicks fundamentally accepts the traditional neoclassical idea that a decrease in the rate of interest will induce the adoption of more capital-intensive, cost-minimizing techniques –and, as a consequence, aggregate investment can be considered an inverse function of the interest rate. As noticed in section 1.2, Hicks eventually wonders “How will the quantity of intermediate products – the quantity of capital – be determined [in stationary equilibrium]?” And he explains:

It turns out to be determined through the rate of interest. A fall in the rate of interest would encourage the adoption of longer processes, requiring the use (at any moment) of larger quantities of intermediate products. (VC: 118)

Here Hicks seems to be unproblematically accepting the traditional marginalist thesis that the demand for capital as a stock decreases with the rate of interest. Then, considering that in a stationary economy the demand for investment is a reduced-scale representation of the aggregate demand for capital, a decrease in the rate of interest also increases aggregate investment. This is not surprising actually: indeed, we have seen that while Hicks accuses traditional theory of being “condemned” to study secular equilibria, he in any case argues that it is a “*plausible theory* for the stationary state” (VC: 119). In other words, Hicks does not object to the internal consistency of that theory; he simply limits himself to point out the irrelevance of the notion of secular equilibrium to have a correspondence with observation. Incidentally, in the above citation, Hicks’s reference to “larger quantities of intermediate products” neglects the changes in the types of capital goods that will be used when the rate of interest changes, hence he actually treats the different capital goods as “embodiments” of a homogenous factor of production.

3.1. The ‘tilting’ of the production plan

Now, in order further to understand how Hicks reaches the conclusion that, even in a TGE framework, a decrease in the rate of interest still induces profit-maximizing firms to employ larger “quantities of intermediate products”, and which as a result gives some justification to the traditional neoclassical belief in

the tendency of investment to adapt to the full-employment level of savings, we must now turn to the second explanation of the negative elasticity of investment decisions with respect to the rate of interest, since it is the one that plays the dominant role in VC. Along chapters XV to XVIII, Hicks attempts to “rehabilitate” a *sui generis* interpretation of the mechanisms of substitution in terms of labour and capital inputs within a TGE framework. The “laws of working of a TGE system”, Hicks argues, do not very much differ from the mechanisms that operate in the atemporal framework. The main difference, he continues, is that within the TGE method firms can also substitute among inputs and outputs *over time*. The conclusion that an increase in savings will be sooner or later absorbed by investment through a decrease in the rate of interest emerges as an outcome of this mechanism of substitution. Let us reconstruct how Hick reaches this conclusion.

Hicks (VC: XV) starts the analysis by assuming that the representative firm maximizes the present value of its (expected) sum of per-period net yields, subject to the usual technology constraint. Formally (VC: 326), the firm solves the following problem:

$$\max_{x_{rt}} C = \sum_{t=0}^v \sum_{r=1}^n \beta_t^r p_{kt} x_{kt} \text{ s.t. } f(x_{11}, \dots, x_{1n}, \dots, x_{v1}, \dots, x_{vn}) = 0 \quad (1)$$

where x_{kt} stands for the quantity of commodity k in period t , and p_{kt} stands for its respective expected price (as usual, Hicks treats factors as negative

products); $\beta_t \equiv \frac{1}{R_t}$ stands for the discount factor applied in period t (R_t is the

gross rate of interest ruling on that period). Hicks usually assumes that this rate of discount is the same across periods^[120]. The condition

$f(x_{11}, \dots, x_{1n}, \dots, x_{v1}, \dots, x_{vn}) = 0$ describes the production function in an implicit form.

The assumption of sufficiently decreasing returns to scale at the firm level, allegedly caused by “elements of risk” and “increasing difficulty of management and control as the firm gets larger” (VC: 200), allows the British author to argue that the expected value of the single plan, C , will be positive in equilibrium (i.e.

¹²⁰ This is clear when Hicks (VC: 327-328) formally derives the direction of change in the interest rate in the production plan by means of “the average period of the production plan”, to be discussed below.

“pure” profits will be positive)[¹²¹]. Once the optimal plan is determined, Hicks goes on to analyse how this plan is affected by a) changes in prices and price-expectations under the assumption of a *given structure of interest rates* (chapter XVI) b) long-lasting changes in the rate of interest under the assumption of exogenously *given expected prices* (Chapter XVII). In other words, along part IV of VC Hicks reasons *as if* the expected yields of investment *were given independently of the rate of interest*.

With regard to the effect of a change in the rate of interest on the production plan (chapter XVII), Hicks argues:

A fall in the rate of interest for loans of t weeks will thus raise the discounted prices of... the outputs and inputs planned to the week starting t weeks ahead. (VC: 216)

While “cases are conceivable in which the reaction on particular output or particular input may go the opposite way” (*id.*: 215), Hicks in any case asserts that the effect of a fall in the rate of interest is “simply this: the surplus planned for the $(t+1)$ th [period] must be increased... this principle holds quite generally” (*id.*). Hicks also points out that i) “the discounted prices of outputs and inputs further ahead in time are regularly affected more than the discounted prices of the nearer outputs and inputs” (*id.*: 214) and ii) that within a particular period, the change in the interest rate will affect all outputs and inputs in the same proportion, hence, always within the single analytical period, inputs and outputs can be treated as a single composite commodity (*id.*: 215). Therefore, he concludes that “the effect of interest-changes can then be regarded as consisting in substitution among surpluses [of different periods]” (*id.*).

¹²¹ The assumption of positive extra profits in equilibrium (i.e. a positive value of C) due to the alleged difficulty of control seems to be devoid of legitimacy. If any, the assumption could explain why firms are limited in size, but when due to *free entry* the creation of new firms is considered (and Hicks does consider this possibility, *cf.* VC: 200), the level of output at the industry will vary by variation in the number of firms, hence the expected level of profits should be zero. Even in the short period, when correctly computed, the level of extra profits is zero. If decreasing returns is caused by the existence of a fixed factor (e.g. a given number of managers), the level of extra profits obtained by the firm corresponds to the quasi rent obtained by the fixed factor. As to the “element of risk”, first, Hicks does not offer a convincing reason as to why this should limit the size of the firm. And moreover, even if the legitimacy of the argument were accepted, it could only justify, again, why firms are limited in size, but not the existence of extra profits: again, when free entry is considered, the level of output should be such that extra profits are zero.

Hicks is particularly interested in studying the effect of a change in the rate of interest that is expected to last several periods since “this is the most interesting case” (VC: 213). In this case, he goes on to explain:

If rates of interest per week fall for loans of all periods, the discount ratios (that is to say, the ‘price’) corresponding to all future surpluses will be raised; and this in itself induces a direct tendency for substitution in favour of future surpluses, against current surplus.(VC: 216)

The substitution among “surpluses” takes place in the following way: when the interest rate decreases profit-maximizing firms are induced to substitute between inputs and outputs *over time*, the net effect being what Hicks calls a “tilt” of the production plan to the right: there is a decrease in the level of current outputs at the expense of future outputs, and an *increase* in the current demand for inputs at the expense of their future demand. “Since a surplus can be expanded either by an expansion of the corresponding outputs or by a contraction of the corresponding inputs”, Hicks says,

[t]he effect of this tilt on the output and input streams which compose the plan would be as follows. Output streams will be tilted upwards to the right, like this $X_0 \downarrow, X_1, X_2, X_3, \dots, \uparrow X_n \dots$ But input streams would be tilted in the opposite direction. $A_0 \uparrow, A_1, A_2, A_3, \dots, \downarrow A_n$. (VC: 216-217)

To put it in a nutshell: according to the mechanism of substitution over time, when the rate of interest decreases, future (expected) inputs prices increase relative to current input prices, and future output prices increase relative to current prices. Therefore, profit-maximizing entrepreneurs, who are *assumed* not to expect future-price changes to occur even though the rate of interest has changed, are induced to reduce their present level of output relative to its future level, and to adopt techniques that require the employment of more current inputs relative to future inputs. The overall effect, to be discussed in the following section, is that the new production plan becomes “longer” than the old one, the length of the plan being measured by its “average period” (as to the legitimacy of the assumption of *given* expected prices, its discussion is postponed to section 3.3).

3.2. Hicks’s “average period of the production plan”.

Once the action of the mechanism of substitution over time has been explained, and the effect of the rate of interest on the production plan ascertained, Hicks proceeds to provide

[a] numerical index to the character of the plan, which can be relied upon to change in a given direction when the interest rate varies (VC: 217-218).

For this purpose he defines the “average period of the production plan”, P , which measures

[t]he average length of time for which the various payments are deferred from the present, when the times of deferment are weighted by the discounted values of the payments (VC: 186)

where by payment Hicks means here the expected yield or net “surplus”

obtained in period t , $X_t \equiv \sum_{k=1}^n p_{kt} x_{kt}$. In formal terms (cf. VC: 186, 328), for given

relative prices, p_{kt} , (and which are assumed not to change as the rate of interest changes) P is defined as the elasticity of the (expected) present value of the

production plan, $C = \sum_{t=0}^v \beta^t X_t$, with respect to the discount factor β :

$$P \equiv \frac{\partial C}{\partial \beta} \frac{\beta}{C} = \frac{\sum_{t=0}^v t \beta^t X_t}{C} \quad (2)$$

The average period as defined by equation (2) is therefore a weighted average of the different surpluses X_t , and the weights are given by the proportion of the net

surplus obtained in period t to the total surplus of the plan, i.e. $\frac{\beta^t X_t}{C}$.

Hicks, however, immediately notes that the average period of each production plan “has to meet one apparently fatal objection” (*id.*: 219). When the interest rate changes, the average period will change “even if the production plan is not changed at all”, (*id.*). In order to avoid this problem, Hicks suggests proceeding in the following way:

[e]ven when we are considering the effect of changes in the rate of interest on the production plan, we must not allow the rate of interest which we use in the calculation of

the average period to be changed. What we must do is to start with a certain rate of interest, a certain production plan drawn up in view of that rate, and an average period calculated from this production plan at this rate of interest. Then we must suppose the rate of interest to fall, and the production plan to be varied in consequence. Finally, we must calculate the average period of the new plan, using the same rate of interest in its calculation as before –that is to say, the old rate of interest. (VC: 220)

In other words, assuming a change in the discount factor β (i.e. in the rate of interest R), in equation (2) Hicks examines the change in the average period caused by a change in the time-distribution of the different surpluses X_t (that in turn takes place because of the process of substitution over time), but he does not allow the discount factor β to vary in the computation of the change in the average period. In formal terms (VC: 328)^[122]:

$$dP = \frac{\sum_{t=0}^v \beta^t [t - P] dX_t}{C} \quad (3)$$

And after some manipulations, Hicks (VC: 328) shows that a decrease in R (a rise in β) rises the average period P :

$$\frac{dP}{dR} < 0 \quad (4)$$

(see the APPENDIX to this chapter for a derivation of this last result). As Hicks himself writes:

The average period, calculated in this way, must be longer than the old. A fall in the rate of interest lengthens the average period (VC: 220).

So, if e.g. we interpret each production plan as a different stream of physical inputs, e.g. homogenous labour, that in the last period matures in a particular net output as in the typical Austrian flow input-point output kind of process, Hicks seems to have found a measure of the degree of ‘roundaboutness’ of the

¹²² This condition is derived as follows:

$$P \equiv \frac{\sum_{t=0}^v t \beta^t X_t}{\sum_{t=0}^v \beta^t X_t} \Rightarrow P \sum_{t=0}^v \beta^t X_t = \sum_{t=0}^v t \beta^t X_t \Rightarrow dP \sum_{t=0}^v \beta^t X_t + P \sum_{t=0}^v \beta^t dX_t = \sum_{t=0}^v t \beta^t dX_t \Rightarrow dP = \frac{\sum_{t=0}^v \beta^t (t - P) dX_t}{\sum_{t=0}^v \beta^t X_t}$$

different techniques that contra-varies with the interest rate. “It was this”, Hicks in fact asserts referring to the measure P ,

[w]hich the Austrians were looking for. (VC: 219)

Hicks here clearly attempts to establish a sort of *continuity* between traditional Austrian theory and the new TGE method, and his claim that “A fall in the rate of interest lengthens the average period” strongly suggests that he still believes that those traditional tendencies envisaged by the founders of the marginalist approach, i.e. the tendency of firms to adopt “longer processes” of production as the rate of interest decreases, and that were originally used by Böhm-Bawerk and Wicksell to justify the supply and demand explanation of prices and distribution, can be more rigorously established within the new TGE framework [123]. However, as will be shown in the APPENDIX to this chapter, closer inspection reveals that Hicks’s average period can not serve the purpose with which the “Austrians” created and developed this notion[124]: as recently shown by Fratini (2012), the reason is not so much that the average period of a particular technique will change when the rate of interest used in the calculation of P changes, as Hicks himself seems to believe; rather, the main deficiency of Hicks’s measure consists of the fact that the *order* in which the different techniques can be ranked will have itself changed when nothing but the rate of interest used in the calculation changes. In other words, contrarily to what Hicks seems to suggest, the measure P is useless to order techniques *independently* of distribution (as it is shown in the appendix, Hicks will admit this in his later contributions, first in *Capital and Growth* (1965) and then in *Capital and time* (1973)).

¹²³ Interestingly enough, in a recent contribution Malinvaud (2003) has argued that Hicks’s notion of average period provides a consistent measure of the degree of roundaboutness of the different techniques: “it is interesting to know that the average period of production, a measure of the degree of roundaboutness, contra-varies with the interest rate” (Malinvaud, 2003: 518). Little wonder that, as I will show in the seventh chapter of this thesis, despite allegedly embracing the neo-Walrasian, temporary equilibrium method to explain the trends in actual economies, Malinvaud still relies on the traditional notion of capital in its demand side role to argue that a decrease in wages will eventually increase labour employment.

¹²⁴ A more thorough examination of the traditional notion of average period of production, its aims and limitations, is discussed in the third part of this dissertation (chapter 6), in my evaluation of Allais’s work on equilibrium and capital.

I wish to conclude this section with two remarks: the first one is that it is the mechanism of substitution among inputs over time -from which the tilting of the production plan and its average period are derived- what allows Hicks to reach the conclusion that an increase on full-employment savings will be eventually absorbed by investment. This conclusion is drawn in chapter XVIII, “Spending and lending”: on the one hand, and due to ‘perverse’ income effects, an increase in the rate of interest, so Hicks argues, may well increase or decrease the difference between the planned levels of income and consumption (i.e. the supply of savings) (*id.*: 233). On the other hand, the net borrowers of investible funds, i.e. the “entrepreneurs who are undertaking real investment” (*id.*: 234), are argued to undoubtedly decrease their level of expenditure:

When the rate of interest rises, such a person’s [the borrower’s] expenditure must infallibly be reduced. (VC 234)

(by expenditure Hicks clearly means expenditure in capital goods, or “real investment”). Given that the influence of the rate of interest on investment decisions is argued to be stronger than the effect exerted on the supply of savings, a change in the rate of interest will affect aggregate net expenditure in the opposite direction. “For the market as whole”, Hicks concludes,

a rise in the rate of interest will reduce current expenditure, a fall in the rate of interest increase it. (VC: 235)

As can be readily seen, the conclusion reached is fully traditional here: Hicks assigns to the rate of interest the role of accommodating investment decisions to the full-employment level of savings, *as if* things continued working in the way justified by the traditional conception of capital.

The second remark is that while the alleged effect of a decrease in the rate of interest is to tilt the production plan to the right (and hence an increase in the average period), “it is possible”, so Hicks (VC: 216) eventually admits, that, due to the existence of *complementary* relations among, e.g., qualitatively different kinds of inputs employed in the different periods, the time shape of net yields is not tilted in the way the mechanisms of substitution over time predict; the implication, not explicitly stated by Hicks however, would be that alleged tendency of investment to adapt to the full-employment level of savings would

be jeopardized. Without any justification however, Hicks restricts this possibility to a “limited number” (VC: 216) of cases, evidently because of a faith that things could not work differently from what neoclassical theory postulates^[125].

3.3. Some remarks on the assumption of *given* expected prices in the derivation of the investment function.

We have seen in section 3.1. that behind the working of the mechanism of substitution over time (and hence also behind the claim that a decrease in the rate of interest increases the average period of the production plan), from which the negative relationship between the interest rate and investment demand is derived in VC, there is clearly the presumption that expected relative prices will not change when the rate of interest changes. However, I wish to conclude my discussion of Hicks by stressing that this assumption does not appear to be acceptable. In fact, the rate of interest is a part of the unitary costs of production; therefore, when the rate of interest decreases, one should expect competition to exert a downward pressure on money prices. Then, as Garegnani points out,

[t]he incentive to increase investment demand will be eventually countermanded when, as it began to be carried out, distribution and relative prices adapt themselves to the rate of interest ruling in the loan market. (Garegnani, 1979: 66, fn. ++)

The implication of Garegnani’s remark (*cf.* also Pivetti, 1991 and Petri 1997, 2004: chapter 7) is that the influence of the rate of interest on investment decisions cannot but be derived under the assumption of cost-of-production relative prices; otherwise, one would be implicitly neglecting the effect of competition and free entry on relative prices. In other words, only under the hypothesis that investment decisions are taken on the basis of a normal level of profitability, the relationship between investment and the interest rate can be argued to capture the action of sufficiently persistent forces at work. This is particularly the case for the plausibility of Hicks’s argument, because Hicks is

¹²⁵ But there is no reason why this possibility should be dismissed as unimportant. In this connection, Hatta (1976) has shown that under very plausible conditions, the possibility of complementary relations among inputs employed in the different periods can well be a cause of reswitching of techniques, which by itself undermines the belief in the tendency of investment to adapt to the full-employment level of savings.

mainly concerned with the effects of changes in the rate of interest that are expected to last for a sufficient interval of time. So, as notably Petri (2004: 267) points out, even if one attempts to predict the influence of the rate of interest on investment decisions at a particular *point in time*, the fact that the yields of a particular investment project are spread over considerable length of time implies that, contrarily to what Hicks seems to believe, one has no right to assume that entrepreneurs will expect future prices as not changing if, at the same time, the change in the rate of interest is expected to last for a long period of time.

In any case, one should be highly suspicious about the definiteness of the conclusions that can be drawn regarding the influence of the rate of interest on investment decisions at each moment of time, because at an arbitrarily given moment these decisions will be no doubt conditioned by all sort of accidental circumstances, whose influence on investment largely escape the domain of the theory. As Garegnani (1978) argues in this connection, the decisive role that one would be forced to attribute, for instance, to *expectations* in shaping investment decisions would condemn the theory to indefiniteness, it would seem:

The attempt to determine the effects on investment of changes in the rate of interest on such indefinite grounds would seem liable to dissolve into casuistry concerning the influence of these changes on the expectations of entrepreneurs. And this influence would differ from situation to situation, thus making impossible any general and unambiguous conclusions concerning direction and intensity of the effects of interest on investment. (Garegnani, 1978: 347)

In VC, Hicks in fact comes very close to admitting that the influence of subjective expectations on the relevant economic variables under examination cannot be asserted at a sufficiently general level. Once the trends predicted by the theory are argued to depend on the influence of these subjective factors, which as Hicks himself acknowledges, must contemplate people's reactions about "the political news", or "future movements of demand and supply", then "The current economic situation", so Hicks is forced to admit, "may perhaps react along these channels in *mysterious... ways*" (VC: 204, emphasis added). Consider in this connection that at any arbitrary instant of time, the possibility that a decrease in the rate of interest induces the expectation of further decreases in this variable cannot be excluded, implying therefore a

postponement, rather than an increase, of investment decisions (Petri, 2009: 16-17). To summarize: the outcome of this discussion is that Hicks's implicit assumption of expected prices as given independently of the rate of interest is unacceptable, because the analysis must aim to reach definite conclusions by determining the action of persistent forces at work, and this implies that, along the analysis, prices must be assumed to be equal to their minimum average costs. An important implication is that, within neoclassical theory, the negative relationship between investment demand and the rate of interest cannot but be argued on the basis of the traditional factor substitution mechanisms, anyway marred by their illegitimate notion of capital as a single factor of production measured in value terms^[126].

4. FINAL CONSIDERATIONS: ON HICKS'S RECANTATION OF THE NEO-WALRASIAN APPROACH.

Among the first generation of neo-Walrasian scholars, Hicks has been no doubt the most influential author in the subsequent development of this approach. An authority in the field like Kenneth Arrow (1981, quoted from Weintraub, 1983: 29), for instance, has explicitly recognized that "of course Hicks's *Value and Capital* made the biggest impression on me". Quite paradoxically though, also among the forerunners, Hicks is the author who will subsequently show major concerns, mainly methodological, about the framework developed in *Value and Capital*. And in fact, it should come as no surprise that this has been the case, given that the neo-Walrasian framework is unable to provide an accurate description of the working of real economies.

¹²⁶ This point is rather important because, among the justifications that one currently finds for the negative interest-elasticity of investment demand, one of them is particularly popular (cf. e.g. Krugman and Wells (2008: 216-217): the so-called 'Array-of-opportunities' approach. Precisely, the main idea behind this approach is that, at each point in time, the entrepreneur is confronted with several investment projects, which can be ordered according to their expected internal rates of return, derivable from the expected yields of the project. The aggregate investment function is therefore derived by calculating, for each rate of interest, the sum of all the investment projects that are profitable to implement. So, it is argued, the lower the rate of interest, the higher the number of projects that have an internal rate of rate of return higher than the rate of interest, hence investment is argued to be a decreasing function of the interest rate. The similarity between this approach and Hicks's can be readily seen, since both illegitimately presuppose that the future yields of investment can be assumed to be independent of the interest rate (cf. Ackley, 1978: 260-265 and Petri, 2004: 262-268 for a detailed discussion of the deficiencies of the Array-of-opportunities approach as an explanation of the negative elasticity of investment demand)

In this connection, Petri (1991) has documented Hicks's "recantation" of the temporary equilibrium method. In *Capital and Growth* in fact, Hicks puts forward harsh critiques on the notion of TGE. He argues, for instance, that the "artificiality" of the Hicksian week and Hicksian Monday "was too obvious", and he concludes that

The fundamental weakness of the Temporary Equilibrium method is the assumption, which is obliged to make, that the market is in equilibrium –actual demand equals desired demand, actual supply equals desired supply- even in the *very* short period, which is what the single period must be taken to be. (Hicks, 1965: 76)

In sum, in *Capital and growth*, Hicks's appraisal of the temporary equilibrium method simply evidences the evident conceptual deficiencies behind the notion of TGE, shortcomings that considerably narrow, in Hicks's opinion -that we share- the usefulness of the neo-Walrasian method to have a correspondence with the actually observed trends in prices and distribution.

But actually, I believe that the main contribution of the chapter is to have shown that *already* in VC Hicks recants of the notion of temporary equilibrium in all markets due to the "highly unrealistic" assumption of instantaneous equilibration; indeed, we have seen that in Hicks's view both the notion of "fair" wages (VC: 270) and the "specificity of the initial equipment" (*id.*: 211) may well prevent market clearing in the labour market in the short-run. While Hicks never abandons his faith in the supply-and-demand explanation of distribution, we have seen that these factors oblige him to resume the old neoclassical idea that it is only after sufficient time that the forces of supply and demand will eventually exert themselves fully, as if things continued working in the way justified by the traditional conception of capital.

In fact, as I attempted to show in the third part of the chapter, Hicks's argument that a decrease in the rate of interest will induce firms to adopt "longer processes of production", hence increasing their demand for "larger quantities of intermediate products" (VC: 118) is clearly marred by its acceptance of the traditional notion of capital in its demand side role. While less clearly, this demand side role also seems to be present in Hicks's explanation of the mechanism of substitution over time that operates in the TGE framework,

since from this mechanism Hicks derives a sort of average period of production that increases with decreases in the rate of interest (hence investment is a decreasing function of the interest rate), *as if* things in fact continued working essentially in the way envisaged by the founders of the marginalist approach. We have then argued that the mechanism of substitution over time is hardly defensible because it is derived on the assumption that expected prices will not change even if the interest rate is assumed to change for rather long periods, and that Hicks’s average period of production is actually an inadequate index because it does not allow ordering techniques according to their degree of ‘roundaboutness’^[127]. But in any case the survival, along Hicks’s work, of the old neoclassical idea that a decrease in the rate of interest induces firms to adopt longer processes of production, *as if* things continued working in the way justified by the traditional notion of capital as a single factor of production, seems to be highly relevant to better understand why the shift towards a neo-Walrasian approach to prices and distribution was not envisaged by the beginners of this approach as a drastic departure from traditional neoclassical theory: in the case of Hicks, the traditional neoclassical tendencies, namely, the tendency of the economy to gravitate only after sufficient time around a full-employment growth path still holds, but, within the new TGE method, now operates along the different periods of the economy’s life span.

APPENDIX: ON HICKS’S AVERAGE PERIOD OF PRODUCTION.

In the main text (equation (2)) we have seen that Hicks defines the average period as the elasticity of the expected value of the plan P , with respect to the net discount factor β :

$$P \equiv \frac{\partial C}{\partial \beta} \frac{\beta}{C}$$

and concludes that “A fall in the rate of interest lengthens the average period” (VC: 220). This is the measure, so Hicks says, “which the Austrians were looking for” (VC: 219). By relying on Fratini (2012), the purpose of this

¹²⁷ As argued, the proof has been recently given by Fratini (2012). The reader is referred to the appendix for a reconstruction of Fratini’s argument.

appendix is to show that, actually, the average period P could *not* have been the measure that the Austrians were looking for, not only because this measure changes with changes in distribution (a problem that, as seen, Hicks is aware of), but specially because the *order* of techniques will change depending on the rate of interest used in the calculation of the average period.

To show this, let us consider a typical Austrian process of the flow input-point output type: y stands for the net output (its price is set as the numéraire); X_0 , X_1 and X_2 indicate, respectively, the amount of labour directly employed in the production of y , the amount of labour employed in the production of the direct means of production of y , and finally, the amount of labour used in the production of the indirect means of production (assumed to be produced only by labour). Wages are paid in arrears. The final output is produced under a twice differentiable, well-behaved production function: $y = f(X_0, X_1, X_2)$ [128]. The individual firm maximizes the following profit function:

$$\max_{X_0, X_1, X_2} f(X_0, X_1, X_2) - w[X_0 + X_1R + X_2R^2] \quad [\text{A1}]$$

Where R stands for the gross rate of interest, which following Hicks (*cf.* VC: 327-328) is assumed to be *constant* across periods, and w stands for the real wage rate. The price of the output is set equal to 1. The first order conditions are given by:

$$\frac{\partial y}{\partial X_t} = wR^t \quad t=0,1,2 \quad [\text{A2}]$$

If constant returns to scale are assumed (see below), conditions [A2] do not uniquely determine the optimal X_t^* since these equations are not independent.

We add the condition of full-employment:

$$X_0 + X_1 + X_2 = L \quad [\text{A3}]$$

¹²⁸ This representation of technology is made for convenience only. The results reached do not depend on it. For a more general representation of the technology that does not assume a well-behaved production function the reader is referred to Fratini (2012)

The system [A2]-[A3] allows determining the optimal levels $X_t^* = (X_0^*, X_1^*, X_2^*)$ for a given interest rate R . The average period calculated by means of equation (2) is:

$$\frac{wX_1^*R + 2wX_2^*R^2}{y - w[X_0^* + X_1^*R + 2X_2^*R^2]} \quad [A4]$$

Notice that we have used the interest rate R in the calculation instead of the discount factor β . The reason for this modification (which anyway does not alter the main results) is that, with respect to Hicks's formulation, we are inverting the chronological order of the different payments, i.e. given that we are assuming that the output comes out in period $t=0$, the first payment (negative surplus) is equal to wX_2 , while the last one is equal to wX_0 (*cf.* Fratini, 2012: 6, fn 11).

There is however a more relevant modification that must be done before pursuing the argument further. We have noticed in the main text that due to the existence of decreasing returns to scale at the firm level, Hicks assumes that the expected value of the plan is positive. However, while decreasing returns may explain why the firm is of limited size, once free entry is allowed (and Hicks does allow free entry, *cf.* VC: 200), at the industry level there must be constant returns to scale anyway, so the level of extra profits is zero. But this implies that the denominator of equation (2) is zero, and hence the average period becomes infinite. This point has been noticed, e.g., by Samuelson:

Hicks's definition [of the average period] must take into account the fact that, under perfect competition with free entry and constant returns to scale, the prices of all final, intermediate, and input goods will change with the interest rate until net present-discounted-values are again zero. Then his average is found to be always infinite! (Samuelson, 1966: 570-571, fn. 3)

So, in order to overcome this issue, I will follow Malinvaud (2003) and Fratini (2012) and redefine the average period as the elasticity of output with respect to the discount factor β :

$$P \equiv \frac{\partial y}{\partial \beta} \frac{\beta}{y} \quad [2']$$

In our example, we have^[129]:

$$P = \frac{[X_1^* R + 2X_2^* R^2]}{[X_0^* + X_1^* R + X_2^* R^2]} \quad [A4']$$

We are now in condition to use [A4'] to show i) the negative relationship between the average period and the interest factor argued by Hicks (equation (4) in the main text) and, ii) more importantly, to disprove the argument that P is an adequate measure of the degree of roundaboutness of the different techniques. To show the first point, let us first find the equivalent of condition (3) in the main text under the modified definition of the average period. We get^[130]:

$$dP = \frac{\sum_{t=0}^2 R^t [t - P] dX_t^*}{\sum_{t=0}^2 X_t^* R^t} \quad (3')$$

Let us now differentiate the first order conditions [A2] and obtain:

$$\sum_{s=0}^2 \frac{\partial^2 y}{\partial X_t^* \partial X_s^*} dX_s^* = w R^t dw + w t R^t \frac{dR}{R} \quad t=0,1,2 \quad [A5]$$

Now, considering that the zero-profits condition implies:

$$w = \frac{y}{\sum_{t=0}^2 X_t^* R^t} \quad [A6]$$

¹²⁹ $P = \frac{w[X_1 R + 2X_2 R^2]}{w[X_0 + X_1 R + X_2 R^2]} = \frac{[X_1 R + 2X_2 R^2]}{[X_0 + X_1 R + X_2 R^2]}$

¹³⁰ $P[X_0 + X_1 R + X_2 R^2] = [X_1 R + 2X_2 R^2] \Rightarrow dP[X_0 + X_1 R + 2X_2 R^2] + P[dX_0 + dX_1 R + dX_2 R^2] = dX_1 R + 2dX_2 R^2 \Rightarrow dP = \frac{\sum_{t=0}^2 R^t (t - P) dX_t}{\sum_{t=0}^2 X_t R^t}$

Differentiating in [A6] w with respect to R , and using the definition [A4'], we get^[131]:

$$P = -\frac{w}{R} \frac{dw}{dR} \quad [A7]$$

Equation [A7] will be particularly important for the second part of the argument. But first, introducing this equation into [A5] we get^[132]:

$$\frac{1}{w} \sum_{s=0}^2 \frac{\partial^2 y}{\partial X_t^* \partial X_s^*} dX_s^* = R^t \frac{dR}{R} (t-P) \quad t=0,1,2 \quad [A8]$$

Multiplying each equation t by dX_t^* and summing over t :

$$\frac{1}{w} \sum_{t=0}^2 dX_t^* \left(\sum_{s=0}^2 \frac{\partial^2 y}{\partial X_t^* \partial X_s^*} dX_s^* \right) = \frac{dR}{R} \sum_{t=0}^2 dX_t^* R^t (t-P) < 0 \quad [A9]$$

which is negative, since the left hand side is negative due to assumption of non-increasing returns to scale^[133]. Now, because of conditions [3'] and [A6], we

have: $\frac{dR}{R} \sum_{t=0}^2 dX_t^* R^t (t-P) = \frac{dR}{R} dP \sum_{t=0}^2 X_t R^t = \frac{dR}{R} \frac{y}{w} dP$. Because of condition [A9],

this means that,

$$\frac{dR}{R} \frac{y}{w} dP < 0 \quad [A10]$$

¹³¹ We get: $\frac{dw}{dR} = \frac{-y \sum_{t=0}^2 t R^t X_t}{R \left(\sum_{t=0}^2 R^t X_t \right)^2} = \frac{-y \sum_{t=0}^2 t R^t X_t}{R \left(\sum_{t=0}^2 R^t X_t \right) \left(\sum_{t=0}^2 R^t X_t \right)} = -\frac{wy}{wR \left(\sum_{t=0}^2 R^t X_t \right) \left(\sum_{t=0}^2 R^t X_t \right)} = -\frac{w}{R} P$

¹³² We get: $\frac{1}{w} \sum_{s=0}^2 \frac{\partial^2 y}{\partial X_t^* \partial X_s^*} dX_s^* = R^t \frac{dw}{w} + t R^t \frac{dR}{R} = -\frac{dR}{R} R^t P + t R^t \frac{dR}{R} = R^t \frac{dR}{R} (t-P)$

¹³³ Under the assumption that the production function exhibits non-increasing returns, its Hessian matrix H at point $X^* = (X_0^*, X_1^*, X_2^*)$ is negative semi-definite, that is: $xHx \leq 0 \forall x$. But if it is further assumed that H has rank 2, there is just one non-null vector such that $Hx=0$, and it is collinear to X^* . Due to the assumption of full employment, we must have that $\sum_{t=0}^2 X_t^* = L$, and hence $\sum_{t=0}^2 dX_t^* = 0$. So at least one element of dX_t^* is negative, meaning that dX_t^* and X^* cannot be collinear. Equation [A9] is therefore negative.

which implies that the average period and the rate of interest (the discount factor β) move in opposite directions (the same direction), as Hicks stated.

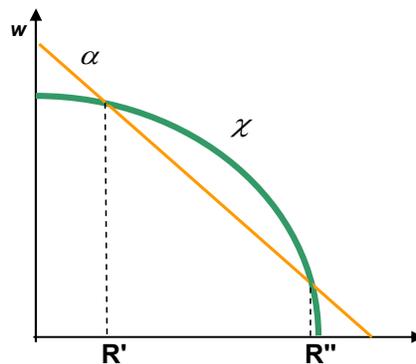
Let us now proceed to the second aim of the appendix: to show that Hicks's notion of average period does not allow ordering techniques according to their degree of roundaboutness. Let us follow Fratini (2012: section IV) and consider the case in which there are two techniques $\phi = \alpha, \chi$ to produce the final output. Each technique is characterized by a different stream of labour inputs X_t^ϕ . As is well known, the technique that will be actually employed is the one that allows paying the highest real wage for a certain rate of interest. According to equation [A6], for each technique we have:

$$w^\phi(R) = \frac{y^\phi}{\sum_{t=0}^2 X_t^\phi R^t} \quad [\text{A12}]$$

Where y^ϕ is the net output obtained under technique ϕ , and X_t^ϕ stands for the different quantities of labour employed in the different stages under technique ϕ . And according to equation [A7]:

$$P(\phi) = -\frac{w^\phi(R)}{R} \frac{dw^\phi}{dR} \quad [\text{A13}]$$

Equation [A13] essentially shows that, for a *given* R , the technique with the highest average period is the one with the most elastic wage curve in absolute terms. In order to show the implication of this result, let us assume that α and χ exhibit reswitching as shown in the w - R diagram below:



Assume that at the switch point R' , the average period P is higher for technique α than for technique χ . Let us represent this with the following notation: $P(\alpha, R') > P(\chi, R')$. By equation [A13] we know that this will be the case *if and only if* α is the technique with the steepest wage curve at R' . But at the other switch point R'' the order will be necessarily reversed: for at R'' it must be necessarily the case that technique χ has the steepest wage curve, hence the highest average period, i.e. $P(\alpha, R'') < P(\chi, R'')$. In sum, at R' we have $P(\alpha) > P(\chi)$ while at R'' we have that $P(\alpha) < P(\chi)$. It is still true of course that, despite reswitching, when R moves within a neighbourhood of a switch-point, the technique that is adopted when the interest rate decreases is the one with the highest average period as defined by equation [B]; this is what Hicks's result, according to which "A fall in the rate of interest lengthens the average period" (VC: 220), shows.

To conclude is worth pointing out that, interestingly enough, the weakness of the measure P as a way to order techniques according to their degree of roundaboutness has been eventually admitted by Hicks himself, first in *Capital and growth* (1965: 166-167), and then in *Capital and time* (1973). "It is of course true", Hicks says, "that whenever a rise in the wage induces a change in technique, the change must always be such that, at the switch-point, the new efficiency curve has the greater slope" (Hicks, 1973: 45), and then adds:

It was this which I endeavoured to express, in the chapter of *Value and Capital* just cited [chapter XVII], in terms of a 'period of production' that was weighted by discounted values. There is nothing wrong in that treatment; but, except for its particular purpose, of criticising the 'old' Austrian theory, it is not very useful. (1973: 45, footnote).

**LANGE'S (1944) *PRICE FLEXIBILITY AND
EMPLOYMENT*^[134]**

INTRODUCTION.

In this concluding chapter of the second part I review some of Oscar Lange's works on equilibrium and capital. This is firstly attempted because, to the best of my knowledge, there has not yet been a systematic examination of Lange's contributions on these topics during the 1930s and 1940s, when the first concerns on how to treat capital in neoclassical equilibrium theory are raised. Secondly, although Lange's work is probably less-well known than e.g. Hicks's or Hayek's, his *Price flexibility and employment* (henceforth PFE, 1944) also belongs to the first generation of neo-Walrasian contributions that had considerable impact on the subsequent development of neoclassical theory; for instance, the analytical framework of PFE is then taken up and developed by Lange's pupil, Don Patinkin (1956), whose contributions were no doubt particularly important in spreading and consolidating the so-called 'neoclassical synthesis' (cf. Blanchard, 2008), and hence the belief that Keynes's (1936) –and as we shall see, Lange's themselves- alleged reasons to question the tendency towards the full-employment of resources were of secondary importance, since they could not by themselves exert autonomous and long-lasting negative effects in the economy.

In view of these considerations, an examination of Lange's work should shed further light on the reasons and implications behind the change in the notion of equilibrium within neoclassical prices and distribution theory, the object of the present Ph.D. dissertation. In order to illustrate Lange's theoretical evolution, in the first section I review some of his 1930's articles, specially "*The place of interest in the theory of production*" (1936a) but also some passages of "*On the economic theory of socialism*" (1936b) to show that Lange's early work fully

¹³⁴ An abridged version of this chapter will come out as an independent article, co-authored with A. Lazzarini: "On Oskar Lange's theoretical position on equilibrium and capital in some 1930's and 1940's writings", to be published in *History of economics review*.

falls within the realm of traditional neoclassical economics. Accordingly, I attempt to show that Lange's approach evidences that a supply-and-demand equilibrium is capable of having the role of a centre of gravitation of actual variables *only* if the endowment of capital is specified in value terms.

In sections 2 and 3 I proceed to examine PFE, where Lange analyses the stability of the neoclassical equilibrium by adopting the TGE framework under the influence of Hicks (1946 [1939]). In section 4 I compare Lange's approach in PFE with Keynes's and Patinkin's to further examine and discuss the aims, scopes and limitations of Lange's work. Section 5 draws the main conclusion of the chapter, which can be summarized in the following way: in his work, Lange moves from a traditional Wicksellian position in his early writings to a neo-Walrasian TGE framework in PFE under the influence of Hicks (1946 [1939]), but without having a clear understanding of the implications of this shift: despite the change in the datum relative to the capital endowment, Lange still envisages the several capital goods in a very traditional way, i.e. as embodiments of a single factor of production that is capable of changing form without changing in quantity. Thus, on the one hand, the survival of this traditional notion of capital explains why Lange still envisages equilibrium as a position that the economy tends to realize after sufficient time. On the other hand, and quite paradoxically indeed, while in PFE Lange endeavours to strengthen Keynes's negative results regarding the stability of the supply-and-demand equilibrium, given that Lange still accepts the notion of capital as a single scalar, and hence he must also accept the traditional neoclassical mechanisms and tendencies that stand or fall with this notion, I argue that his objections to the neoclassical claim that a full-employment equilibrium is a position that the economy tends to realize are quite weak, since Lange's focus is essentially on purely subjective elements (uncertainty, expectations, systematic mistakes), and hence his critique loses much of its force.

1. EQUILIBRIUM AS A CENTRE OF GRAVITATION IN LANGE'S EARLY WRITINGS.

This first section examines some features of Lange's principal contributions on equilibrium and capital during the 1930s, and discussed, specially, in "*The*

place of interest in the theory of production” (Lange, 1936a), but also in some passages of “*On the economic theory of socialism*” (Lange, 1936b). These articles can be considered fully internal to traditional neoclassical economic thought; Lange thus attempts to determine normal or cost-of-production relative prices, i.e. prices endowed with sufficient persistence so as to represent *a centre of gravitation* around which market prices tend to gravitate over a sufficient interval of time. It is, therefore, this basic aim to explain normal relative prices in terms of supply and demand schedules that forces the Polish author to specify the capital endowment of the economy in value terms. In these early writings, Lange indeed provides three main reasons for this to be the case. First, the capital endowment must be so measured because the uniform return condition requires an endogenous determination of the composition of the capital stock. As we have seen in the first chapter, this position is sufficiently persistent not to be affected by the accidental details of disequilibrium transactions and productions; second, because an endogenous determination of the composition of the capital stock also appears necessary to allow for enough substitutability among factors of production. Third and finally, because Lange’s model shows that the value measurement of capital is *necessary* to determine a position of rest that is persistent but not strictly stationary (i.e. not stationary in a secular sense).

1.1. “Money-capital” as accumulated savings.

It is the aim of Lange’s 1936a paper to provide some clarification on the relation between general price theory and the theory of interest in neoclassical economics. Lange considers a competitive economy in which there are two industries: industry I, which produces the consumption good, “wood” (x), and industry II, which produces the capital good or “real capital”, “axes” (\bar{m}). Production is assumed to take place in yearly cycles, and all capital is circulating. Wood is produced by direct labour (l) and axes (m), while axes are produced by axes (m') and what Lange calls ‘indirect’ labour (l'). Both the

consumption and the capital goods are produced under well-behaved, constant-returns-to-scale production functions^[135]:

$$x = F(m;l) \quad (1)$$

$$\bar{m} = \phi(m',l') \quad (2)$$

The assumption that the economy is in a self-replacing state implies that

$$\phi(m',l') = m + m' \quad (3)$$

Each entrepreneur maximizes her own profits, but, in order to simplify the analysis further, Lange (1936a: 171, fn. 1) only considers aggregate profits within each sector. He subsequently observes that the optimal methods of production chosen by firms are constrained by their disposal of “money capital”, i.e. an amount of value that will be necessary to purchase the required amounts of physical factors of production – axes and labour, since wages are assumed to be paid in advance- determined by the optimal methods. Then, sector I (respectively, sector II) maximizes profits π (respectively, π') subject to its availability of capital disposal κ (respectively, κ'):

$$\pi - \lambda\kappa = xp_x - mp_m - lp_l - \lambda(mp_m + lp_l) \quad (4)$$

$$\pi' - \lambda'\kappa' = (m + m')p_m - m'p_m(1 + \lambda') - l'p_l(1 + \lambda') \quad (5)$$

where p_x stands for the price of wood, p_m for the price of axes, p_l for the wage rate and $\kappa = p_m m + p_l l$ (respectively, $\kappa' = p_m m' + p_l l'$) represents the quantity of “money capital” available for sector I (sector II); λ and λ' are the Lagrange multipliers whose interpretation is discussed below. Under the admitted influence of Schumpeter (1934: 116-117), Lange characterises κ and κ' as “a general ‘command over means of production’” (Lange, 1936a: 173) but, as he clarifies at the outset of the article,

¹³⁵ It seems useful to distinguish between the role of axes as outputs (\bar{m}) and their role as inputs (m and m'). This distinction however does not appear in Lange’s original work, i.e. the variable \bar{m} is not explicitly specified in Lange (1936a). This however will not alter the results.

By ‘money capital’ in this paper the purchasing power at the disposal of entrepreneurs is meant and the only way to increase it is, according to our assumptions, through saving, i.e. through refraining from spending purchasing power for consumption and through transferring it to the disposal of entrepreneurs (Lange, 1936a: 160)

Like Schumpeter’s (*cf.* chapter 1) then, this conception of capital is fully traditional: Money capital simply represents *past* or *accumulated savings* that are, so to speak, *incorporated* in the “real capital”, i.e. axes -and, in Lange’s example, also in advanced wages. Capital being heterogeneous must therefore be measured in the same units as are savings, some composite unit of consumption that, as the passage suggests, individuals “refrain” from consuming when they save. In this passage we thus find the idea that the services that could be used in the production of consumption goods and are instead saved and devoted to the production of capital goods, have the same value as those consumption goods that are not produced.

Now, while Lange initially assumes entrepreneurs from all industries to have an exogenously given amount of money capital at their disposal, he ultimately suggests that it is actually “*the total amount of money capital in the whole economic system which is fixed and constant*” (Lange, 1936a: 176, emphasis added). Thus, Lange’s assertion amounts to saying that the capital endowment will be specified as an *aggregate sum of value* for the determination of normal prices. This means that,

$$K = \kappa + \kappa' \tag{6}$$

where K represents the value capital endowment of the economy and is a *datum* of the problem. While on the one hand it must be noticed that in the case particular considered by Lange, namely, only one capital good, the endowment of capital could be directly measured in technical units, Lange *explicitly* remarks that the one-capital-good case

[a]llows a considerable simplification... while a generalisation of the theory to the case of production of... many real capital goods does not encounter any logical difficulty” (Lange, 1936a: 159)

Then, in the general case, K must be *necessarily* measured as an amount of value. The first order conditions (FOC) of the maximization problem for the firms in industry I are:

$$p_m = \frac{F_m P_x}{1 + \lambda} \quad (7)$$

$$p_l = \frac{F_l P_x}{1 + \lambda} \quad (8)$$

Respectively, for the firms in industry II,

$$1 = \frac{\phi_m'}{1 + \lambda'} \quad (9)$$

$$p_l = \frac{\phi_l' P_m}{1 + \lambda'} \quad (10)$$

The meaning of the FOC is postponed to the following section, since we must first interpret the multipliers λ and λ' . By simple manipulations^[136] we obtain:

$$\lambda = \frac{d\pi}{d\kappa} \quad (11)$$

$$\lambda' = \frac{d\pi'}{d\kappa'} \quad (11')$$

These multipliers are dubbed by Lange “the marginal profitableness of the money capital invested” (Lange, 1936: 176). Indeed, given that an increase in k (k') by $\Delta\kappa$ ($\Delta\kappa'$) makes firms belonging to sector I (II) increase their profits by an amount $\frac{d\pi}{d\kappa}\Delta\kappa$, the multipliers measure the rate of increase of profits due to an increase in the amount of capital at the firms’ disposal, and hence the rate of interest that the firm would be willing to pay in order to borrow the extra sum $\Delta\kappa$. Hence, $\frac{d\pi}{d\kappa}$

¹³⁶ From (4) we have $d\pi = p_x dx - p_m d_m - p_l d_l$, and since from (1) we have that $dx = F_m dm + F_l dl$. So if we substitute this last condition for the expression dx in the RHS of the previous equation, and by employing FOC (8) and (9) we obtain: $d\pi = \lambda(P_m dm + P_l dl)$. But from the condition $\kappa = p_m m + p_l l$ we know that $d\kappa = p_m dm + p_l dl$. We thus obtain (11). A similar procedure allows us to determine (11’).

[i]s then the rate of interest paid for money capital. The Lagrange multiplier $[\lambda]$ is thus the rate of interest on the money capital employed by firm I. (Lange, 1936a: 176)¹³⁷

Lange then argues that, through the force of competition among firms and lenders of money capital,

[t]he total fund of money capital is distributed among the firms so that each firm pays the same rate of interest, the marginal profitableness of money capital is the same for each firm. (Lange, 1936a: 176)

This condition is formalized by

$$\lambda = \lambda' \quad (12)$$

Therefore, at a theoretical level, a first reason emerges as to why the capital endowment must be specified as a given quantity of value: equilibrium prices must yield a uniform return on the total amount of capital invested (K), which in this case equals the cost of axes plus the value of advanced wages; and for this condition to be satisfied within the system, the quantity of capital goods –but also their form, when more than one kind of capital good is considered (recall that Lange considers the one-capital-good case as a simplifying assumption only)- and hence the quantity of labour used in the production both of axes (indirect labour) and of the consumption good (direct labour) must be endogenous variables, i.e. a *result* of the adjustment process. We add the full employment condition

$$l + l' = L \quad (13)$$

and by setting $p_x=1$, we have ten independent equations, namely, 1-3, 6-10, 12, 13 to determine the ten unknowns: $\{x, \bar{m}, m, l, m', l', p_l, p_m, \lambda, \lambda'\}$ ^[138].

1.2 The role of the *direct* and *indirect* labour.

Lange then proceeds to investigate the effect of a decrease or “shortage” in the quantity of capital (K) on the interest rate (λ) and hence on the profit maximizing methods, by examining how a change in λ affects the distribution of the given endowment of labour between its *direct* and *indirect* employment.

¹³⁷ The same condition holds for firms of sector II in the case of λ' .

¹³⁸ In Lange’s original model \bar{m} is not an explicit unknown, so he has one equation less (Lange does not include equation 2). In his original model these equations are: (1)-(3), (15)-(18), (23)-(24).

But before we must first notice that, according to the conditions (7)-(10) determined in the previous section, equilibrium requires that “the prices of the factors of production are equal to the discounted value of their marginal product” (*id.*: 176) and, given that wages are paid in advance, this implies that the marginal product of direct labour is discounted once while indirect labour is discounted twice at the uniform rate of interest λ in order to be put equal to the wage. This can be readily seen by inserting (10) into (7) and obtaining $p_l = \frac{p_x F_m \phi_l}{(1 + \lambda)^2}$, where the right hand side of the equation measures the value marginal product of indirect labour ($p_x F_m \phi_l$) which is discounted twice at the rate λ . On the other hand, the equality between the wage rate and the value marginal product of direct labour ($p_x F_l$), which is discounted only once, is directly given by equation (7), i.e. $p_l = \frac{p_x F_l}{1 + \lambda}$. Equilibrium thus requires that:

$$\frac{p_x F_m \phi_l}{(1 + \lambda)^2} = \frac{p_x F_l}{1 + \lambda} \quad (14)$$

i.e., the discounted value marginal product of direct labour equals the discounted value marginal product of indirect labour. Lange thus goes on to argue that a decrease in K raises λ and, because of (14), the marginal product of direct labour will rise *relative* to the marginal product of indirect labour (*id.*: 180). To restore equilibrium, his argument goes, it is necessary that “a shift of the labour resources towards their direct use” (*id.*) takes place. The contrary is the case when the rate of interest falls after an increase in the quantity of capital. Finally, Lange proceeds to analyse the *effect* of a change in the rate of interest on relative rental prices. Dividing equation (7) by equation (8) we obtain,

$$\frac{p_l}{p_m} = \frac{F_l}{F_m} \quad (15)$$

Therefore, an increase in λ , by increasing the amount of labour *directly* employed, causes a decrease of wages relative to the price of the capital goods:

$$\frac{\partial \left(\frac{p_l}{p_m} \right)}{\partial l} = \frac{F_{ll}F_m - F_{ml}F_l}{F_m^2} < 0 \quad (16)$$

which is negative under the assumption that F_{ml} is positive. Now, as I have noticed in the previous chapters (*cf.* especially chapter 2), it is by now generally acknowledged that the possibility of reswitching and reverse capital deepening undermines the legitimacy of the kind of adjustments suggested by Lange because, given that the same technique, and hence the same distribution of labour between its indirect and direct employments, can be actually adopted at very different rates of interest, the demand for the several inputs need not necessarily vary monotonically with respect to their rentals as the rate of interest varies, implying an unstable equilibrium. Lange (1936a: 180) in fact comes close to admitting the problem by noting that, had F_{ml} been negative^[139], it could well be the case that the sign of (16) would have been positive; but this would imply that, as the interest rate increases (real wages decrease), the price of axes relative to wages decreases, hence inducing cost-minimizing firms to increase the production of the former; the overall result is that indirect labour requirements increase, rather than decrease -as the stability of the equilibrium requires- relative to direct labour units. *Without* any justification however, Lange *dismisses* this case (*id.*: 180). It is not surprising therefore that, having discarded the cases that contradict the underpinnings of the theory, Lange found that its main insights were confirmed; in other words, his defence relies on a purely tautological argument.

In any case, the point I wish to make is that the description of the working of the substitution mechanisms in terms of direct and indirect labour inputs clearly evidences the Wicksellian influence on Lange's work (as Lange himself admits in several parts of his essay, *cf.* in particular Lange, 1936a: 169, fn.

¹³⁹ It must be noted that under the assumptions of Lange's model, where $x=F(.)$ is a "well behaved", constant returns to scale production function which has uses two inputs, l and m , F_{ml} must necessarily be positive. In fact, due to constant returns to scale, we have that $F(.) = F_l l + F_m m$. Then, given that $f_l = \frac{F}{l} - f_m \frac{m}{l}$, we obtain that $f_{lm} = \frac{-f_{mm} * L_m}{m} > 0$, which is positive due to the assumption of decreasing marginal productivity of m . In order to have "perverse behaviour" of inputs demand it is necessary that the production function has more than two inputs as arguments (*cf.* Hatta, 1976).

4)[¹⁴⁰], and makes it easier to understand why Lange is forced to determine the composition of the capital stock endogenously. On the one hand, given that the (discounted) marginal product of labour must be the same in its direct and indirect employments, the distribution of these labour inputs in the different industries must be endogenously determined. And in the many-capital goods case[¹⁴¹], this requires that not only the *quantities*, but also the *forms* of the capital goods, that are used and reproduced in equilibrium by means of indirect labour, be endogenous magnitudes. On the other hand, there is a second, independent, reason for this to be case: enough substitutability among productive factors requires an endogenous determination of the composition of the capital stock because different productive methods will usually call for different kinds of capital goods into use. Although this point is not explicitly discussed in the 1936a article, the issue does emerge in Lange's (1937b) reply to Knight's (1937) review of that contribution. Knight objects that "the only use of capital admitted in Dr. Lange's set-up is that of increasing the number of capital instruments of a given kind (axes in his illustration)" (*id.*: 224). But Knight correctly notices that a more realistic model must consider that:

[t]he addition of new capital to a system will generally involve both new forms of equipment for making old products and new forms of final product (Knight, 1937: 224)

In his reply, Lange (1937b: 234) admits that "additional capital may be used, as Professor Knight is quite right in observing... to make new forms of products", but he goes on to argue that the main insights of his simplified model would not be substantially modified under this more realistic set-up: given that an increase in capital in any case implies a decrease in the rate of interest, the re-establishment of equilibrium still requires that factors are shifted from their direct to their indirect employments, so as their respective value marginal products can be re-equalized among industries. All this suggests that Lange is aware that an endogenous determination of the capital stock is also necessary to justify, in a plausible way, the working of the factor substitution mechanisms.

¹⁴⁰ As Lange himself notes in that same footnote, the main difference between his and Wicksell's approaches is that the former's analysis is a generalization of the latter's, since Lange considers the possibility of basic goods (axes), which are excluded by Wicksell.

¹⁴¹ I remind the reader that Lange considers his example as a valid simplification of the more general situation in which many capital goods and many consumption goods are produced.

1.3. A persistent, non-stationary equilibrium

In the last section of his 1936a article (Lange, 1936a: 189-192), Lange addresses a third and last feature of the centre-of-gravitation notion of equilibrium that explains why a value capital endowment must be among the data of the equilibrium rather than endogenously determined. In long-period equilibrium, he writes,

[t]he amount of capital is assumed as fixed and interest is deduced from the assumption that this amount is less than the amount required to saturate production with capital. (Lange, 1936a: 190)

On the other hand, when the secular conditions of accumulation are examined:

[T]he amount of capital is itself a variable to be determined by all the equations of economic equilibrium. In long-period equilibrium all adjustments are accomplished and the economic system becomes stationary. (Lange, 1936a: 190).

It is important to notice that, as Lange explicitly states (Lange, 1936a: 191, fn. 1), in the latter and following passages that are here reproduced, the expression “long-period equilibrium” refers to a *secular* equilibrium, and is clearly distinguished from the “static” long-period equilibrium in the sense used by traditional neoclassical authors, i.e. a situation where the quantity of capital is given while its ‘form’ changes so that the capital goods that are reproduced in equilibrium yield a uniform return on their supply price. As also clarified by Lange (*id.*), this notion of “static” long-period equilibrium is the one he uses in the 1936a article. So, while in long-period equilibrium in this latter sense the “amount of capital is assumed to be fixed”, when the secular conditions of accumulation are examined, this variable “is determined by all the equations of economic equilibrium”, i.e. it becomes an endogenous magnitude. Lange subsequently explains that

The way towards a long-period [secular] equilibrium with regard to interest is necessarily a slow one. For capital accumulation adds per annum but a small fraction to the existing stock of capital. As the annual accumulation is but a small fraction of the existing stock of capital the movements towards a long-period equilibrium in interest must be of a secular character, too. (Lange, 1936a: 191).

And he concludes that the study of these “secular trends of accumulation could be topic for a separate study” (Lange, 1936a: 191, fn.2). In his intervention in the Socialist Calculation Debate with the Austrian school (Lange, 1936b), the author

clears up his argument further and, after remarking that the “ultimate resources” are “labour, capital and natural resources” (Lange, 1936b: 58) he notes that,

As to capital, its amount may be regarded in the short-period as constant, whereas in the long run the rate of interest certainly affects savings. In long-period equilibrium the amount of capital is determined by the condition that the rate of its marginal net productivity (the interest rate) is equal to the time preference of the individuals. (Lange, 1936b: 58, fn. 3)^[142]

These pieces of evidence show that Lange’s approach is fully traditional; he thus shares the idea held by traditional authors that the process of capital accumulation is “slow” enough so then, as a first analytical approximation, the theory can neglect this issue –i.e. *assume* zero net savings, as implied by his writing ‘constant’- when explaining normal prices and distribution. The persistent, slowly-changing quantity of value capital implies that those secular movements related to significant changes in the capital disposal of the economy could be safely dealt with later on, in a subsequent analytical stage. Additionally, notice how in the latter quotation Lange openly acknowledges that the assumed constancy of the factor capital is perfectly compatible with capital accumulation over time, and hence that the equilibrium thus determined should not necessarily be stationary. Indeed, Lange proceeds, capital accumulation would come to a halt only when the marginal productivity of capital would equal the time preference of the individuals; but in any case, whatever the discrepancy between these magnitudes in the shorter run, capital accumulation “adds per annum but a small fraction of the existing capital stock”, and hence, equilibrium prices determined under the assumption of zero net accumulation, i.e. of a *given* quantity of value capital whatever prices and distribution, could be safely assumed to describe the centre of gravitation of market prices over a sufficient interval of time. Moreover, because actual economic conditions are far from being stationary, Lange might have rightly argued that a characterization of a secular equilibrium would give no indication of the actual trend of real economies. This position of Lange is fully traditional (recall from chapter 1, for instance, Ramsey’s).

¹⁴² Again, here short period must be interpreted as “static” long-period since it is contrasted with a “long-period equilibrium” which is clearly a secular one.

In this connection, it is worth adding that the slow change –i.e. slow relative to the process of equilibration- that the data determining prices and distribution may be undergoing over time is no doubt the reason why Lange, in his interventions on the Socialist Calculation Debate (Lange, 1936b, 1937a) does not feel the need to have recourse to an auctioneer-guided process of equilibration. Although he admits that, when conditions change, “a change in the quantity supplied [of commodities] requires a period of time” (Lange, 1936b: 60, fn. 1) he anyway argues that the equilibrium outcome would be slowly reached by trial and error, through a “Marshallian process of adaptation” (*id.*) such that the proportions among factors employed in each industry are gradually adjusted to the conditions of static long-period equilibrium. Moreover, in his description of the working of the price mechanism under a socialist organization of production, Lange argues that “the process of price determination is quite analogous to that in a competitive economy” (*id.*: 64) and he thus assumes time-consuming adjustments; accordingly, the “Central Planning Board” would correct the originally announced prices only after a physical shortage or surplus of the several commodities reveal themselves at the end of the relevant period, i.e. *after* transactions and productions at disequilibrium prices having *actually* taken place. Lange then proceeds to argue that after successive corrections, the equilibrium position, assumed by the author to be unique (*id.*: 64), is eventually reached.

In a nutshell, Lange’s aim, on the one hand, to characterize a persistent position of rest by determining relative normal prices and, on the other hand, his awareness that a secular equilibrium would be achieved, if at all, in the very-long run, forcedly led him to place interest rate theory in the realm of the traditional, static or long-period notion of equilibrium. Thus, I submit, although Lange is aware that, when dealing with secular conditions, the “amount of capital is itself a variable to be determined”, still, in his equilibrium system, the author has to resort to a specification of capital as a single, exogenously given magnitude in value for the determination of prices and distributive variables (wages and interest rates).

2. THE SHIFT IN METHOD IN LANGE'S *PRICE FLEXIBILITY AND EMPLOYMENT* (1944): SIGNIFICANT CHANGE IN FORM, MINOR CHANGE IN CONTENT.

We now turn to examine Lange's approach to the problem of equilibrium and capital as dealt with in PFE. The aim of PFE is to assess the neoclassical claim that, if not disturbed, the forces of supply and demand will eventually push the economy towards a position of full employment. Under this view, Lange explains, unemployment can only be the result of the "rigidity of factor prices" (PFE: 1). However, he continues (PFE: 1), this vision has been "subjected to serious criticism", for instance by Keynes in the *General Theory* (1936). Indeed, it is well known that in chapter 19 of his masterpiece, Keynes examines the possible effects on unemployment of a decrease of money wages, and he concludes that the effects are likely to be negative. PFE can therefore be read as an attempt at re-examining these results, but resorting to the method of temporary equilibrium developed by Hicks (1946 [1939])[¹⁴³].

Lange in fact underlines that Hicks's *Value and Capital* (Hicks 1946 [1939]) is the 'most up-to-date formulation' of the theory of general equilibrium (PFE: Preface). Lange thus abandons the traditional neoclassical framework used in his early writings and adopts, instead, Hicks's TGE method; accordingly, he specifies the capital endowment in physical terms, along with the expectation functions, among the data. Yet, it seems relevant to recall from chapter 4 that, since the first article where Hicks seriously attempts to develop a TGE, "*Wages and Interest: the Dynamic Problem*" (Hicks 1935), Hicks justifies the relative convenience of the TGE approach over the traditional concept of equilibrium on the grounds that the latter approach would have fallen under a "condemnation" (Hicks 1935: 456), that is to say, it would have been 'quite incompetent to deal properly with capital, or interest' outside secular equilibria (Hicks 1946 [1939]: 116, *cf.* also Hicks 1935: 456-7). As seen in chapter 4, Hicks does not object to the internal consistency of the theory, since he freely admits that the traditional neoclassical theory of capital is a "plausible theory for the stationary state" (Hicks 1946 [1939]: 119). Having wrongly restricted traditional neoclassical

¹⁴³ As we shall see in section 3, the conclusions that Lange reaches on the tendency towards a full employment of resources are very similar to those reached by Keynes in chapter 19 of the *General Theory*. The main difference between Keynes and Lange is addressed in section 4.1.

economics to the realms of secular stationary states, Hicks only disputes the *relevance* of the traditional concept of equilibrium for understanding the trends of prices and distribution ruling in actual economies. However, our perusal of Lange's 1936 articles revealed that the Polish author was at that time well aware that a long-period position is *not* a secular equilibrium.

Why, then, in 1944 does Lange finally come to adopt the TGE as *the* central concept of equilibrium? Although Lange does never explicitly justify his advocacy of the TGE method, I wish to suggest three main reasons. First, because the TGE approach allows him to explicitly introduce price and interest rate expectations into the analysis which, after Keynes (1936), had become a central topic in economic theory. Second, because by 1944 Lange seems to be more aware of the difficulties behind the specification of the capital endowment as a given amount of value. Finally and I submit, more importantly, because Lange still reasons as if the changes in the data with respect to the factor capital do not cause major modifications of the theoretical structure of the supply-and-demand approach; i.e. as if, despite the change in the specification of the capital endowment as a set of physically heterogeneous capital goods, it were still possible to characterize the neoclassical equilibrium as a centre of gravitation of actually observed variables.

2.1. The problem relative to measuring the capital endowment in value terms. The investment function.

A first indication that Lange is aware of the difficulties of conceiving the capital endowment as a single factor of production can be found in his assessment of the stability of the temporary equilibrium; in particular, in the way he justifies the negative elasticity of investment demand. From the outset of his work, Lange (PFE: Preface) points out that traditional neoclassical theory failed to properly deal with the role of money, and hence that it is the substitution among money, commodities and bonds what "provides the key for understanding the equilibrating as well as the disequilibrating processes of the economy" (*id.*). According to Lange, if unemployment causes wages and prices to fall, a position of full employment will be eventually re-established if individuals attempt to decrease their money holdings, or cash balances (*CB*), when prices (*P*) fall, that is to say, if they buy commodities or bonds. In the first

case, aggregate demand for commodities increases and the subsequent attempt of producers to satisfy the demand forthcoming ends up by raising labour demand and wages. The price level eventually returns to its old equilibrium position, compatible with the full employment of resources. If, instead, individuals spend their excess of money holdings by buying bonds, the rate of interest will decrease and investment demand will increase. So, equilibrium will be re-established if, for a given money stock (M^s), the ratio of M^s to CB rises. Following Lange (PFE: 7), we shall say that when this condition is satisfied, the monetary effect is positive. This is, according to Lange, the neoclassical position. If, on the contrary, this ratio decreases as prices fall, the monetary effect is negative. We thus have:

$$\left\{ \begin{array}{l} \text{Positive monetary effect : } \nabla P \Rightarrow \Delta \left(\frac{M}{CB} \right) \\ \text{Negative monetary effect : } \nabla P \Rightarrow \nabla \left(\frac{M}{CB} \right) \end{array} \right.$$

As will be shown in section 3, Lange argues that the monetary effect may be negative, i.e. equilibrium can be unstable, but only owing to the possible disturbing effects of price and interest rate expectations and uncertainty. Accordingly, Lange does not object the validity of the neoclassical mechanisms if the monetary effect is positive; in particular, he does not question the traditional role of the rate of interest i.e. that of bringing into equilibrium investment and saving decisions.

What I wish to notice is that Lange does not justify the negative interest-elasticity of investment demand in the traditional way, namely, by arguing that a decrease in the rate of interest induces firms to adopt more capital intensive methods of production; he assumes instead that that expected prices are independent of the rate of interest. Essentially, under the influence of Hicks (1946 [1939]: chapter XVII), Lange argues that as the interest rate decreases, for given undiscounted (expected) capital goods prices, the present value of their expected market price increases relative to their current market price, and hence profit maximizing firms are induced to ‘tilt’ their production plan to the

right: they substitute inputs *over time* by shifting their purchases of capital goods to the present in order to increase their future production of output, and hence they increase current investment demand.

Now, on the one hand it is certainly true that this justification of the negative interest elasticity of investment is foreign to traditional neoclassical thinking: Lange neglects that, if intended to represent the action of persistent forces at work, the negative relation between investment and the interest rate must be derived under the assumption of cost-of-production relative prices (*cf.* Ackley 1978: 623-34)^[144]. However, on the other hand, the traditional neoclassical conception of capital as a single value-magnitude, that is embodied in the several physical capital goods and that inversely varies with the rate of interest is *still* present in Lange's analysis of investment. First, as noticed in chapter 4, although it takes a different form, the traditional Wicksellian claim that it is possible to order the different production methods according to their degree of 'roundaboutness' is still there: when the rate of interest decreases, the 'tilting' (Hicks 1946 [1939]: 216-217) of the production plan to the right causes the *average* time-lag between inputs and outputs, to increase.

But in order to confirm that in his analysis of investment Lange still envisages the different capital goods as embodiments of a single factor, we must turn to consider Lange's analysis of the potential effects of a decrease (increase) in the aggregate marginal propensity to consume (save) (PFE, chapter IX). Not only does Lange argue that the *level* of current consumption is reduced as a result of the shift in individuals' preferences, but he also notices that the *composition* of consumption will generally change^[145]. And, because consumption goods are produced by capital goods, these changes will eventually express themselves in the savings-investment market. First, the aggregate level

¹⁴⁴ It could perhaps be objected to the above claims, that this investment function aims at determining investment decisions at a particular point of time; hence the validity of the assumption of given expected prices. However, we have already noticed in the fourth chapter that, at every instant of time, expectations are conditioned by a myriad of accidental events, which can prevent the theory from arriving at any definite result regarding the relationship between investment and the rate of interest.

¹⁴⁵ Lange writes: 'Not only a change in the level (i.e. the total amount of expenditure) of the propensity to consume, but also a change in its composition (i.e. the direction of expenditure) causes disequilibrium. The equilibrium propensity to consume therefore, implies not only a definite level but also a definite direction of expenditure' (PFE: 55, n. 18).

of savings becomes higher than aggregate investment; but second, the *composition* itself of investment will sooner or later start changing as entrepreneurs attempt, in due course, to satisfy the new kinds of consumption goods desired by the community. Lange in fact admitted earlier (PFE: 53, n. 11) that it may well happen that decreases in the aggregate marginal propensity to consume “may be associated” with *increases* in the demand for some consumption goods. And hence, owing to what he calls ‘the principle of derived demand’ (PFE: 53, n. 13), the demand for the investment goods used in the production of those consumption goods whose demand has increased, will increase as well^[146]. Lange goes on to argue that, as long as the money rate of interest remains constant, there will be a “downward Wicksellian process” (PFE: 55, n. 19) because the excess of aggregate savings over investment will cause the ‘natural rate’ of interest to decrease. He writes:

[t]he excess supply of investment goods, resulting from a propensity to consume [that is below the] equilibrium requirement, lowers the ‘natural rate’. (PFE: 55, n. 19).

Note first that, while Lange does not explicitly refer to the determinants of the natural rate, the reference to Wicksell makes it clear that the natural rate must be determined by the marginal product of capital, the single homogeneous magnitude, which in Wicksell (1934 [1901]) is unambiguously measured as an amount of value. And in any case, although Lange speaks of an ‘excess supply of investment goods’, it seems to be sufficiently clear that these ‘investment goods’ are envisaged as embodiments of a single factor ‘capital’. It is the additional savings, namely, the supply of capital as a flow, and not the relative abundance of the single ‘investment goods’, what pushes the ‘natural’ rate downwards. In fact, given that Lange openly admits that changes in the aggregate marginal propensity to consume will change the composition of investment in unpredictable ways, unless he treats each single capital good as an embodiment of a single factor, he can reach no general conclusion regarding the effects of changes in the forms and quantities of the investment goods on the rate of interest^[147].

¹⁴⁶ This is important for what follows, because here Lange is in fact admitting that no general conclusion can be reached on the evolution of the single capital goods as the marginal propensity to save changes.

¹⁴⁷ And actually, it should come as no surprise to us that Lange argues in these terms since in an earlier article of his (Lange 1938), he also assumes a short-period framework and accordingly specifies the capital endowment in physical terms. However, Lange explicitly follows Lerner’s

2.2. The analysis of accumulation

It is to Lange's analysis of accumulation that we must now turn to find the author of PFE *explicitly* admitting the problem behind the measurement of the factor capital as a single magnitude – albeit the issue is confined to a footnote. Consider what Lange writes when he deals with the effects of capital accumulation on employment (PFE, chapter XI: 67-70):

It is thus assumed in the text that none of the investment goods decreases in stock. Actually, capital accumulation need not imply this restrictive assumption. All that is necessary is an increase of the aggregate real value of the stock of investment goods in the community. The latter, however, leads straight into the tricky subject of real aggregates, namely the problem: what is to be meant by the aggregate quantity of capital in the community? The whole difficulty is avoided by the assumption made in the text, without loss of any significant aspect of the problem under discussion. (PFE: 67, n. 4)

On the one hand, the passage openly shows that, by 1944, Lange is aware that a value specification of the capital endowment is problematic for the theory. On the other hand however, closer inspection of his analysis of accumulation clearly suggests that Lange believes that the results he can easily prove thanks to his assumption that “none of the investment goods decreases in quantity” are sufficiently general, i.e. “without loss of any significant aspect of the problem under discussion”, *as if* things would continue working in the way justified by traditional theory, that considers the different capital goods as embodiments of a single factor, *value* capital.

Lange claims that decreasing returns must cause the marginal value productivity of the whole capital stock to decrease. Since Lange argues that “the demand for an investment good, like that for any other factor of production, is determined by the equalization of the marginal value productivity with the price of the good”, then, due to the law of diminishing returns, “an increase in the stock of (some or all) investment goods that is not accompanied by a

approach to investment, arguing that ‘the investment function is based on the theorem that the amount of investment per unit of time is such as to equalise the rate of net return on that investment ... to the rate of interest’ (Lange 1938: 13, n. 2), and he thus asserts that the rate of return on investment is derived from the rate of net return (marginal efficiency) on capital (Lange 1938: 13). And Lerner does speak of capital as a single factor. (Lerner's approach to investment is summarized in Lerner (1944). Cf. Petri (2004: 273-6) and Ackley (1978: 629) for a detailed analysis of this approach.) So, despite the assumption of a given endowment of physical capital goods, in Lange's (1938) analysis the negative relationship between investment – i.e. the demand for capital as a *flow* – and the rate of interest is ultimately derived from the negative relationship between the rate of interest and the demand for the factor capital as a *stock*. The physical capital goods are accordingly seen as crystallizations of this single magnitude.

proportional increase in the supply of primary factors [labour or land] leads to a decline in the marginal physical productivity of the former” (PFE: 67-8). For given selling prices of the capital goods, “this implies a decrease in their marginal value productivity and also ... in the demand for them” (PFE: 68). But then, Lange proceeds, spare capacity means that there is also labour unemployment, money wages thus fall and this “is bound to reduce the cost of production, and thus also the prices of investment goods to an extent that compensates for the decline in their marginal productivity” (PFE: 68) so that, in the new equilibrium, the new capital stock and the labour force can both be fully employed.

In PFE then, the author essentially accepts the traditional neoclassical idea of a decreasing marginal productivity of capital, which is here derived by assuming that ‘some or all’ of the capital goods increase in quantity with capital accumulation, i.e. by implicitly assuming that the changes in the kinds of capital goods in existence, generally brought about by changes in income distribution, can be neglected because they entail no difference relative to a situation in which there is no change in the physical composition of capital – which amounts to treating the different capital goods as portions of a single factor. But Lange must proceed in this way because, as capital accumulation takes place, there is no a priori reason to presume that none of the quantities of the single capital goods will fall. It will generally happen that some capital goods will increase in quantity, some others will fall, some will appear that were previously not utilized at all and others will disappear completely. In fact, we have seen that in Lange (1936a) it is clearly pointed out that, to be a plausible foundation for the factor demand curves, the substitution mechanisms should encompass an endogenous determination of the composition of the capital stock when the total amount of capital in the economy changes, as is the case with capital accumulation. Then, while it is clear that an explicit specification of the capital endowment in value terms would have directly made Lange face “the tricky subject of real aggregates” (an issue which, as we have seen, by all means Lange tries to bypass) he continues to think of the different capital goods as representing a homogenous factor of production.

The main outcome of the assumption that “some or all investment goods increase in quantity as accumulation proceeds” is that the traditional conclusion that accumulation must decrease the marginal product of capital is preserved. That is to say, Lange reasons as if the more relevant outcomes of traditional theory – the negative relationship between the demand for productive factors and their rental prices – are preserved, and therefore also, the plausibility that the economy gravitates around a full-employment growth path, despite his attempt to abandon the notion of capital as a single factor of production. Otherwise he could not conclude that the assumption regarding the evolution of the form of the existing capital stock entails no “loss of any significant aspect of the problem under discussion”, denying or forgetting that no general conclusion can be reached regarding the effects on marginal products and on prices of the evolution of the existing forms of the capital goods as accumulation takes place, unless he assumes that these effects are the same as if the several capital goods could be treated as in fact elements of a single factor ‘capital, like he had assumed in his earlier writings where this factor ‘capital’ was a quantity of value. So, Lange is conscious that unless he makes some restrictive assumption ensuring that things work out as if capital were a single factor, he cannot go on reasoning in the old, Wicksellian way. This implies that he is conscious that there is no guarantee that things work out as he wishes in the absence of the restrictive assumption; but then he is implicitly admitting that he has no right to make the restrictive assumption! And yet he makes it, evidently because of a faith that it cannot be that things work differently.

2.3. Some ‘methodological’ difficulties ignored by Lange in PFE

In the previous section we have seen that despite the formal change in the data relative to the factor capital, in PFE the physical capital goods are still seen as a single composite quantity of capital. This may help explain why Lange does not show any concern with respect to the impermanence problem, nor with respect to the substitutability problem (the indefiniteness problem due to expectations is discussed in section 3). In fact, Lange’s aim to represent plausible, time-consuming adjustments leads him to consider a balancing process where actual transactions and productions may generally take place at out-of-equilibrium prices. Consider for instance his description of the

adjustment process to the new conditions after the aggregate marginal propensity to consume decreases. He says that

[a]t the old output, the demand price for (at least) some final products is reduced, while the demand schedules for all other final products (and direct services) remains unchanged. As long as the prices of the factors of production and of all other goods (including securities) are unchanged, this results, under conditions of perfect competition ... in a contraction of output of the products for which there has been a decrease in demand. In consequence, the demand for the factors of production used in making these products also decreases. This causes excess supply of ... these factors and a fall of their prices. Substitution of these factors for other factors and expansion of output of commodities produced with them is attempted and causes a decline of the prices of the other factors. At the same time, marginal costs are reduced on account of lower prices. This causes an attempt to expand the output of (at least some) products. (PFE: 53)

The passage suggests that Lange is thinking of mechanisms which only make sense in a long-period framework, i.e. it takes a sufficient interval of time for firms to distinguish between transitory and persistent shifts in tastes, and also to realize which specific consumption goods have permanently decreased in demand. It may also take considerable time for rentals of unemployed factors to decrease. In turn, only after sufficient time will the firms' initial 'attempt' to utilize more intensively those factors whose prices have decreased effectively materialize, and also the 'attempt' to expand the outputs of those consumption goods whose demand increases on account of their lower prices. During the adjustment process to the new conditions, mistakes on the part of firms usually occur, and transactions and production decisions generally take place at 'false prices'.

And in fact, that Lange is thinking of mechanisms that may take considerable time to assert their effects fully is openly admitted in the last chapter of PFE, when the author relies on the TGE framework developed in the previous chapters to explain the behaviour of actual economies over the last two centuries. He thus argues that during the period from the 1840s until 1914 "there are good reasons to believe that [the conditions that ensure the tendency towards the full employment of resources according neoclassical theory] were approximately realized in the long-run" (PFE: 83). Accordingly, while during the 'short-run' the economy was subjected "to strong fluctuations of employment, output, and prices" (PFE: 84), still

[p]rice flexibility operated as a long-run stabilizing force in the economy. (PFE: 84)

Lange thus openly accepts that, granting stability, ‘price flexibility’ allows equilibrium to assert itself only after sufficient time. Then, this mistaken idea that, if stability can be assumed, the TGE framework can easily accommodate a realistic, time-consuming, balancing process as the one that, if any, takes place in real economies, may help explaining why, unlike Hicks in *Value and capital*, who, de facto considers that equilibrium is instantaneously reached by assuming an economy that is ‘always in equilibrium’ (Hicks 1946 [1939]: 131), in PFE ‘false’ production and trading are not explicitly forbidden¹⁴⁸].

Therefore, there is some ground for believing that Lange actually does not notice that his assumption of time-consuming adjustments and, accordingly, of ‘false-price’ trading, is in sharp contradiction with the TGE approach of a given vectorial endowment of capital goods and given expectation functions, due to the lack of persistence of these latter variables. In other words, Lange’s lack of awareness of the potential problems of path dependency, and hence of the indeterminacy of the equilibrium, must certainly be an important reason why he still assumes time-consuming adjustments, despite that the change in the data relative to the factor capital no longer allows him to proceed that way. Moreover, it is also worth pointing out that, despite his careful analysis of the substitution mechanisms within the neoclassical approach in his early articles, and which will generally entail a change in the composition of the existing capital stock, Lange does not seem to notice that, due to the specific character of the capital goods that are arbitrarily given for the first periods of the TGE, the

¹⁴⁸ Lange only limits himself to warning the reader of the possibility that “when considerable friction is present in the economy” (PFE: 19) the working of the adjustment processes, “although to be expected according to pure theory, may be hampered to such a degree by friction as to be negligible in practice” (PFE: 19). So, it seems evident that, having considered the possibility that the economy presents ‘considerable friction’, Lange cannot have been assuming that equilibrium is instantaneously reached. In this connection, we can notice that in a footnote appended to the Introduction of PFE, Lange (PFE; 1, n. 1) accepts that the “degree of realism” of the analysis could “be increased” by considering the “possible lags in reaction”. So he comes close to admitting that the time-span considered within a single TGE is not sufficiently long for equilibrium to emerge as the result of repeated interactions in the market. However, the issue is simply dismissed by Lange as being of secondary importance. The very different attitude taken by Hicks in *Value and Capital* (1946 [1939]: 212) – and which gives clear indication that Hicks is much more aware than Lange of the difficulties entailed by the notion of TGE – is expressed in his remark that ‘a theory which leaves out the probability of input lags is likely to be *gravely* misleading’. While Lange simply points out that the degree of realism ‘could be increased’ by incorporating lags in reactions, Hicks argues that ignoring this issue is “gravely misleading”.

possibilities of substitution between capital inputs and labour will actually be very limited, hence causing, e.g., implausibly low-value equilibrium factor rentals, possibly zero.

3. LANGE'S OBJECTIONS TO THE SELF-ADJUSTING NATURE OF THE MARKET

As we have seen in section 2.1 Lange argues that it is only under a "positive monetary effect" that equilibrium is stable. In Lange's view, neoclassical authors concluded that the monetary effect is positive because they implicitly relied on special assumptions with respect to i) the role of the monetary authorities in controlling the stock of money, ii) the price and interest rate expectations functions of the individuals, iii) risk and uncertainty. Taking these three features into account, Lange's objections can be summarized in four main arguments.

First, Lange argues that, even in an economy without bonds, when P falls, the monetary effect may be negative because, although the desire for CB decreases, the monetary authorities may well decide to decrease the existing M^s by a greater proportion. However, Lange immediately recognizes that, while this possibility cannot be excluded a priori, neoclassical authors assumed that M^s remained constant and, therefore, the conclusion that equilibrium is stable 'appears to be fully justified' (PFE: 14). It is thus to the remaining objections that we must now turn to understand the crucial reasons why Lange questions the general validity of the above-discussed mechanisms.

In his second and third strands of criticism, Lange points out that neoclassical theory overlooked the crucial role played by price and interest rate expectations. When this role is duly considered, Lange continues, the monetary effect may well be negative. The author thus asserts that the conclusion of a positive monetary effect actually relies on the implicit assumption of *static* price expectations (PFE: 20), i.e. that current prices are assumed to hold also in the

further future^[149]. Then, the second destabilizing cause may arise due to elastic commodity-price expectations. If the value taken by these expectations is higher than one, when the average price level falls, agents expect the future average-price level to fall even further, and hence, by shifting their purchasing decisions, both on consumption and on investment goods, from the present to the future, they find it optimal to *increase*, rather than to decrease – as the stability of the equilibrium requires – their current money holdings. (As noticed above, Lange here echoes Hicks (1946 [1939]) and labels the substitution among commodity bundles of different periods as *intertemporal* substitution or substitution *over time*, to be distinguished from the *intra-temporal* substitution that takes place among bundles of the same period). Planned sales, on the other hand, are shifted from the future to the present (PFE: 22). Assuming the stock of money to be constant, shifts in both purchases and sales therefore imply a negative monetary effect. Only when the elasticity of price expectations is equal to or smaller than one does a decrease in the average price level raise the ratio of the monetary stock to the desired cash balances^[150].

The third possible destabilizing force may come from the action of expectations over the interest rate. If, when money prices fall, individuals attempt to decrease their cash balances by buying bonds, the current interest rate will fall and, for *given* (expected) consumption goods and inputs – i.e. capital goods – prices, this will increase the price of inputs used in the immediate future relative to the price of current inputs, and also raise future output prices relative to their present prices. Therefore, firms will attempt to tilt their production plan to the right: on the one hand, aggregate planned purchases of capital goods will be shifted to the present while, on the other hand, planned sales will be shifted to the future^[151]. The overall effect will thus be a decrease in current money holdings as the tendency towards equilibrium

¹⁴⁹ Lange states that static expectations are a particular case of elasticity of expectations being equal to one; namely, that a current change in prices or interest rates changes future prices in the same proportion.

¹⁵⁰ If price expectations are equal to one, there is no intertemporal substitution, and only intratemporal substitution takes place. As prices fall, individuals decrease their money holdings and, assuming a constant stock of money, the monetary effect is positive.

¹⁵¹ While Lange considers the possibility that a decrease in interest rates may also induce households to increase their current purchases of consumption goods, he argues in several instances (PFE: 17, 27-8, 57) that the most important effect of a fall in the interest rate is on aggregate investment.

requires (PFE: 28). However, Lange warns us that, if interest rate expectations are sufficiently inelastic (zero in the limit), the expected interest rates in the more further future and, therefore, the discounted prices ruling in those more distant periods, will hardly vary relative to present prices; the tilting of the production plan to the right due to the process of intertemporal substitution will be negligible in practice and investment demand will not sufficiently increase so as to re-establish full employment.

In his fourth and last objection, Lange (PFE: 55-9) argues that even under the assumption of a positive monetary effect, full employment may not be spontaneously reached by market forces due to the destabilizing roles of risk and uncertainty, since “the intertemporal substitution resulting from the fall in interest rates may be very small and practically even negligible” (PFE: 60). First, uncertainty shortens the “economic horizon of the firm” (PFE: 60) and, since in Lange’s view (PFE: 61) the discounted values of the expected prices in the near future are hardly affected by changes in expected interest rates, this implies that “the intertemporal substitutions that fall out are the most important ones” (PFE: 61). Second, due to uncertainty, the excess of money holdings might be well directed towards short-term rather than long-term bonds, and, again, given that investment decisions are spread over long periods, Lange stresses that in order for intertemporal substitution to properly work “it is necessary that the corresponding long-term rates of interest... decline” (PFE: 62). Then, he concludes, if the increase in the demand for bonds has a short-term bias and, accordingly, fails to induce long-term interest rates to fall, the effect on real investment would be very limited.

4. SOME REMARKS ON LANGE’S EQUILIBRIUM AND STABILITY ANALYSIS.

The aim of this section is twofold. First, given the similarity between Lange’s objections to neoclassical theory and Keynes’s, a brief comparison between these two authors is useful to further understand Lange’s position on equilibrium theory. Second, given that Lange (1944) reasons as if the notion of capital as a single quantity holds, and hence he accepts the pillars of neoclassical theory, I argue that Lange’s critiques of the neoclassical approach are weak. To

sustain this claim, I will show how these critiques were subsequently overcome by the so-called neoclassical synthesis by briefly inspecting Patinkin's classic *Money, Interest and Prices* (1956).

4.1. Lange and Keynes

As hinted in section 2, most of Lange's negative results regarding the tendency towards a supply-and-demand equilibrium are anticipated in chapter 19 of the *General Theory*[¹⁵²]. The main difference between Keynes and Lange lies in the way these scholars interpret these results. As noted by Garegnani (1988: 220; cf. also Aspromourgos 1997: 123), from the instability results with flexible wages and prices Keynes draws the *conclusion* that wages must be rigid downwards, i.e. that the forces of supply and demand are unable to account for the trends in income distribution in market economies, and hence there must be other factors that persistently explain these trends; otherwise one would reach the absurd result that wages and prices would indefinitely fall in the presence of unemployment, something that has no correspondence with observed facts. This point is summarized by Klein (1947):

Within the framework of Keynesian economics wage flexibility does not correct unemployment and leads merely to hyper-deflation if carried to its logical conclusion. But in the real world one observes neither hyper-deflation nor full employment. The explanation is that wages *are* sticky; they are not flexible... Because workers do not bid against one another, we do not experience the hopeless downward spiral (Klein 1947: 90)

The previous passage by Klein traces the line that divides Keynes from Lange: unlike Keynes, in PFE, Lange does not carry his results to their 'logical conclusion', he only limits himself to adopt the *normative* position that wages should be sticky as a matter of policy (PFE: 87-8), but it never occurs to him that the instability results he reaches in his work are in fact an *expression* of a

¹⁵² When examining whether or not a decrease in the demand for money due to a fall in prices could affect the interest rate and therefore stimulate investment, Keynes concludes that if the quantity of money shrinks along with money income, 'there is ... nothing to hope in this direction' (Keynes 1936: 266). Keynes however denies that, when the quantity of money increases in wage units, full employment could be achieved, because an increase in the quantity of money could have a 'disturbing effect on confidence' (Keynes 1936: 266-7). The case of elastic price expectations is addressed in Keynes (Keynes 1936: 263, 265) and the so-called 'liquidity trap' can be interpreted as the case of completely inelastic interest rate expectations. An important qualification must be made, however: Keynes studies the possible disturbing effects caused only by long-run expectations. In his framework, short-run expectations cannot exert a disturbing influence in the economy because they are endogenously determined and hence are *correct* in equilibrium. Lange, on the other hand, by adopting the TGE method, must extend his analysis to include short-run expectations as exogenous variables.

more fundamental issue, namely, that the forces that determine income distribution *are not* the supply and demand forces. By not reaching this conclusion, Lange implicitly reveals his faith in the neoclassical approach. And in fact, Lange expresses this faith when he argues that, under its own premises regarding expectations and uncertainty, neoclassical theory ‘is perfectly valid’ (PFE: 65)^[153]

4.2. Lange, Patinkin and the neoclassical synthesis

I now proceed to assess the strength and limitations of Lange’s critiques of neoclassical theory owing to the possible destabilizing effects of expectations. It may be interesting to bring Patinkin’s *Money, Interest and Prices* (1956) into the analysis. As argued in the introduction, Patinkin’s considerations are particularly relevant for this discussion because, as a student of Lange, he takes up and develops the analytical framework of PFE. In his influential 1956 contribution, Patinkin argues that ‘expectations are not pulled out of the air, but are related to past price experience’ (Patinkin 1956: 311); he therefore denies that, e.g., interest rate expectations may exert an autonomous and long-lasting disturbing influence on the economy because these subjective elements eventually accommodate to variations in the objective factors of the theory, e.g. the marginal productivity of capital. Precisely, this is how Patinkin interprets the ‘liquidity trap’ (in PFE, the ‘trap’ corresponds to the case of zero-interest-rate expectations): as soon as the current interest rate falls, individuals expect it to return to its old level in the future. But Patinkin (1956: 354) goes on to argue that there is an objective reason for this to be the case: the minimum rate of interest below which individuals are unwilling to hold their wealth in the form of bonds ‘is not an absolute constant but reflects the public’s state of expectations at the point of time in question’. And then,

Since expectations, in turn, are based on historical experience, it follows ... that the rate of interest ... can be more appropriately interpreted as reflecting a real phenomenon:

¹⁵³ In this connection, we cannot omit mentioning Modigliani’s (1944) contribution on this issue and the similarities and differences between the approach of this author and Lange’s. Although neither Lange nor Modigliani use Keynes’s negative results regarding the tendency towards the full employment of resources to question the explanation of distribution in terms of supply and demand forces, for the reasons discussed in the main text, it is clear that Lange is aware that the negative results regarding the tendency towards full employment reached by Keynes in the *General Theory* are *not* due to the assumption of rigid wages, which is instead the well-known interpretation of Keynes (1936) put forward by Modigliani.

namely, the fact that the productivity of capital has historically been higher than (say) 2 per cent, so that when the interest falls to the neighbourhood of this level the public anticipates a subsequent rise and acts accordingly. (Patinkin 1956: 354)

Given that over sufficient time interest-rate expectations will vary in accordance with the marginal productivity of capital, inelastic interest rate expectations are viewed as an exceptional case (e.g. Patinkin 1956: 349) because, agreeing with Lange, Patinkin – and neoclassical theory in general – argues that the marginal productivity schedule is, at least over long periods, sufficiently elastic with respect to the rate of interest^[154].

We now turn to the possible instabilities caused by elastic price expectations. We have in fact already shown in chapter 4 that Hicks reached the same conclusion as Lange in this respect. However, as also shown in that chapter, Hicks (1939: 271-272) dismisses the relevance of this case by arguing that, in the light of *experience*, people usually form an idea of the ‘normal’ price of a commodity^[155], and hence “it takes a very neurotic community to show much sensitivity in total over a very short-period of actual time”, namely the time-span of a TGE. Therefore, given that individuals “do not usually expect to be able to foresee the actual prices ruling on any particular day with complete accuracy”, then “an appreciable variation from what they had thought to be the most probable price may fail to disturb their expectations at all”. (*id.*: 272). In this sense, Hicks’s conclusion is very traditional: although the stabilizing factors –as for instance, the notion of ‘normal prices’- may not be “so powerful as to prevent the system from being liable to fluctuations” (*id.*: 271), the system still has “sufficient factors of stability to enable it to work” (*id.*), that is to say, over sufficient time the tendency towards full employment of resources will assert itself. Patinkin (1956: 310-312) essentially shares Hicks’s view. Patinkin argues

¹⁵⁴ Patinkin (1956: 380) for instance writes: “Variations in the average long-term rate of interest ... have originated primarily in technological changes which have affected the marginal productivity of capital”. The reader must notice that also Patinkin, although formally adopting the TGE framework, relies on neoclassical gravitational ways of reasoning that presuppose the traditional notion of capital as single factor of production.

¹⁵⁵ As argued in chapter 4, Hicks’s attempt to introduce the notion of a ‘normal price’, and that presumably emerges as the result of repeated interactions of the individuals in markets, is hardly defensible within a TGE framework because it presumes persistent data, and hence in a heterogeneous-capital-goods world, the notion of capital as a single factor. But in any case, as also argued in that chapter, Hicks’s argument is an *expression* of the survival of traditional gravitational ways of reasoning in a framework like the TGE that is clearly inhospitable to those reasonings.

that, even if elastic price expectations initially trigger an increase in current money prices, the negative real-balance effect caused by the continuous rise in these prices will sooner or later be sufficiently important to the extent that individuals

[j]ust do not have the means by which they can indefinitely increase their demands in accordance with their expectations. Hence, after a certain point, these expectations will cease to be self-justifying; and accordingly, after a still further point, they will be replaced by more stable ones which reflect the levelling-off of prices. In brief, the presence of inflationary expectations may well make the price level rise above its... equilibrium level at some stage of the dynamic process; but the real-balance effect ... will ultimately push it downwards again. (Patinkin 1956: 311)^[156]

But interestingly enough, even Keynes admits that his negative conclusions regarding the alleged self-adjusting nature of market economies would be seriously impaired if they had to depend on the possible destabilizing role of expectations. He writes to Kalecki:

I hope you are not right in thinking that my *General Theory* depends on an assumption that the immediate reaction of a capitalist is of a particular kind ... I regard behaviour as arrived at by trial and error, and no theory can be regarded as sound which depends on the *initial* reaction being of a particular kind. (Keynes 1973-79, vol. XII, p. 797, also noticed by Petri 1997: 28, n. 32).

Keynes thus acknowledges that individuals' initial expectations are essentially *irrelevant* to understand the behaviour of the system over sufficient time, because these expectations are modified later on in the light of experience, and hence they must be considered as endogenous magnitudes if the theory is to arrive at definite conclusions.

These considerations by Patinkin, Hicks and Keynes thus help chiefly to understand why subsequent neoclassical literature managed to incorporate Lange's kind of concerns into orthodoxy: given that the analysis of PFE does not question the pillars of neoclassical theory, in particular, the notion of capital as a single factor, and on which the tendency towards the full employment of resources is ultimately assumed to rest, the possible destabilizing role of those

¹⁵⁶ Patinkin (1956: 311) also notes that in order for price expectations to exert a destabilizing influence, the elasticity of expectations should be greater than one. If they are equal to one, then 'there will be no further increase in current demand as a result of inter-temporal substitution'. And at the same time, there is the 'dampening pressure' of a negative real-balance that implies that 'the stability of the system would be assured'.

subjective factors referred to by Lange were easily dismissed by subsequent literature as being of secondary importance. In this sense, as stressed by Patinkin and Keynes, it is experience over the underlying, persistent conditions of the market what eventually makes individuals to endogenously adapt their expectations. So, when these conditions change, when, e.g., the ‘natural’ rate of interest decreases because the aggregate propensity to consume decreases (something that, as seen in section 2.1, Lange accepts), it usually takes considerable time for individuals to adapt to the new ruling conditions. Accordingly, Patinkin and the so-called ‘neoclassical synthesis’ in general, do not deny that over a shorter interval of time, this lack of experience may cause other more transitory elements – e.g. political news, the media, possible future conditions etc. – to influence the current state of expectations, and destabilize the economy in the way suggested by Lange. However those scholars also point out that, if those other more persistent causes that according to neoclassical theory ensure the tendency towards the full employment of resources (and whose action, we may insist, is not denied by Lange) are given sufficient time to assert themselves, then those more passing disturbing factors like elastic price expectations, or inelastic interest-rate expectations, will eventually accommodate to the new conditions, i.e. will *not* play a truly *autonomous* destabilizing role in the economy.

In fact, Lange acknowledges that the destabilizing force of expectations is actually an *endogenous* response to situations that can make individuals lose their notion of what is ‘normal’ (PFE: 85; the two World Wars and the Great Depression are given as possible examples). But then, one would expect that once these events cease to exert a disturbing influence on the economy, also expectations will be subsequently corrected, and accommodate to the new ‘normal’ conditions. Lange indeed does not deny that this will be the case, he simply points out that this adjustment process may take ‘a long time’ (PFE: 85), and hence exhorts the Government to accelerate it by direct intervention. In view of the foregoing considerations, it seems likely to conjecture that a traditional neoclassical economist – or an economist belonging to the neoclassical synthesis – would not have essentially disagreed with Lange in her policy prescriptions. At a theoretical level however, the possible disturbing effect

of expectations would be confined to the short-period when, under more particular assumptions, the possible causes of the trade cycle around a full-employment position are dealt with. To this, we may add that this is essentially the method Lange himself pursued in his early writings.

The role of expectation functions in PFE allows us to trace two last, but not less serious, drawbacks in Lange's analysis. First, the author does not notice that the TGE assumption of exogenously given expectation functions introduces a serious element of indeterminacy into the theory. For instance, Friedman (1946) pointed out in his assessment of PFE that,

[Lange] seeks to enumerate all possible economic systems to which these functions [excess demand functions which depend, in turn, on price expectation functions] could give rise. The kind of economic system and the results in that system will depend on the specific character of the functions and their interrelations, and there *clearly are a very large number of permutations and combinations* (Friedman 1946: 618; emphasis added).

Secondly, given that the introduction of expectation functions allowed Lange to provide an *ad hoc* justification for both stable and unstable equilibria, in his framework it is always possible to reconcile economic phenomena with the theory; the latter thus becomes tautological. Then, this lack of definite conclusions only allows Lange to argue *ex post* that, given an *actual* situation of economic stability (instability), the parameters of the fundamentals must have been such so as to generate a stable (unstable) equilibrium. (*Cf.* e.g. Lange's documentation of the trends in the capitalist system since the 1870s in PFE: 83-5.) As Patinkin (1956: 257) summarizes it, 'expectations ... introduce many additional "degrees of freedom" [and hence] we can obtain any conclusion we might desire'. But also Friedman noticed this second shortcoming, and claimed: "A theory that has no implications that facts, potentially capable of being observed, can contradict is useless for prediction: *if all possible occurrences are consistent with it, it cannot furnish a basis for selecting those that are likely*" (Friedman 1946: 618; emphasis added). He further remarked that, in Lange's 1944 work:

For the most part, the crucial question, 'What observed facts would contradict the generalization suggested, and what operations could be followed to observe such critical facts?' is never asked; and the theory is so set up that it could seldom be answered if it

were asked. The theory provides formal models of imaginary worlds, not generalizations about the real world. (Friedman 1946: 618)^[157]

5. CONCLUDING REMARKS

The overall conclusions from the foregoing analysis can be summarized as follows. Under the influence of Hicks (1946 [1939]), Lange moves from an essentially Wicksellian position in his 1936 writings to a neo-Walrasian temporary equilibrium framework in PFE, without showing a clear understanding of the implications, both theoretical and methodological. At a methodological level, Lange seems to be less conscious than the other forerunners of the neo-Walrasian approach, Hicks in particular, of the new problems that appear when the set of capital goods is specified among the data of the equilibrium. Confirmation of this is that, unlike Hicks, and despite Lange's considerations regarding the need to specify the capital endowment in value terms examined in his 1936 writings, in PFE he does not show any concern with respect to the 'impermanence' and the 'substitutability' problems, nor with the indeterminacy of results due to expectations. The importance of these issues should not be underestimated: as argued, Lange's analytical framework has been subsequently adopted and developed by his pupil Patinkin (1956), whose contributions have been no doubt highly influential in the subsequent development of neoclassical theory. In view of this fact, it can be plausibly conjectured that Lange's vivid discussion of the equilibration process towards a temporary equilibrium, as if this framework could easily accommodate time-consuming, realistic adjustments, may have had a considerable influence on the subsequent development of the theory, by easing the spread of the mistaken idea that, if stability could be assumed, the TGE – or the sequence of TGEs – could be susceptible of having the role of a centre of gravitation of actually observed prices and quantities.

At a more theoretical level, an examination of PFE gives a clear indication that the change in the specification of the factor capital does not prevent Lange – nor subsequent neoclassical literature, e.g. Patinkin (1956) – from continuing

¹⁵⁷ Friedman's negative remarks on PFE were not the only ones. It must be noticed in this connection that PFE was also criticized by Harrod (1946) (*cf.* also Timlin 1946). At any rate, as argued in the main text, PFE would gain recognition when taken up by Patinkin (1956).

to believe in the traditional notion of capital as a single factor, and hence in the basic traditional mechanisms that stand or fall with that notion. Paradoxically, it is the faith in these traditional tendencies that seems to have prevented Lange from developing fully convincing reasons to question the tendency towards the full employment of resources. In other words, given that in PFE the pillars of neoclassical theory are not questioned, Lange is forced to confine the possible destabilizing causes to purely *subjective* elements (uncertainty, expectations, systematic mistakes), which seem to lose importance over longer periods relative to those more objective factors that are supposed to regulate the profitability of investment in neoclassical theory (e.g. the marginal product of capital), and that ultimately justify the tendency towards the full employment of resources. In a nutshell, Lange's inability to abandon traditional neoclassical ways of thinking, even in a framework like the TGE that is completely inhospitable to them, helps understanding why subsequent literature – e.g. Patinkin (1956), and more generally, the so-called 'neoclassical synthesis' – easily managed to dismiss the relevance of Lange's sort of critiques by interpreting them as *frictions* or *imperfections* that, although capable of explaining the short-run fluctuations of the economy, would vanish over longer periods of time.

PART III

MAURICE ALLAIS'S NOTION OF EQUILIBRIUM

6.

ALLAIS'S TRAITE D'ECONOMIE PURE AND ÉCONOMIE ET INTERET

INTRODUCTION.

Allais is no doubt one of the most important authors of the 20th century, and several scholars, e.g. Grandmont (1989), Dréze (1989), Munier (1995) and Arena (2000) among others, have all stressed the richness of his work. Moreover, contemporary commentators have also recognized that Allais's major publications during the 1940's, i.e. *Traité d'économie pure* (1943, henceforth, TEP) and the continuation of that work, *Économie et intérêt* (1947, henceforth, EI)^[158], contain important and original contributions to neoclassical theory of prices and distribution; just to name a few, in his TEP Allais gives the earliest formalization of an intertemporal general equilibrium (IGE) in a finite horizon economy, later or extensively developed by Arrow and Debreu (1954) and Debreu (1959), and he offers a proof of tâtonnement stability under similar conditions than those of gross substitutability (Negishi, 1962). On the other hand, in EI, Allais became the first author to model an overlapping generations (OLG) economy (Malinvaud, 1986, 1987), later on rediscovered by Samuelson (1958), and he also introduces the golden rule of capital accumulation, popularized several years later by Phelps (1961).

Currently however, Allais is probably better known for his work on decision theory –e.g. the well known “Allais's paradox”- and secondary literature devoted to his early contributions on the theory of prices and distribution is very limited indeed. In this regard, his work during the 40's is barely mentioned by some well-known contemporary books devoted to the history of economic analysis (e.g. Screpanti and Zamagni, 2005; De Leplace, 2008; Negishi, 1989) or not mentioned at all (e.g. Blaug, 1985; Roncaglia, 2006); moreover, his early

¹⁵⁸ Indeed, TEP and EI must be conceived as a part of one major treatise in economic theory, as Allais (1994: 19) acknowledges in the introduction to the third edition of TEP. In TEP, Allais usually refers to chapter VII as the one that will specifically deal with capital and interest; this chapter is missing however, and its content is the subject of EI.

contributions on price and distribution theory have neither received the deserved attention of those studies specially devoted to the history of general equilibrium analysis (e.g. Weintraub, 1985; Ingrao and Israel, 1990; Mandler, 1999; Tieben, 2009) and, finally, the list of published papers devoted to studying Allais's contributions on capital and equilibrium theory is very limited (Weintraub, 1991 deals with Allais's proofs of stability in TPE; Malinvaud, 1986 explains some features of the OLG model, and specially Béraud, 2008; 2010, who compares some features of Allais's early work with Hicks's). The lack of attention on Allais's work on equilibrium and capital during the 1940's is quite surprising indeed, if for no other reason, because some of the most important neo-Walrasian scholars of the second half of the 20th century, such as Debreu and Malinvaud, have both acknowledged to be disciples of Allais^[159].

In this regard, this sixth chapter has two main objectives. In the first place, in view of the abovementioned lack of interest that Allais's thought on equilibrium and capital has received so far, I aim to examine Allais's TEP and EI to analyze the aims and scopes of his contributions on these topics. In the second place, and more specifically, I aim to show that, while Allais might be seen as one of the founders of the neo-Walrasian school, the line of continuity between his own work and subsequent neo-Walrasian theory is however very limited. In fact, while on the one hand in TEP Allais develops an IGE model *tout court*, in EI on the other hand, where he specifically deals with the topics of capital and interest, he abandons the realms of IGE and closely follows the Austrian school; accordingly, he concentrates on stationary states ("régimes permanents"). Walras's work, e.g., is simply not mentioned in EI. In this sense, the aim of this chapter is to show the *specific* reasons given by Allais to abandon the notion of IGE and to embrace, instead, the more traditional notion of stationary state.

Three main sections follow this introduction. Section 1 discusses Allais's notion of equilibrium. I will show that he assigns to equilibrium its traditional

¹⁵⁹ For the case of Malinvaud, cf. e.g. Krueger's (2003: 182) interview to Malinvaud, where to the question "where there particular teachers who had an influence on your interest in economics?", Malinvaud replied "Certainly there was one, Maurice Allais". For the case of Debreu, cf. e.g. Dréze (1989: 12), where he quotes Debreu acknowledging: "to nobody is my debt greater than to Maurice Allais, whose interest in this kind of question [Pareto optimality] is the origin of mine".

role, a position that the economy tends to realize after sufficient time; therefore, to have an explanatory-predictive role, Allais argues that this equilibrium concept should be determined on the basis of *plausible* assumptions; moreover, it must be *stable* and *persistent*. Section 2 shows, on the one hand, that Allais puts forwards strong reservations against the notion of IGE, precisely because it is based on unrealistic assumptions (complete markets, finite horizon life-span, perfect foresight, the auctioneer-guided tâtonnement) that cannot be reasonably justified within the intertemporal framework, and which seem to deprive the IGE to have a clear correspondence with observation. On the other hand, we shall see that Allais seems to envisage the stationary framework as a more plausible setting than the IGE to indicate the trends of actual economies. Finally, the section discusses Allais's treatment of the physical endowments of capital goods in some parts of TEP: it is there argued that, while in the formalization of the IGE provided in TEP the vectorial endowment of capital goods is included as part of the data of the equilibrium, these variables are in fact considered as *endogenously* determined magnitudes, somehow anticipating the treatment of capital we find in EI.

Section 3 presents the stationary framework as Allais develops it in EI. In the first place, it aims to show how in the attempt to determine a stationary equilibrium, Allais definitely abandons the neo-Walrasian framework of a given endowments of capital goods, and he resorts to a value conception of the factor capital, capable of changing 'form' without necessarily changing in quantity. I will additionally show the connection between the demand for capital as a stock and the demand for capital as a flow (investment) in Allais's framework, a connection that allows us to understand why he argues that the interest rate must be determined in the savings-investment market. The second goal of this third part consists of showing that, while Allais is aware of the "supply side problem of capital", he avoids facing the negative consequences of this issue because he determines a *secular* stationary state. It will be argued in this connection that this solution to the problem of capital from its supply side is unsatisfactory since secular equilibria, like IGE, are also inconsistent with a notion of equilibrium as a centre of gravitation. Section four concludes.

1. ALLAIS'S CONCEPTION OF EQUILIBRIUM.

1.1. The realism of the assumptions.

This first section discusses in some detail Allais's notion of equilibrium. Allais argues that, while abstraction is no doubt necessary in economic theory, the assumptions made by the theorist should be capable of some justification. "Any model", Allais (1971) explains,

[i]s necessarily *an abstraction of reality*. Obviously, no theory can represent all reality. But while abstraction is a necessity, the way it is done is not a matter of indifference. *Reality can be simplified to advantage without danger if the simplification is such that it does not change the real nature of the phenomena. On the other hand, under no circumstances should the [aim] for simplification lead to a change in the nature of reality* (Allais: 1971, 168, emphasis in the original)

"Any simplification that is liable to change completely the underlying nature of phenomena", Allais concludes, "must be avoided" (*id.*: 169, emphasis in the original) and also that "assumptions should not be chosen as a function of theory; it is the theory that should be chosen so that its assumptions are in line with observed data" (*id.*, emphasis in the original). These passages are clear: Allais simply claims that the simplifying assumptions should be *plausible*, i.e. they should ease the understanding of reality but they should never be ad-hoc hypotheses that substantially change the nature of the phenomenon under examination.

1.2. Equilibrium should be stable and persistent.

We must now turn to consider that in Allais's view, given that the real economy is actually never in equilibrium, the equilibrium position is an interesting object in itself only to the extent that it describes a position that the actual economy *tends* to realize. He writes:

L'Économie réelle ne montre en fait qu'un déséquilibre perpétuel et l'équilibre théorique ne présente *d'intérêt que dans la mesure où il est l'aboutissant d'une évolution hors d'équilibre*. Il en résulte que, même au point de vue théorique, l'on ne saurait se limiter à la considération de l'équilibre et qu'une Dynamique du déséquilibre reste nécessaire, si l'on veut comprendre la pleine signification de la théorie de l'équilibre. (TEP: 549, emphasis added)

And in fact, he strongly objects to those scholars who simply assume the equilibrium prevails.

Nous ne saurons donc trop souligner le grave danger de la position que tendent à prendre certain économistes mathématiciens, qui en arrivent à substituer à une économie sans cesse en évolution un modèle mathématique abstrait, figé dans des équations. (TEP: 549)

These considerations are fully traditional: they simply stress that the relevance of the theory presupposes that equilibrium must be stable; as argued in chapter 1 (sections 1.1. and 3), an unstable equilibrium would cast serious doubts about the plausibility of the forces assumed by the theory to explain or predict the trend or average of the observed magnitudes. As we shall see in greater detail below however, the stability of the equilibrium is necessary, yet no sufficient to argue the *actual* tendency of the economy towards the equilibrium determined by theory: granting stability, Allais is particularly clear that this alleged tendency can be plausibly justified only if the givens of the theory are sufficiently *persistent* relative to the process of equilibration. It seems better however, to arrive at this last issue by steps. Then, it might be worth pointing out first that, in both TEP (e.g. pp. 216-217; 443-444; 671-672) and EI (e.g. pp. 27-28,fn1) Allais distinguishes between two kinds of economic “laws” or “theories”: i) the theories about “effets primaires” or “immédiats” and ii) those about “effets secondaires” or “durables”.

As to the first ones, Allais explains that they are “theories of disequilibrium”, in the sense that they deal with transitory effects that are bound to disappear within short intervals of time. These laws are thus incomplete because some of the endogenous variables –the most *inert* ones- have not yet adjusted, and hence the system is still out of equilibrium^[160]. More importantly perhaps is to notice that Allais additionally argues that the plethora of causes that may affect the object of study over short intervals of time prevent those “primary” theories from arriving at definite conclusions regarding the exact position of the object in

¹⁶⁰ “En fait, dans l’Économie réelle on observe immédiatement certaines modifications que nous avons appelées *effets primaires ou momentanés*. Ces modifications correspondent à l’évolution du système hors d’équilibre sous l’influence des variations de structure (modifications des goûts, des techniques). Ces modifications sont distinctes des modifications qui correspondraient au passage direct à l’équilibre et que l’on peut appeler *effets secondaires ou durables*... D’une façon générale nous appellerons théories primaires celles qui ne s’appliquent qu’aux effets primaires et théories secondaires celles qui s’appliquent aux effets secondaires. Les théories primaires sont des théories incomplètes qui ne rendent compte que d’une partie des phénomènes. En général les lois qu’elles énoncent relèvent de l’observation courante et sont le plus souvent intuitives. Les paramètres, qu’elles supposent constants, sont ceux qui sont les plus inertes et qui en première approximation peuvent être considérés comme non variables.”(TEP: 216-217)

the short period. The second kind of theories bears a higher theoretical hierarchy because they capture the action of those durable or long-lasting causes that affect the object of study, and therefore, the equilibrium determined by those forces can be reasonably expected to describe the average position of the object over sufficient time. Consider for instance what Allais writes about the movements of the sea:

Les mouvements économiques sont absolument comparables aux mouvements de la mer. Si l'on se place à un instant donné du temps, la surface de la mer est ridée d'une multitude de vagues et sa considération ne mène à l'existence d'aucune régularité quelconque. Si au contraire on considère un période plus étendue, de l'ordre d'une dizaine de minutes, on voit qu'en tout point existe un niveau moyen, que la surface formée par les différents niveaux moyens est régulière et qu'en un point donné elle peut être assimilée à un plan. Toutefois, on constate que ce plan n'est pas une position constante dans le temps et qu'il se déplace; c'est le phénomène des marées. Si cependant on considère une période plus longue de l'ordre d'une année par exemple, on constate que le niveau moyen de ce plan est bien déterminé et que le lieu de ce niveau moyen constitue une surface régulière qui serait en fait la position d'équilibre de la mer, si l'action perturbatrice de la lune n'existait pas. (TEP: 443-444)

In accordance with the division traced, at a methodological level, between “primary” and “secondary” laws, Allais points out that it is actually impossible to predict the level of the sea at *each* instant of time: in all likelihood, Allais reasonably argues, the surface of the sea will be subjected to any sort of accidental disturbances (it will be “wrinkled by a multitude of waves”). The implication, Allais acknowledges, is that the actual level of the sea, as it is actually observed at any given time, is subjected to “*no regularity whatsoever*”. But these countless disturbing factors are purely accidental in their effects, and they will most likely disappear with time. It is therefore to the durable forces that Allais turns to explain the equilibrium level of the sea, because over sufficient time, one may plausibly expect its *average* level to be determined by these causes, and only by them; and the *actual* level, no doubt constantly affected by countless accidental factors, will in any case oscillate around this average or equilibrium position.

Economic movements, Allais notes, are “absolutely comparable” to those of the sea; it is worth quoting the analogy in extense; for the reader’s convenience, I will divide the quotation in four parts ([A], [B], [C] and [D]). He explains,

[A] Aux mouvement des vagues correspondent des *équilibres instantanés* qui s'établissent sous l'action des conditions relatives à l'époque et au lieu particuliers

considérées. Aux diverses positions de la surface moyenne de la mer correspondent des *équilibres médiats* qui s'établissent sous l'action des conditions moyennes relatives à un espace et un période de temps donnés. A la position moyenne de la surface moyenne de la mer correspond enfin un *équilibre de longue durée* qui est celui qui *s'établirait* si les actions perturbatrices n' existaient pas et si les conditions de structure restaient constantes.

[B] Naturellement ce parallèle comporte des limites et il ne constitue qu' une image commode. En fait, alors que les conditions qui déterminent le niveau moyen des mers restent immuables, il n'est pas de même de celles auxquelles pourraient correspondre des équilibres économiques de longue durée. Les conditions qui définissent la structure de l'économie subissent en effet des modifications d'allure systématique dans le temps en raison de l'évolution juridique, psychologique, technique... de l'Économie.

[C] Les déplacements des équilibres instantanés et de équilibres médiats correspondent aux effets primaires, les déplacements des équilibres de longue durée aux effets secondaires.

En fait, dans la théorie économique, il faut tenir compte de l'idée de temps et distinguer les prix momentanés de ceux que se forment pendant des périodes plus ou moins longues. Ainsi le prix instantané se détermine compte tenu de la demande et de la quantité disponible du bien, c' est-à-dire d'un façon indépendante du coût; pendant les périodes moyennes, le coût marginal tend à s'adapter au prix de marché, de sorte que le prix médiat est égal au coût marginal; au bout d' une longue période enfin, le prix tend à s'adapter au coût moyen, de sorte que le prix d'équilibre est égal à ce coût.

[D] Les prix momentanés d' une marchandise, son prix courant sur le marché est mobile comme les vagues de la mer parce qu'il se forme dans les conditions journalières de l'offre et de la demande, conditions sujettes à des changements rapides et passagers. Après une pêche abondante, le prix du poisson baisse, il monte quand le marché est mal approvisionné par suite d' une circonstance fortuite, comme une tempête ou une grève de services de transport. Mais les changements accidentels de l'offre et de la demande apparaissent comme négligeables quand on cherche les lois de la formation des prix pendant une période assez longue pour que leur influence ait les temps de s'effacer par le jeu des moyennes. (TEP: 444, emphasis in the original)

In paragraph [A] Allais observes that in economic theory it is possible to distinguish among three kinds of equilibria: the instantaneous equilibrium, which depends on particular –i.e. transitory- conditions; the medium-term equilibrium and, finally, the long-period equilibrium, which describes the expected, average position of the economy over a sufficient time. The first two equilibria are explained by those “primary laws”, while “secondary laws” explain the last long-period position of the economy. As Allais argues in [C], it is the different time-spans considered, and hence the different variables that are taken as given, what actually defines the different equilibria. For instance, in the determination of “instantaneous equilibrium prices”, only *market* supply and demand conditions matter. However, when more time is allowed, supply is able to adapt to expected demand, and therefore commodity prices will tend to coincide with minimum-average costs. As Allais had earlier argued in TEP, it is because “les facteurs de production ne peuvent être modifiés instantanément et sont relativement inertes” (TEP: 384), that the adjustment of supply conditions to their demand forthcoming may take considerable time-spans; it is only with

time that firms can adopt cost-minimizing techniques of production, and hence to change the quantity of factors employed, so that prices tend to minimum-average costs. This last condition, a “durable” cause, is the feature that defines the equilibrium of “long dureé”.

Passage [D] is perhaps more interesting because Allais readily admits there that those so-called “instantaneous-equilibrium” prices, and their associated quantities, will, like the waves of the sea, suffer from constant disruptions because their determinants are subject to continuous changes which are both fast and transitory. On the one hand these countless accidental factors -as for instance bad weather conditions, or a strike, as Allais acknowledges- may render impossible to determine those “instantaneous” magnitudes with sufficient approximation; on the other hand, it is implicit in Allais’s argument that the *evanescent, short-lived* character of these factors will no doubt jeopardize the convergence towards the instantaneous equilibrium –hence its plausibility as an outcome of some realistic adjustment process- since it is highly likely that before these very-short-run equilibrium prices have time to emerge, the primary determinants themselves have already changed. If we take these shortcomings into account then, it is easy to understand why Allais seems to minimize the importance of instantaneous-supply-and-demand equilibria: being determined by purely fortuitous and accidental events that would in any case disappear with time, the chaotic disruptions of instantaneous-equilibrium prices could be *safely* neglected when explaining the long-lasting determinants of equilibrium prices^[161]. It would thus seem that, although in Allais’s framework instantaneous-prices imply, somehow, equilibrium between supply and demand, this equilibrium concept should be better interpreted as a synonym of market (empirical) prices. Indeed, Allais observes:

Il ne saurait y avoir d’équilibre *proprement dit* que si le prix du marché est égal au coût minimum. (TEP: 385, emphasis added)

Notice then how Allais identifies the “*equilibrium of longue durée*” –i.e. the position such that the condition price equals minimum average cost is satisfied- with the unique *proper* equilibrium: as explained already, only the “durable” or

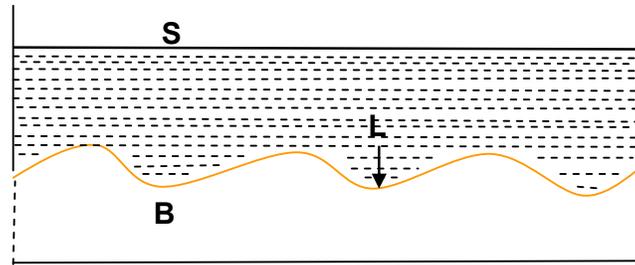
¹⁶¹Although Allais does not explicitly recognise it, the reader should notice that Marshall’s influence in his work is notorious (*cf.* e.g. Marshall, 1890: 349-50).

persistent causes may provide an accurate indication of the expected or average position of the economy. In fact, note that in [D] Allais identifies the instantaneous price of a commodity with “son prix courant sur le marché”; that is, he makes explicit the identification of instantaneous-equilibrium prices with market (empirical) prices.

The discussion of paragraph [B] has been intentionally left to the end because it highlights that Allais *does not* actually consider these durable or long-lasting determinants as *invariable*; these conditions are so stringent that they would be hardly met in the real world. Allais notices in fact that in economic phenomena the underlying conditions, i.e. the *givens*, are continuously changing. But this in no way undermines the relevance of the equilibrium to have a correspondence with observation, for the usefulness of the equilibrium concept only requires that the exogenous or given variables are sufficiently persistent relative to the process of equilibration.

That it is the *relative slowness* of the speed of change of the exogenous variables and not their *absolute invariability* what the French author has in mind to justify the role of equilibrium as a position that the actual economy tends to realize is made explicit in chapter IV of TEP (pp. 545-546) by means of the following example. Allais considers a basin full of a viscous liquid (L) as in Figure 1. Owing to the chemical characteristics of the liquid, it exercises a constant pressure (down-pointing arrow) over the irregular bottom of the basin (B) and hence the latter is constantly being “deformed” by the action of the former. Now, the question is, if we put a certain amount of liquid L in the basin, given that the bottom will be in fact constantly deformed by the action of L itself -and of course, assuming that the surface of the liquid (S) would have reached a position of rest had the liquid not deformed the bottom of the basin - is it possible to say that S will be in equilibrium?

Figure 1



We have to compare, Allais asserts, two different speeds: the *speed of deformation* of the basin due to the viscosity of the liquid and the *speed of adaptation* of the surface of the liquid towards its equilibrium level –i.e. the position the surface had reached had the chemical features of the liquid been non-existent. And then, Allais argues:

En faisant usage d’une telle notion, on pourra alors dire que si la vitesse de déformation est suffisamment petite par rapport à la vitesse d’adaptation, autrement dit si l’équilibre tend à chaque instant à se rétablir plus vite que le fond ne se déforme, on pourra à tout instant considérer en première approximation la surface S comme une surface plane et que cette approximation sera d’autant meilleure que le rapport de la vitesse de déformation à la vitesse d’adaptation sera plus faible. Si au contraire le fond se déforme beaucoup plus vite que l’équilibre ne tend à se rétablir, la surface S sera considérablement déformée et elle le sera d’autant plus que le rapport considéré des vitesses sera plus grand. (TEP : 545)

Notice then that, to argue that the surface S does show a tendency towards equilibrium is enough that its “speed of adaptation” –i.e. the rate of change of the endogenous variable S- is of greater order of magnitude than the “speed of deformation” –the rate of change of the exogenous variable B, due to deterioration. If V_a and V_s stand for the speed of adaptation and deformation respectively, and $V=V_a-V_s$ represents “the effective speed towards equilibrium” (TEP: 547, fn. 5), then Allais observes that, as long as $V=V_a-V_s$ is positive (negative) the system actually *tend* towards (move away from) its equilibrium position. In economic theory, Allais continues, the same principle applies.

Dans l’Economie réelle lorsque le marché n’est pas trop imparfait et lorsque les prévisions sont suffisamment justes, autrement dit lorsque les modifications non prévues de la structure dans l’unité de temps sont suffisamment petites par rapport aux vitesses d’adaptation, on peut considérer, en première approximation, les paramètres économiques comme fournis par les équations de l’équilibre. (TEP: 548, emphasis in the original).

On the contrary,

Dans l’évolution hors d’équilibre de l’Économie et lorsque les conditions de structure varient très rapidement de telle sorte que le jeu des mécanismes d’adaptation se trouve constamment dépassé par de nouvelles modifications non prévues du milieu, les valeurs

des paramètres économiques ne sauraient être considérées, même en première approximation, comme les solutions des équations générales de l'équilibre. (TEP : 548-549)

Allais's conception of equilibrium can be briefly summarized in the following way: to be capable to sufficiently approximate actual economic conditions, the equilibrium position should be determined on the basis of *realistic assumptions*. Moreover, Allais continues, the relevance of the theory to have a correspondence with observation also requires that there must be an *actual* tendency towards the theoretically determined magnitudes. Granting stability, the fact that real adjustments are time consuming implies that the data of the equilibrium must be sufficiently persistent relative to the adjustment process. In the next and following sections we shall see that, in Allais's view, the unrealistic assumptions (e.g. complete markets, perfect foresight, finite-horizon economy) needed to determine an IGE seem to deprive this equilibrium concept of plausibility; and we shall also argue that Allais envisages the notion of stationary state as a much more plausible equilibrium concept to explain the trends of actually observed prices and quantities.

2. INTERTEMPORAL EQUILIBRIUM AND STATIONARY EQUILIBRIUM IN ALLAIS'S WORK.

2.1. Intertemporal general equilibrium (IGE) in TEP.

In TEP (p. 514 and Annexe I H, pp. 845- 846), Allais gives the earliest formalization of IGE in a finite horizon economy (Belloc and Moreaux, 2008: 117). He also assumes perfect foresight *and*^[162] complete futures markets. The specification of the economy is given by i) individuals' preferences; ii) technology; iii) endowments of factors of production and of consumption goods and iv) the distribution of property rights shares –which includes the distribution of fixed capital. Allais does not provide however any formal proof on the existence of equilibrium and he limits himself to count the number of equations and unknowns to argue that the problem has a well-determinate solution. The equilibrium is assumed to come about through an auctioneer-guided tâtonnement (TEP: 484):

¹⁶² In the following section we address the reasons given by Allais to postulate the existence of both perfect foresight and complete markets.

Pendant toute cette évolution, il convient de supposer qu' aucune transaction effective n'est réalisée... Les échanges ne sont supposés s'effectuer qu'une fois réalisé l'équilibre général, c'est-à-dire lorsque se trouve déterminé un système de prix réalisant l'égalité de l'offre et de la demande sur les différents marchés aux différents instants. (TEP: 484, emphasis in the original)

Additionally, Allais argues that, as long as prices are seen as *present-value* or *discounted* prices, the equations that characterize the intertemporal economy are formally equivalent to those that describe the atemporal framework^[163]. The formal equivalence between the atemporal and the intertemporal economies would allegedly make of the IGE a more *general* framework to analyse the determinants of relative prices without significantly altering the analytical structure of the problem with respect to the atemporal setting.

2.2. Allais's misgivings about the intertemporal equilibrium I: the unrealistic assumptions of complete markets and finite life-span.

Despite the alleged generality of the IGE framework, Allais will show strong reservations on this equilibrium concept. But before discussing the reasons addressed by Allais, it is worth stressing that the French scholar assumes both perfect foresight *and* complete markets. The reason seems to be that Allais sees the hypothesis of complete futures markets as one possible way in which agent's correct foresight over the infinite future manifests itself. Allais writes:

Cette hypothèse [perfect foresight] nous a elle-même amené à considérer autant de marchés élémentaires distincts que de vies et services futurs. (TEP : 534)^[164]

¹⁶³ Allais observed that "Il est facile de voir ce que devient le système général dans le cas où toutes les opérations économiques sont supposées concentrées à un même instant. Le système auquel on arrive...correspond en fait aux conditions dans lesquelles s'étaient placés Walras et Pareto. Nous appellerons une telle économie 'économie non temporelle' par opposition à l'Économie générale que nous considérons, qui fait intervenir le temps et que nous appellerons 'Économie temporelle'. On voit que l'introduction de l'avenir et de procès de production échelonnés au cours du temps n'apporte aucune modification analytique essentielle au système général relatif à une économie, où toutes les opérations seraient concentrés dans le présent" (TEP : 519) And he added in a footnote that the equivalence between the atemporal and the temporal economies would hold "A condition de faire correspondre aux prix de l'économie non temporelle les prix actuels de l'économie temporelle" (TEP : 519, fn. 14)

¹⁶⁴ While currently in the theory of general equilibrium it is usually assumed either perfect foresight or complete markets, Allais's claim that the former hypothesis is implied by the latter seems to be correct. Consider for instance that in the further future there is an event- e.g. an invention, that allows some commodities to be produced with less quantities of inputs- that was not foreseen when intertemporal equilibrium prices were established, i.e. at the beginning of the first interval of the economy's life-span. In this state of the world, quantities brought to the market will generally differ from the quantities that would have been brought had the invention been foreseen. This in turn implies that, had transactions taken place at the set of prices effectively established at beginning of the first period, the quantities of some commodities will remain unsold or be in excess demand. Equilibrium prices and quantities established in the

But Allais immediately affirms with respect to the assumption of complete futures markets:

Un telle représentation n'est pas conforme à la réalité; en effet, dans l'Économie réelle, il n'y pas de marché particulier pour un quelconque disponible á terme. Seul, s'offre et se demande un bien abstrait, le capital monétaire, évaluation en valeur des biens matériels. Il en résulte que l'aspect réel des phénomènes est différent de celui de notre Économie pure (TEP : 534)

Allais's passage is clear: future markets *do not* exist; individuals allocate resources over time *only* through the capital market, i.e. by supplying and demanding an abstract commodity that he calls money capital. The assumption of complete future markets is eventually relaxed and replaced by an “*Economie à biens abstraits*” (*id.*: 492), allegedly to obtain a second, closer approximation to reality (*id.*: 481). Under this setting, complete future markets are missing, but agents are able to correctly forecast the (expected) evolution of future prices; the missing markets are replaced by a loan market, i.e. the market for “*capital abstrait*” (*id.*: 486, 492). This alternative representation of the economy can be thus interpreted as a temporary equilibrium which, given the assumptions of *perfect foresight* and *finite-horizon economy*, is still formally represented as an intertemporal equilibrium^[165]. It is clear however that the hypothesis of perfect foresight is still in need of justification. We shall return to this issue in section 2.3 below.

In regards to the *finite-horizon* hypothesis, Allais recognizes that “La conception d' une vie économique limitée à une période \bar{T} donnée est naturellement irréaliste” (*id.*: 535). He nonetheless justifies this assumption due to difficulties “d'ordre purement mathématique” (*id.*) that would arise in an

initial period will thus not reflect the compatibility of plans of the individuals. Alternatively, if at a generic period t a new commodity is brought to the market and the discovery of this commodity was not foreseen when equilibrium prices were effectively established, by definition the equilibrium price and the quantity sold of that commodity could not have been established at the beginning of the first period. When the commodity comes to the market in period t , it will have no market price and consumers will not be able to buy it -assuming this commodity is desirable by them. But then, given that individuals would be willing to buy the commodity in question and are unable to do it, the conditions of equilibrium initially established cannot reflect their maximizing plans. It is thus seems that the assumption of complete future markets implicitly carries with it the assumption of perfect foresight. The claim is also extensible to contingent markets where individuals must correctly foresee all the possible contingent events that may take place at each instant of time.

¹⁶⁵ Petri (2004: 197-202) and Fratini and Levrero (2011) have convincingly argued however that the analogy between temporary equilibrium with perfect foresight and intertemporal equilibrium is not correct. *Cf.* also Mandler (1999).

infinite-horizon setting. These difficulties, Allais argues, are related to the fact that, when the horizon is infinite, it is also infinite the number of variables and equations to deal with. This issue in turn, makes the analysis “très délicate” (*id.*: 536). However, Allais explains in a footnote that

Nous verrons d’ ailleurs prochainement comment des problèmes relatifs à des intervalles infinis peuvent être résolus de manière rigoureuse. (TEP: 536, fn.3)

Allais subsequently refers the reader to the very-short discussion of stationary equilibria that takes place in section F of TEP (pp. 665-669) to find the alleged solution to the “problèmes relatifs à des intervalles infinis”, a section that is a very brief introduction to the topics much more thoroughly developed later in EI. In that subsequent book in fact, Allais definitely abandons the finite-horizon hypothesis and provides the earliest formalization of an overlapping generations (OLG) model, but under stationary conditions. We must notice in this connection that, as e.g. Malinvaud (1986: 113, cf. also 1961: 150) points out in his review of Allais’s OLG model, the assumption of stationariness has the advantage that there is no need to define “in advance” (*id.*) the terminal conditions regarding the appropriate volume and composition of the capital stock. These terminal conditions, Malinvaud subsequently notices, seem “unsatisfactory, since only knowledge of the ensuing activity would determine what terminal capital would be suitable” (*id.*). The issue, in my view correctly addressed by Malinvaud is that, if one realistically assumes that economic activity *does not* finish after the last period (say T) of an Arrow-Debreu economy, one must consider that there will be capital goods left after T. But since there is no information of how conditions -i.e. preferences, endowments of factors and technology- will evolve in T+1, T+2, etc., the structure of the capital stock that is left in the last period cannot but be arbitrary, it would seem, because the usefulness of that stock in the production process can only be checked in the periods that follow T, which by assumption are not considered by the model. On the other hand, within a stationary economy, there is no last period –or alternatively, the last period coincides with the first one- and hence the capital stock left in any period t is determined by the economic conditions that will prevail in $t+1$, which are in turn known due to the stationary character of the economy.

2.3. Allais's misgivings about the intertemporal equilibrium II: the problematic assumption of perfect foresight.

It is, however, to Allais's attempt to justify the assumption of correct future foresight that we must now turn to better grasp the French author's reasons to defend the stationary state as a more plausible setting than the IGE framework to represent actual economic conditions. As a first reason to justify the assumption of perfect foresight, we find Allais arguing that it is a necessary assumption

par la nécessité d' éliminer du phénomène de l'intérêt l' élément risque qui n'est pas susceptible d'une représentation simple. (TEP: 534)

Why such a *simple* representation does not exist? The answer is given in EI. Allais notices that "Lé négociant qui fait venir à Marseille une cargaison de bananes la reçoit en général dans les délais prévus et la vend aux prix prévus" (EI : 34). Therefore, to accurately represent merchants' production decisions, one should proceed to model *the way* in which these agents form their expectations. Allais thus explains with respect to the perfect foresight assumption that

Une telle approximation en rendant exact tout calcul économique engageant *l'avenir*, en éliminant le risque et par là même *des éléments arbitraires difficilement représentables*, nous permettra de donner une représentation simple des mécanismes économiques essentielles qui font intervenir le temps ; elle rendra ainsi possible une détermination précise du taux de l'intérêt. (EI: 33-34, emphasis added)

The assumption of perfect foresight, so Allais stresses, avoids the recourse to "*arbitrary* elements" when modelling agents' expectation functions; these elements, no doubt, would condemn the theory to indefiniteness. However, Allais (*id.*) readily admits that correct foresight over the infinite future is certainly a "très abstraite" hypothesis. And when in TEP he comes to indicate the plausibility that the perfect foresight intertemporal path describes actual paths with sufficient approximation, we find him acknowledging that:

Nous verrons que dans la réalité toutes les fois que la prévision est instable, l'évolution de l'économie réelle l'est également. La question de la prévision constitue en fait un des points où les phénomènes réelles peuvent notablement différer de ceux de l'économie pure. (TEP : 485, fn. 6)

Allais accepts here that as long as forecasts about future conditions turn out to be incorrect, the actual evolution of the economy tends to *diverge* from the trend predicted by the intertemporal equilibrium path. “En effet”, Allais argues:

[c]e n'est que dans la mesure même ou la prévision est parfaite... que la stabilité de l'équilibre démontée pour l'évolution spéculative vaut également pour l'économie réelle. (TEP : 485)

where by “évolution speculative” Allais means the instantaneous adjustment process formalized through the tâtonnement (*cf.* TEP: 484). So, as Allais's argument goes, it is *only* when foresight is perfect that the stability results reached through an instantaneous adjustment process can be legitimately extended to the actual economy too. But if the correspondence between theory and observation must be argued on the basis of the highly unrealistic assumption of perfect foresight, the usefulness of the IGE would be seriously impaired, it would seem. One must indeed realistically acknowledge that individuals do not have perfect foresight when taking their production and consumption decisions; if any, they reach equilibrium prices and quantities by a trial-and-error process that may take considerable time to assert itself. Allais is in fact well aware of this; for instance, he explains that individuals are unable to solve “numerically” the equations of the equilibrium; granting stability, equilibrium prices and quantities are reached only after successive market rounds, through trial and error and experimentation:

En fait, nous sommes incapables... de résoudre numériquement ces équations en nombre immense... Dans la réalité, les individus et les entreprises font *des essais* et *les répètent* jusqu'à ce qu'ils portent leurs satisfactions ou leurs revenus aux maxima compatibles avec les conditions générales de la structure économique; par la même, ils résolvent, *par approximations successives*, le système général des équations de l'équilibre. (TEP: 531, emphasis added)^[166]

We have already seen (section 1.2) that Allais is particularly clear that, given that in actual economies the reaching of equilibrium takes time, the plausibility of the equilibrium requires that its data are sufficiently persistent relative to the

¹⁶⁶ It is worth noting that Walras's influence of this passage is particularly clear. In the first chapter we have shown in fact that Walras distinguishes between the “mathematical” and the “empirical” solutions of the equilibrium, and argues that individuals are unable to solve the first one; they only ‘solve’ the empirical solution, i.e. they reach equilibrium through trial and error and experimentation after sufficient time.

process of equilibration. He adds in this connection that the equilibrium position will approximately indicate actual economic conditions as long as

[l]es vitesses des modifications non prévues de la structure économique sont suffisamment faibles. Lorsqu'une telle condition est réalisée, la référence à l'équilibre théorique a un sens et elle doit être effectuée. (TEP : 531)

It is therefore clear that Allais is certainly aware that in actual economies there is no such thing as perfect foresight (notice how he explicitly admits that there are changes in the economic structure that are “non expected”); granting stability, equilibrium is reached as a result of *successive* interactions of the individuals that trade in markets, and that will generally involve disequilibrium transactions and productions. Allais (TEP: 485, fn. 7) openly admits for instance that in real economies “à tout instant des échanges s’effectuent aux prix instantanés”, i.e. at disequilibrium prices, *cf.* section 1.2). In other words, Allais admits that equilibrium prices and quantities are *discovered* by individuals through a trial-and-error process that usually takes a long period to assert itself fully; the implication is that the correspondence between theory and observation *cannot* be plausibly argued on the basis of perfect foresight, namely, by simply assuming without any justification that individuals can correctly forecast the equilibrium quantities that must be brought to the market, and their respective prices.

So, the strong assumption of sufficiently correct foresight in equilibrium must be justified. The reader may suspect at this juncture why the assumption that the economy is stationary becomes now particularly relevant for Allais. For in a stationary economy considered in its traditional, *static*, sense, the relative persistence of the data reasonably justifies correct foresight as an *outcome* of the adjustment process. And in fact, immediately after the French author acknowledges that “[c]e n’est que dans la mesure même ou la prévision est parfaite, que la stabilité... etc.” (TEP: 485), we find him writing:

Il en est approximativement ainsi lorsque l'économie réelle se présente sous l'aspect d'un régime *sensiblement permanent*. (TEP: 485)

Essentially the same claim is put forward in EI (117-1178): Allais explains that entrepreneurs can only take their production and investment decisions on the basis of *expected* relative prices and profit rates. And he goes on to argue:

Comme des prévisions efficaces ne sont possibles que dans des conditions économiques et sociales *relativement stables*, on aperçoit ici quelles importance peut avoir pour l'esprit d'entreprise et la prospérité économique la stabilité de ces conditions. (EI : 117-118)

And with respect to those “relatively stable” conditions, he adds in this connection:

Les régimes permanents peuvent être considérés comme constituant une première approximation de l'économie réelle, dans les périodes de stabilité économique tout au moins. En effet, dans la plupart des secteurs et pour des périodes pouvant comprendre plusieurs années, les consommations et les productions restent sensiblement constantes et de même nature. (EI: 35 ; TEP: 665)^[167]

Allais convincingly explains here that the assumption of correct foresight can be plausibly justified if the underlying conditions remain *fairly* stationary over relatively long periods of time. If these conditions change relatively slowly, the reaching of equilibrium prices and quantities will eventually occur not because individuals had been ‘gifted’, so to speak, with perfect foresight, but because they have eventually learnt those fairly correct –i.e. equilibrium- prices and quantities along the process of equilibration. To put it in a nutshell, when the data have the necessary persistence the fact that individuals will correctly foresee market prices comes out as a *result*, and not as a *presupposition*, of the process of equilibration itself. Notice in fact that Allais speaks here of conditions that are not invariable but rather “fairly” stationary, implying therefore only persistent, and not invariable, data; Allais therefore seems to be speaking of the traditional notion of *static* –rather than the secular- stationary state^[168]. It is finally worth noticing that the plausibility of the stationary assumption is stressed by Allais himself, who argues that in the “majority” of sectors production and consumption conditions remain reasonably constant for rather long periods of time. He adds in this connection that, over sufficient time, if actual conditions change slowly, an economy in its “normal operation” will reach a “relatively stationary” situation:

¹⁶⁷ Exactly the same passage appears both in TEP and EI.

¹⁶⁸ However, as we shall notice in section 3.2. when Allais comes to formalize the conditions of stationary equilibrium in EI, he actually models a secular equilibrium state.

En fait, cette induction paraît confirmée par l'observation, qui semble bien montrer que lorsque les conditions de structure varient *peu*, l'économie dans sa marche normale tend à prendre un régime *sensiblement* permanent. (TEP: 669, emphasis added)

To conclude the following observation is worth making: a brief excursion to EI seems to bring further support to the claim that Allais firmly believes that the assumption of relatively constant conditions allows identifying the persistent forces at work in actual economies. Allais (EI: 478) for instance argues that “l'expérience montrant que le taux d'intérêt est, et a toujours été, positive...il reste à expliquer les raisons profondes de cette permanence” (*id*). And he goes on to argue that these reasons must be found by assuming that fairly constant conditions prevail, because only under conditions that remain essentially permanent over rather long periods it is possible to assert that the positivity of the rate of interest is not explained by purely contingent events that will vanish with time. He writes:

Or l'existence constante d'un taux d'intérêt nominal toujours positif constitue une permanence dont l'explication doit être recherchée, non à partir de probabilités essentiellement contingentes, mais à partir de données de structure également permanentes. (EI: 479)^[169]

The discussion of this second section can be briefly summarized in the following way: to begin with, Allais believes that the unrealistic assumptions of complete markets and of finite time-span horizon seriously weaken the plausibility that the IGE has a correspondence with observation. The notion of stationary equilibrium on the other hand, so Allais' argument goes, does not need relying on these highly implausible and yet indispensable assumptions; and this gives initial reasons that in part explain why Allais envisages the stationary state as a more plausible equilibrium notion than the IGE to represent actual economic conditions. The content of this last sub-section has strengthened this claim: for Allais is particularly clear that agents do not have perfect foresight when taking their production and consumption decisions, which implies that equilibrium magnitudes are only *discovered* through a trial-and-error process that can take protracted periods to assert itself, and that will generally entail disequilibrium productions and transactions. The implication is

¹⁶⁹ Similar contentions are to be found for instance in Wicksell, who argued that “The real theoretical difficulty is ... to explain how, under stationary conditions, the possession of capital can remain a permanent source of income. The application to non-stationary conditions offers no difficulty in principle”. (Wicksell, 1954: 154)

that the assumption that individuals eventually come to approximately foresee the quantities to be produced in equilibrium -and their respective prices- can be plausibly justified *only if* the underlying conditions remain essentially constant over sufficient intervals of time, as when e.g. the economy is assumed to be stationary in a static sense. We have finally seen that the plausibility of this hypothesis is defended by Allais himself by reasonably arguing that in actual economies the conditions of production and consumption in the different industrial sectors remain *fairly* steady for rather long periods of time. All this seems to explain why, during the very-brief excursion to the notion of stationary equilibrium, Allais already suggests the path that will be subsequently taken in EI when he argues:

La considération des régimes permanents est particulièrement utile dans l'étude de l'intérêt. (TEP: 665)

We shall return to this issue in the third section below, specifically devoted to examining Allais explanation of the determinants of the rate of interest in terms of supply and demand forces, but under stationary conditions, as it is provided in EI.

2.4. Allais's treatment of the vectorial endowment of capital goods.

I wish to conclude this second part by addressing the following point: although when Allais comes to determine the IGE he formally includes the vectorial endowment of capital goods as part of the data of the equilibrium, he does seem to be clear that an equilibrium conceived as a position that the economy tends to realise forcedly needs treating these endowments as endogenous variables, i.e. determined by the conditions of the equilibrium. Consider for instance the following passages:

À un instant donné de la dynamique du déséquilibre la valeur d'un bien est déterminée par la valeur actuelle de ses services futurs. Ce n'est qu'à l'équilibre que la loi du coût de production est réalisée. Cette dernière loi n'est donc qu' une *loi de tendance* (TEP: 266-267)

Suivant la terminologie que nous avons adoptée la loi des valeurs actuelles apparait ainsi comme une loi primaire en ce sens qu'elle est toujours vérifiée qu'il y ait équilibre ou déséquilibre ; la loi du coût moyen apparait, au contraire, comme une loi secondaire que ne peut être vérifiée de manière rigoureuse qu'à l'équilibre. En fait, le prix d'un bien, toujours égal à la valeur actuelle de ses revenus présents et futurs, n'est pas déterminé par le coût de production, mais le jeu du mécanisme économique l'amène à coïncider avec lui, lorsque l'équilibre est atteint. (EI: 69-70)

A l'équilibre, en effet, la valeur de tout capital matériel est égale à son coût de production. Naturellement, cette égalité n'a lieu qu'à l'équilibre. Ce n'est donc qu'une loi tendancielle de l'économie réelle. (EI: 79)

It is here readily seen that Allais *explicitly* defines equilibrium as a situation where *all* existing capital goods yield a uniform return on their supply price, and he remarks that this condition operates only over sufficient time, i.e. as a “tendencial law”. At any a given moment, say, in the initial period of an IGE, the demand prices of the capital goods inherited so to speak, are determined by the discounted value of their expected yields (equations G of the Annexe I H, pp. 846). The equality between selling prices and the discounted value of the future yields, Allais continues, is however unable to determine an equilibrium, because it may well be the case that, for some capital goods, their demand price falls short of their supply price. The determinants of the demand price are, in Allais's words, “primary causes” and their explanation is the realm of what Allais calls “primary laws” (*cf.* the discussion of section 1). Incidentally, given that Allais is well aware that the primary causes only exert a transitory effect on prices, and that, as discussed in the first section, at every instant of time any sort of events can influence market (disequilibrium) prices, it seems hardly possible that he actually believes that the law of present-value-prices is “*toujours vérifiée*”, as he argued; in my opinion the claim must be understood as an attempt to very roughly indicate one of the possible determinants of market prices in the very short run. But anyway, we have already indicated that Allais sees these very-short-run causes as hardly important, and only the durable causes matter for determining equilibrium prices. Because these persistent causes or forces operate over sufficient time only, the price=cost condition of the equilibrium is therefore an outcome that is expected to emerge as a “tendency”, as Allais forcefully admits.

Accordingly, equilibrium requires that the *form* and *quantity* of the capital goods must be *endogenously-determined* variables by the condition that the market price of the capital goods that are actually reproduced in equilibrium must be equal to its minimum-average cost; moreover, the moment one admits that the rentals of the capital goods are endogenous variables that may be altered by changing the quantities of the existing capital goods, the price-equal-

minimum-unit-cost condition implies a uniform return on the supply prices of the capital goods. The previous considerations imply that it is certainly evident that, when determining the conditions of a “*proper*” equilibrium -as Allais occasionally calls the equilibrium where all capital goods yield the same return on their supply price- the vector of capital goods cannot be taken as given; they must be an outcome of the process of equilibration.

That a plausible, realistic adjustment process must treat the endowments of the physically heterogeneous capital goods as endogenous variables is further admitted by Allais in the fifth chapter of TEP, where the problem of the determinants of real wages in the long-run is discussed. Allais there explains that the long-lasting effects of an increase in the rate of real wages can take a long period of time to assert itself fully. He writes:

L'étude des effets durables d'une élévation des salaires réels qui précède a supposé implicitement que les différents facteurs de production et en particulier les travailleurs pouvaient passer sans difficulté d'une entreprise à l'autre. Or, de tels changements impliquent des modifications considérables de l'activité des individus, qui dans les hypothèses les plus *favorables*, ne peuvent s'effectuer que lentement. (TEP : 699, emphasis added)

One cannot but agree with Allais that the long-run effects of a rise in the rate of real wages cannot but manifest themselves “*lentement*”, even “under the most favourable hypotheses”; but this implies that the capital stock will be modified during this adjustment process. Allais in fact seems to be well aware of this: consider that he argues that, when real wages rise, profit-maximizing firms will economize in labour by utilizing “capital sous la forme de machines, d'outillage, d'installations diverses” (TEP: 694). However, he subsequently wonders:

Toutefois, les instruments de production, par lesquels les entreprises remplacent les travailleurs, nécessitent également du travail pour leur fabrication, de sorte que les combinaisons nouvelles adoptées par les entreprises, en augmentant la demande des capitaux de production, augmentent, par là même, la demande de travail dans tous les secteurs produisant l'outillage, les machines et les installations de toute sorte. En fait, quand un entrepreneur commande une machine, il économise du travail chez lui, mais il en crée chez confrère le constructeur. On peut alors se demander si l'effet secondaire d'une telle évolution n'est pas un simple déplacement de main-d'œuvre des industries de consommation vers les industries de production et si, par suite, une élévations de salaires réels n'est pas possible sans que se produise aucune restriction de l'emploi. (TEP: 694)

Given that the capital goods that are to substitute labour are not initially ‘there’, so to speak, when conditions change, but must *still* be produced during the

process of adjustment, Allais wonders whether it is plausible to argue that the secondary, long run outcome of the rise in real wages simply is a *redistribution* of the already-employed working force in the different industries, while leaving the aggregate level of employment unchanged. The answer, Allais (TEP: 695) explains, is that this possibility must be excluded because the negative real-wage elasticity of the labour demand curve (derived from the decreasing marginal productivity of labour) implies that an increase in real wages must decrease labour employment.

In any case, the point I wish to make is that Allais explicitly acknowledges here that, during the adjustment process, the quantities and forms of the initially-existing capital goods will *necessarily* change, i.e. they will be endogenous variables determined by the new equilibrium conditions. An important implication of this discussion is that, albeit implicitly, Allais must have been envisaging the endowments of capital goods as somehow crystallizations of a single factor, which cannot but be measured in value terms. Note in this connection that in the above-quoted passage (TEP: 694), Allais speaks of the rise in real wages as inducing firms to increase their demand for “capitaux de production”, hence implicitly treating these capital goods as embodiments of a homogenous factor of production.

Further confirmation that Allais does treat the single endowments of capital goods as embodiments of a single factor measured in value terms is to be found in the author’s explanation of the long-run trends in income distribution. The marginal product of labour, Allais asserts, is “d’autant plus grande que les quantités de capital et de sol, avec lesquelles le travail peut être combiné, son plus grandes” (TEP: 693); and he notes in this connection:

on aperçoit ainsi pourquoi le niveau des salaires est élevé dans un pays, où le travail est rare en *comparaison du capital* et du sol. (TEP : 693, emphasis added)

where, no doubt, here capital is measured as an amount of value. And anticipating some of the conclusions reached in EI, Allais goes on to argue that

pour un valeur suffisamment grande du capital collectif Γ , le taux d'intérêt servi s'annule de sort que... la part relative du revenu collectif qui le rémunère constitue une fonction décroissante de ce paramètre. (TEP: 693).

Since there is no a priori reason to suppose that the quantities of the single capital goods will *all* fall –or rise- in the same proportion when the rate of interest falls –in general, some capital goods will increase in quantity, some of them will fall, and their price in terms of the chosen numéraire may change in any direction- it is clear that in his explanation of the long-run trends in income distribution Allais actually treats the single capital goods as embodiments of a single quantity of capital in value terms, which is assumed to be in a negative relation with the rate of interest. Further, even more explicit, indications that in his explanation of income distribution Allais in fact treats the single capital goods as endogenous variables, somehow embodiments of a single factor measured in value terms, will be given in the next section, where the long-run determinants of the rate of interest in Allais's theory are discussed.

3. CAPITAL AS A HOMOGENEOUS MARGNITUDE IN EI.

This section examines Allais's explanation of the long-lasting causes of income distribution as developed in EI, where he adopts a stationary notion of equilibrium for that purpose; in fact, for the reasons addressed in section 2, Allais envisages this equilibrium concept as a more plausible framework than the IGE to explain the persistent factors that determine the interest rate in actual economies. EI is composed of two volumes; however, here I will be mainly concerned with the first five chapters of volume I, where the “secondary” or persistent forces that determine the rate of interest are thoroughly discussed^[170]. This final section aims to show that Allais's explanation of income distribution takes for granted that techniques can be ordered according to their degree of roundaboutness *independently* of the rate of interest, and hence the indefensible notion of capital as a single factor in its demand-side role. Given that Allais relies on the notion of Average Period of Production, this section will also allow me to discuss in some detail the limits of this concept to justify the negative inclination of the demand for capital with respect to the rate

¹⁷⁰ In the rest of volume I, Allais defines the optimal conditions of capital accumulation and incorporates money into the analysis. In volume II he criticises the earlier theories of capital and interest and takes up again the issue of the relationship between the monetary and the rate of interest that is determined in the capital market.

of interest; it will be shown in this connection that Allais does not take these limitations into account. This discussion of the negative interest elasticity of investment demand in EI will also allow me to point out the relationship between the demand for capital as a stock and the demand for capital as a flow in Allais's work, a relationship that is largely implicit in traditional neoclassical theory. Finally, the last section shows that, while Allais is aware of the problem of capital from its supply side, he avoids discussing the thorough implications of this problem by determining a secular, rather than a static, stationary state, whose usefulness to assess a correspondence with observation is highly doubtful, as already argued in the first chapter (section 4).

3.1. The demand for capital in stationary equilibrium.

Allais formalizes the long-lasting influences (or 'durable', as he occasionally calls them) of the interest rate in a stationary equilibrium by developing, for the first time in the history of economic analysis, an overlapping-generations model, whose formal features are presented in APPENDIX A to this chapter. Essentially, Allais envisages the rate of interest as being determined by the intersection between a negatively sloped investment demand, and an inverted U-shaped supply of savings, the intersection of which determines a stable equilibrium.

Let us analyse the determinants of the demand for capital. Like the bulk of the authors examined in this dissertation, Allais closely follows the Austrian approach of Böhm Bawerk and Wicksell; capital goods are therefore conceived as the product of 'past' labour and land, which are the only original factors of production. At a given instant of time, Allais (EI: 103-104) explains, these original factors are distributed in the different stages of production. This distribution, he continues, is constantly changing due to accumulation: for a given state of technology and for given quantities of original factors, a higher proportion of the available resources tends to be utilized in the earlier stages of production so as to increase the quantity of consumption goods. He writes:

Le progrès économique, c'est à dire l'augmentation à techniques constants de la production en quantité et en qualité pour des quantités données des facteurs primaires de production, est attribuable, en grande partie, au fait qu'une proportion toujours plus grande des ressources disponibles est consacrée aux premiers stades de la production. De

nouveaux stades sont ajoutés ou intercalés, de sorte que l'enchaînement vertical de la production se trouve allongé. En d'autres termes, les méthodes de production deviennent plus indirectes, plus médiates et plus 'capitalistiques', en ce sens que la production de biens de consommation exige, par unité, de plus gros capitaux, c'est-à-dire plus d'articles intermédiaires, machines, matières premières ou produits semi-finis. (EI : 104)

And in a footnote he explicitly remarks that

Les articles intermédiaires et les biens de consommation son mesures ici *en valeur* (EI: 104, fn. 5, emphasis added)

According to Allais then, the quantity of capital per unit of output increases with the adoption of more capitalistic methods of production. However, capital goods are generally technique-specific, and this implies that the adoption of new, cost-minimizing methods of production will generally call for a change in the *form* and quantity of the capital goods that cooperate with the given amounts of labour and land. Allais therefore acknowledges that it is the *value* of the capital goods per unit of output that increases with the adoption of more capitalistic methods of production, i.e., the capital goods that are to be used under the more capitalistic techniques are argued to embody, so to speak, more 'capital' than those goods used under the less capitalistic methods. In other words, Allais seems to envisage the capital goods that are produced and reproduced in equilibrium under cost-minimizing techniques as *crystallizations* of the single homogenous factor measured in value terms (note in this connection that these quotations further strengthen the results reached in section 2.4, where I happened to notice that, albeit implicitly, in TEP Allais already treats the endowments of capital goods as endogenous variables).

Now, if "economic progress"^[171] is the effect of the adoption of more roundabout methods of production, it is the role of savings, i.e. capital accumulation, to induce profit-maximizing firms to adopt of more capitalistic techniques.

Le rôle de l'épargne consiste à distraire constamment de la production de services immédiatement consommable une partie des facteurs de production don dispose l'économie dans son ensemble et à les consacrer à des travaux, construction d'habitations, production de machines ou d'outils, qui, pour n'être pas immédiatement consommables, n'en doivent pas moins apporter à la collectivité, à terme plus ou moins éloigné, in supplément de production et, par conséquent, de bien-être. L'épargne implique ainsi à

¹⁷¹ Although the work of Walras is not quoted in EI, it is highly likely that Allais borrows the terms technical progress and economic progress from him (Cf. Walras, 1954: part VII)

chaque instant un déplacement des facteurs de production par rapport à l'emploi qui leur aurait été fixé, si elle n'avait pas été réalisée. (EI: 146)

Notice additionally that here Allais explains that the adoption of more capitalistic methods needs the factors of production to be transferred among industries and changes in the kinds of capital goods produced, and he adds in this connection:

Par ailleurs, l'accumulation du capital et l'emploi de procédés de production plus détournés qui en résulte nécessite des déplacements importants de facteurs de production de certaines industries à d'autres. Elle nécessite également des changements profonds dans les techniques employées. (EI : 151)

And then in a footnote, Allais remarks:

Par exemple un abaissement sensible du taux de l'intérêt en abaissant le prix de la houille blanche, entraînerait un remplacement considérable de la houille noire par la houille blanche (EI : 151, fn, 6)

So, in this passage it is explicitly argued that, during the adjustment process, the form of the capital goods employed will generally change with the adoption of the new methods – in his example, the extraction of black carbon is substituted by white carbon as the interest rate decreases- and hence the capital goods that will be produced in the new stationary equilibrium cannot but be a *result* of the adjustment process. As I remarked before then, one must conclude, since Allais admits that the form of the capital goods changes with techniques, and techniques become more capitalistic when the interest rate decreases, that he must have seen the several capital goods as embodiments or crystallizations of the substance capital, the value magnitude.

Now, the reason why new savings allegedly induce the adoption of more capitalistic methods is that, when in an otherwise stationary economy the increase in savings causes a decrease in the rate of interest (EI: 150), the quantity of capital embodied in the newly adopted cost-minimizing technique must increase, i.e. the demand for capital as a stock per unit of labour is seen as a decreasing function of the rate of the interest. In order to further explain why this is the case, Allais considers the following example: suppose that only one consumption good A is produced as net product by the aid of labour (L) and land (B). There are several available techniques that can be used to produce A, and each of them is characterized by the distribution of the total quantity of L

and B in the different stages of production. Suppose a particular process is adopted. In equilibrium, Allais (EI: 119) notes, V , the total value of A (price times quantity produced), is given by its total cost of production, i.e.

$$V = \sum_{\rho=0}^n (1+I)^\rho \Delta_{-\rho} \quad (1)$$

where, $\Delta_{-\rho} = wL_{-\rho} + rB_{-\rho}$ is the total value of wages ($wL_{-\rho}$) and rents ($rB_{-\rho}$) paid in period $-\rho$, i.e. ρ periods before the consumption good comes to the market (w and r stand for the rate of money wages and rents respectively, while $L_{-\rho}$ and $B_{-\rho}$ represent the quantity of labour and land used in period $-\rho$). Given that no commodity is basic, the value of the consumption good can be expressed in terms of past rents and wages in a finite number (n) of steps. According to Allais (EI: 119), it is “convenient” to characterize the production process by its Average Period of Production, Θ , i.e. “l’*éloignement moyen pondéré dans le temps de la fourniture des facteurs primaires de production*” (id. 119). This average period, Allais continues, “represente le temps moyen qu’il faut attendre après la fourniture des facteurs primaires de production pour obtenir le produit définitif” (id.). Formally, he defines the average period in the following way:

$$\Theta = \frac{\sum_{\rho=0}^n \rho \Delta_{-\rho}}{\sum_{\rho=0}^n \Delta_{-\rho}} \quad (2)$$

Allais names the denominator of the expression in the right hand side of (2) as “original revenue”, $R_{N\omega}$ (“*revenue originnaire*”), while he calls the numerator of that expression as “original value of capital employed”, $C_{\mu\omega}$ (“*valeur originnaire du capital employé*”). $R_{N\omega}$ thus represents the total value of wages and rents paid in the production process on the different stages. $C_{\mu\omega}$, on the other hand, is the value of the capital employed under the assumption that the ruling rate of interest is infinitely small (it goes to zero in the limit). In order to derive this result, Allais (EI: 128) argues that, in equilibrium, the value of the net product V must be equal to the sum of total wages and rents paid, $R_{N\omega}$, plus the value of accumulated interest on the quantity of “capital employed” (*capital mobilier*), C_μ , that is $V = R_{\mu\omega} + iC_\mu$. Therefore,

$$C_{\mu} = \frac{V - R_{N\omega}}{i} \quad (3)$$

Given that $R_{N\omega} = \sum_{\rho=0}^n \Delta_{-\rho}$, then replacing (2) and into (3) we get,

$$C_{\mu} = \sum_{\rho=0}^n \left[\frac{(1+i)^{\rho} - 1}{i} \right] \Delta_{-\rho} \quad (4)$$

When the interest rate i goes to zero, equation (4) reduces to^[172]

$$C_{\mu} = \sum_{\rho=0}^n \rho \Delta_{-\rho} = C_{\mu\omega} \quad (5)$$

that is, the numerator of (2). However, the notion of “capital originaire” can be given a different interpretation. If prices are computed under the rule of *simple interest*, V in equation (1) turns out to be equal to:

$$V = (wL_0 + rB_0) + (wL_{-1} + rB_{-1})(1+i) + (wL_{-2} + rB_{-2})(1+2i) + (wL_{-3} + rB_{-3})(1+3i) + \dots + (wL_{-n} + rB_{-n})(1+ni) \quad (6)$$

By simple manipulations, it can be shown that equation (6) is equivalent to:

$$V = \sum_{\rho=0}^n (wL_{-\rho} + rB_{-\rho}) + i \sum_{\rho=0}^n \rho (wL_{-\rho} + rB_{-\rho}) \quad (7)$$

Equation (7) then divides the total value of the net output, V , between the total amount of rents and wages paid (first term) and the amount of accumulated interest on capital (second term). But note that in this case, the total quantity of capital directly coincides with $C_{\mu\omega}$ in equation (5). Then, $C_{\mu\omega}$ can be interpreted as the quantity of capital of the economy under the assumption that prices are calculated under the rule of *simple interest*^[173]. Incidentally, as I note in section 3.1.1 below, the rule of simple interest is one of the untenable and yet

¹⁷² $\lim_{i \rightarrow 0} \frac{(1+i)^{\rho} - 1}{i} = \lim_{i \rightarrow 0} \rho(1+i)^{\rho-1} = \rho$

¹⁷³ Cf. Ahmad (1991: 155-156) for a similar interpretation of the numerator of (2).

indispensable requisites that the average period must satisfy to be a consistent measure of the capital intensity of the techniques under use.

In sum, when prices are computed under the rule of simple interest, Allais's average period can be thus seen as the ratio between the value of capital employed, $C_{\mu\omega}$, and the original revenue, $R_{N\omega}$. Equation (2) can thus be expressed as:

$$C_{\mu\omega} = \Theta R_{N\omega} \quad (8)$$

Relying on equation (2) (or its analogous (8)), Allais argues:

Il est visible que dans un régime permanent la valeur originaire du capital employé dans un procès donné est d'autant plus grande que le période Θ est plus élevée. On peut donc considérer la période Θ comme caractéristique du degré d'emploi du capital. (EI: 119)

In other words, there is a positive relationship between Θ and $C_{\mu\omega}$, and hence the former can be taken as a proxy of the latter. The reason, Allais explains, is that:

Comme d'une manière générale un facteur de production est d'autant moins employé qu'il est plus cher et que le prix d'un facteur de production (X_{-q}) fourni à l'instant t_{-q} à un prix x_{-q} , est égal à $x_{-q}(1+i)^q$ à l'instant t_0 , on voit que le facteur (X_{-q}) sera d'autant moins employé pour la production (A_0) que le taux d'intérêt i sera plus élevé. Il en résulte que, si le taux d'intérêt est élevé, les facteurs de production éloignés seront relativement peu employés par rapport aux facteurs de production rapprochés et inversement. *On voit ainsi que le période de production et par suite le degré d'emploi du capital seront d'autant moins grande que le taux d'intérêt est plus élevé.* (EI : 119-120, emphasis added)

The logic behind this argument is essentially Wicksellian: an increase in the rate of interest decreases the cost of late units of inputs X relative to the cost of early units, and this allegedly induces firms to adopt techniques that use the former more intensively than the latter. This process decreases the average period of production and as a result, the "degree of capital employment", i.e. the demand for capital, measured by $C_{\mu\omega}$. Schematically we have:

$$\uparrow i \Rightarrow \downarrow \Theta \Rightarrow \downarrow C_{\mu\omega}$$

Given that the negative relationship between $C_{\mu\omega}$ and i is justified on essentially the same grounds as in Hayek (1941) (*cf.* chapter 2: section 2.2), the proof that this reasoning is not generally valid is left to APPENDIX B to this chapter.

3.1.1. Some qualifications on Allais's average period of production.

At this juncture, some qualifications with respect to Allais's average period of production may be worth pointing out. The first qualification is that the average period as originally developed by Böhm Bawerk (1891) and Wicksell (1893) is an attempt at finding a technical measure of the capital intensity of production techniques. Because the substitution mechanisms that are at the core of neoclassical theory imply that the quantity employed of each factor of production decreases with their rental price, their claim, which is in fact echoed by Allais, is that the higher the rate of interest, the lower the average period, i.e. the lower the quantity of capital embodied in the cost-minimizing techniques. But in order to assert this relationship, Böhm Bawerk and Wicksell were well aware that the average period must be a technical measure *independent* of distribution, i.e. of the rate of interest. Otherwise, the average period would change with pure changes in distribution. But Allais's Θ does not comply with this indispensable requisite. Let us first re-express (2) as

$$\frac{\sum_{\rho=0}^n \rho \Delta_{-\rho}}{\sum_{\rho=0}^n \Delta_{-\rho}} = \frac{\sum_{\rho=0}^n w \rho L_{-\rho} + \sum_{\rho=0}^n r \rho B_{-\rho}}{\sum_{\rho=0}^n (w L_{-\rho} + r B_{-\rho})} \quad (9)$$

If following Böhm Bawerk and Wicksell we define the average periods of labour (Θ_1) and land (Θ_2) as

$$\Theta_1 = \frac{w \sum_{\rho=0}^n \rho L_{-\rho}}{w \sum_{\rho=0}^n L_{-\rho}} \quad (10)$$

$$\Theta_2 = \frac{r \sum_{\rho=0}^n \rho B_{-\rho}}{r \sum_{\rho=0}^n B_{-\rho}}$$

(note incidentally that these average periods *are* independent of distribution because the rate of money wages (rents) in Θ_1 (Θ_2) appear both in the numerator and denominator and hence they cancel each other out); then equation (9) becomes:

$$\Theta = \Theta_1 \frac{\sum_{\rho=0}^n wL_{-\rho}}{\sum_{\rho=0}^n (wL_{-\rho} + rB_{-\rho})} + \Theta_2 \frac{\sum_{\rho=0}^n rB_{-\rho}}{\sum_{\rho=0}^n (wL_{-\rho} + rB_{-\rho})} \quad (11)$$

Equation (11) shows that Allais's period of production is an average of the respective average periods of labour and land, weighted by their share in total income. It is clear then that, unlike the average periods determined by equation (8), Allais's Θ *does* depend on the current rate of money wages and rents, and hence it can not be taken as a technical measure *independent* of distribution, unless we assume $\Theta_1 = \Theta_2$. Only in this case we obtain that

$$\Theta = \Theta_i \quad i = 1, 2 \quad (12)$$

And given that

$$\Theta_i = \frac{\sum_{\rho=0}^n \rho L_{-\rho}}{\sum_{\rho=0}^n L_{-\rho}} \quad i = 1, 2 \quad (13)$$

Θ becomes independent of distribution as well. The assumption of equal average periods, no doubt, is highly restrictive. This problem does not emerge in Böhm Bawerk's analysis because he in fact assumes that labour is the only non-produced means of production. But Wicksell (1893), who wants to extend Böhm Bawerk's framework by including land as a second non-produced factor, in order to avoid this problem is forced to assume that the average periods of labour and land are the same. As advanced in the previous section, it is also worth noting that besides one productive factor (say labour) Böhm Bawerk's and Wicksell's average periods assume that prices are calculated under the rule of *simple* interest. It is under these two highly restrictive, and yet indispensable, assumptions that Θ can measure the degree of capital intensity of the techniques that are used in production, and these limitations may explain why e.g. Wicksell (1934) abandons this concept in his *Lectures*^[174].

¹⁷⁴ Under the assumption of only one non-produced factor (labour) and simple interest, equation (7) becomes $V = \sum_{\rho=0}^n (wL_{-\rho}) + i \sum_{\rho=0}^n \rho (wL_{-\rho})$, and introducing equation (8), we obtain,

In sum, the deficiencies of the notion of average period used by Allais to measure the capital intensity of the techniques can be thus grouped in two different sets. On the one hand, when for instance the average periods of labour and land differ, equation (2) will change when nothing but distribution changes (*cf.* Lutz 1967: 310-312 and Garegnani 1960: 130-134). This will also be the case if one considers only one non produced input, but prices are (correctly) computed under compound interest ^[175]^[176]. On the other hand, even if we consider the case of only one factor of production (say labour) and define the average period as in equation (10), but we still compute prices under compound interest as Allais does in equation (1), the average period will not necessarily contra-vary with the rate of interest (given that this is a well known fact, the proof is left to APPENDIX C to this chapter, which shows why this may be the case by means of Samuelson's (1966) example of reswitching).

3.1.2. The connexion between the demand for capital as a stock and the demand for capital as a flow in stationary equilibrium.

I wish to conclude section 3.1. by quoting a passage of EI where Allais explains the working of the capital market, and shows how equilibrium is to be re-established when in an otherwise stationary economy, the sudden rise in the level of savings causes the interest rate to decrease. The quotation will give me

$V = wL(1 + i\Theta)$, with $L = \sum_{\rho=0}^n L_{-\rho}$. Suppose now that there are two techniques to produce the net product, α and β . The value of the net product under technique $j = \alpha, \beta$ is $V_j = wL_j(1 + i\Theta_j)$. Let us assume that $\Theta_\alpha < \Theta_\beta$. The relative cost of the net product is given by $V_\alpha / V_\beta = L_\alpha(1 + i\Theta_\alpha) / L_\beta(1 + i\Theta_\beta)$. Then,

$\text{sign} \left[\frac{\partial(V_\alpha / V_\beta)}{\partial i} \right] = L_\alpha L_\beta (\Theta_\alpha - \Theta_\beta) < 0$. As the interest rate increases (decreases), the technique with the lower (higher) average period, α (β), becomes cost minimizing, hence more profitable.

¹⁷⁵ In this case, the average period is defined as $\tilde{\Theta} = \frac{\sum_{\rho=0}^n \rho L_{-\rho} (1+i)^\rho}{\sum_{\rho=0}^n L_{-\rho} (1+i)^\rho}$ (Cf. Ahmad, 1991: 158). It is

clear that, like equation (2) above, this measure will also change when nothing but the rate of interest (i) changes.

¹⁷⁶ We have already noticed this problem of the average period in our excursion to Hicks's work (chapter 4).

the opportunity to discuss the relation between the demand for capital as a stock, and the demand as a flow (investment) in EI. Allais observes:

S'il n'y avait par exemple qu'une production, la production de chaussures, une épargne supplémentaire cette année ferait qu'au prix de l'année dernière la production achevée au début de cette année ne pourrait entièrement s'écouler. Les producteurs seraient ainsi amenés à des pertes et a priori on imagine difficilement qu'ils puissent, dans de telles conditions, procéder à de nouveaux investissements.

Toutefois et dans la réalité chaque industriel a ses projets particuliers et devant, d'une part, les difficultés du marché, et d'autre part, la baisse du taux de l'intérêt, il sera normalement amené à rechercher, dans un abaissement de son coût de production grâce à l'amélioration de son équipement que peut permettre l'abaissement du taux de l'intérêt, la solution des difficultés de vente qui l'assaillent. On conçoit donc que la demande de capitaux neufs puisse non seulement se maintenir, mais même se développer.

D'ailleurs, il n'y a pas une seule production, mais un très grand nombre qui seront inégalement atteintes par la diminution du pouvoir d'achat consacré à la consommation. Pour certains d'entre elles, la diminution d'activité sera insensible et seul jouera l'abaissement du taux de l'intérêt, qui constituera dès lors un motif déterminant de rénovation de l'équipement suivant des procès de production plus détournés et plus productifs. (EI : 150-151)

Notice then that the increased level of savings (“épargne supplémentaire”) is argued to cause two main effects: it decreases aggregate consumption demand (i.e. the demand for shoes), and pushes the interest rate downwards. While the first effect tends to decrease investment, the second effect, so Allais argues, will tend to increase it, since the lower rate will allegedly induce firms to adopt new cost-minimizing, more capital-intensive production techniques. In other words, as soon as the “*capital detenú*” takes a “free form”, firms will *renew* their equipment under techniques that employ more capital per unit of output. In many industries, Allais argues, only the second effect will be operative, the implication being that in the aggregate the higher level of savings will be eventually absorbed by a corresponding higher investment demand.

In any case, the point I wish to make is that this passage seems to indicate how Allais envisages the connection between the demand for capital as a stock and the demand for capital as a flow, i.e. investment, a relation that, as argued in the first chapter, is only implicitly present in traditional marginalist authors. Indeed, Allais observes that it is on the *new equipment*, i.e. the equipment that must be renewed in every productive cycle due to wear and tear, that the influence of the lower interest rate will immediately assert itself. The complete adjustment of the capital stock to the new conditions, as Allais will subsequently note, may take protracted periods due to the “important déplacements des

facteurs de production de certains industries à d'autres" (EI: 151). However, if prices and the wage rate adjust without appreciable delay to the new lower rate of interest, it seems plausible to assume that the labour force and land that have been freed this year –i.e. the portion of the total supply of labour and land that has been cooperating with the part of the capital stock that has now taken a “free form”- will be employed under the new methods of production which, due to the higher level of savings, will now be more capital-intensive than before. If conditions do not change, the capital stock that still has the old forms –and hence yields residual quasi-rents- will be gradually replaced under the new physical forms that are needed to implement the new cost-minimizing techniques. Note then that, because initially only new investments adopt the more-capital-intensive, cost-minimizing techniques, while the capital goods in the existing plants still keep their original forms, the rate of interest must necessarily be determined in the market for capital in free form, i.e. where as Petri (2004: 128) argues, “the demand for (the flow of) ‘free’ capital met its supply (also a flow)”.

This last point is explicitly discussed by Allais (EI: 144): he argues that, given that the total demand for capital as a stock includes “*des capitaux anciens*” (*id.*) which cannot be turned back to consumption in the period because they “correspond à des biens immobilisés” (*id.*), the relevant demand schedule to be considered when explaining the forces that determine the interest rate is the demand for capital that corresponds to the *new investments*, that is, the demand for capital as a flow.

Au lieu de considérer la demande globale de capital, on peut ne faire intervenir que cette demande diminuée de la valeur des capitaux anciens, c'est-à-dire la valeur des investissements nouveaux. (EI : 144)

Similar considerations are provided for the total supply of capital, i.e. only the supply of free capital (as a flow) must be taken into account in the explanation of the interest rate. Allais finally notes:

Le point d'intersection des deux courbes ainsi obtenues correspond naturellement à la même valeur du taux d'intérêt. (EI: 144)

That is to say, the rate of interest determined by the intersection between the supply of and demand for capital considered as flows must be the same as the interest rate that results from the intersection between the supply and demand for capital as stocks. This last observation is correct –of course, taking for granted that the premises of neoclassical theory are sound- as long as the analysis is restricted to conditions of stationariness (not necessarily secular though), because in stationary equilibrium the demand for capital as a flow is a reduced-scale representation of the demand for capital as stock. Outside stationary equilibrium however, the rate of interest that is determined in the savings-investment market will not necessarily *reflect* the equilibrium conditions between the *total* demand for and supply of capital because the capital stock that is still invested in the old plants has not yet adjusted to the conditions of the new equilibrium. But besides this qualification, the last two above-quoted passages seem to confirm that according to Allais, the negative interest elasticity of the demand for capital as a flow, i.e. investment, is derived from the demand for capital as a stock, which in turn decreases with the interest rate because of the traditional mechanisms of substitution among factors of production: labour, land and capital, the single factor measured in value terms.

3.2. The supply of capital in value terms and the stationary state.

There is a last issue that I wish to discuss: Allais seems to be aware that the supply of capital cannot be measured independently of distribution. He writes:

De même, une des plus grandes difficultés de la théorie de l'intérêt provient du fait que la grandeur 'capital' n'est pas une grandeur physique comme une quantité de blé ou de vin, mais une grandeur économique dont la valeur dépend directement de celle du taux de l'intérêt. Cette difficulté n'est pas été généralement clairement aperçue. Les classiques avaient pour habitude de dire que le taux de l'intérêt est déterminé par l'intersection des courbes d'offre et de demande de capital, ce qui est exact, mais en omettant de préciser que ces courbes d'offre et de demande dépendaient elles-mêmes directement du taux de l'intérêt, leur théorie présentait une singulière lacune. (EI: 517)

The passage shows that Allais is sufficiently clear about what we have called the “supply side problem of capital”. This problem, we have argued (chapter 1, section 3), is enough to demolish the neoclassical explanation of prices and distribution in terms of supply and demand forces, because once it is admitted that the quantity of capital in the economy cannot be determined before the interest rate is known, the equations of the equilibrium are *undetermined*.

However, Allais does not seem to envisage the issue relative to the value measurement of capital as a seriously disturbing problem for neoclassical price and distribution theory, and I wish to suggest that at the bottom there is the following reason: in EI, when Allais comes to formalize the OLG model (Annexe II, pp. 641-771) under stationary conditions, and that is argued by Allais to represent actual economic phenomena on the basis that *fairly* –but not strictly-stationary conditions prevail among sectors, Allais does not take the quantity of capital as *given*, as the traditional, static, neoclassical concept of stationary state requires; as I show in APPENDIX A, in Allais’s OLG model this magnitude is a *resultant* of the adjustment process. Little wonder that we find Allais writing:

Le taux d’intérêt réel qui s’établit en régime permanent est tel que le capital que désirent détenir les individus soit précisément égal au capital total employé par l’économie. (EI: 54)

Allais thus determines a *secular* equilibrium state, i.e. a position such that the quantity of capital is such that induces individuals to make zero net savings. The deficiencies of the notion of secular equilibrium to represent actual economic conditions were addressed in the first chapter (section 4). However, it seems useful to briefly summarize them, considering in particular that the notion of static equilibrium no longer appears (at least explicitly) in current economic discussions, and hence the difference between this concept and the notion of secular equilibrium has been lost. First, while the assumption that individuals do not want to make positive savings could be defended as a plausible abstraction to study very-long-run or secular trends, in real economies *positive* rates of accumulation are typically observed; if, therefore, Allais’s claim that “fairly stationary conditions prevail in most sectors” (TEP: 665; EI: 35) actually meant that in real economies accumulation is close to coming to a halt, the claim loses plausibility and is liable to objection. Moreover, the secular equilibrium determined in Allais’s OLG model is based on contradictory hypotheses: while he assumes that capital accumulation has come to a halt, the data relative to preferences, population and technical knowledge are the same as those determining a static equilibrium. In a truly secular equilibrium the quantity of labour should be also endogenously

determined. And the assumption of *given* preferences and technical knowledge must also be removed. However, it seems impossible to predict at a sufficient level of generality those preferences or technical knowledge ruling in the further future, i.e. when capital accumulation and population growth would have come to a halt. And even if this could be somehow ascertained, the connection between secular equilibrium conditions and actual economic conditions would be completely lost.

On the other hand, the secular equilibrium *is not persistent* in the precise sense of persistency, that is, a speed of change of the data of a lower order of magnitude than the speed of convergence towards equilibrium, because the speed of convergence of the endogenous variables towards a stationary position where there is no further incentive to accumulation is so slow that it is, at most, of the same order of magnitude than the speed of change of the determinants of the equilibrium (preferences, technical knowledge, population); so, as Allais himself argued, when the condition of persistency is not satisfied, “les valeurs des paramètres économiques ne sauraient être considérées, même en première approximation, comme les solutions des équations générales de l’équilibre” (TEP: 549). In light of these considerations, Allais’s solution to the problem of capital from its supply side must be rejected because, even granting stability, it violates the requisite of persistence, a concept that Allais himself carefully discusses and, as seen in section 1, he considers essential for an equilibrium notion to be capable of having the role of a centre of gravitation of actually observed magnitudes.

4. CONCLUDING REMARKS.

In my perusal of TEP and EI I have attempted to argue that, although Allais provides the earliest formalization of an IGE, he puts forward harsh critiques on this notion and he ends up by abandoning it when developing his theory of capital and interest^[177]. While I have subsequently shown that his formalization of the stationary equilibrium is not satisfactory because, in order to avoid the

¹⁷⁷ Incidentally, it might be interesting to notice that in a well-known article, Malinvaud (1953) considered that, historically, there have been two main different approaches to capital and interest: the Austrian and the Walrasian, and he subsequently (id: 265) argued that Allais’s work must be included in the second one, somehow neglecting the content of EI, a book that anyway is cited in Malinvaud’s article.

problem relative to the specification of the endowment of capital in value terms Allais develops a secular stationary state, I do share Allais's criticisms of IGE theory.

And indeed, his uneasiness with the IGE increased with the years, and in Allais (1971) he will strengthen the tone of his critiques against IGE theory, especially as it has been later developed by Debreu (1959). Among other things, he will object to Debreu (1959) i) that "the concept of savings and investment disappears completely from the analysis" (Allais, 1971: 146); ii) the hypothesis of complete future markets, because "the assumption that as many goods exist as associated events has, in general has no justification whatever in reality" (*id*:148); iii) that "the number of production units cannot be assumed to be given [since] it is an unknown which must be determined by the general conditions of equilibrium" (*id*: 156); and finally iv) he will harshly criticise the no-trade-out of equilibrium "fairy tale" because "outside equilibrium, there is not a single set of prices for all operators, but a series of sets of prices specific for each exchange transaction" (*id*: 161) and hence "the exchanges leading to equilibrium take place successively at different prices and that, at any given moment, the price sets used by different operators are not necessarily the same." (*id*.)

I generally agree with Allais, I only wish to add that the *unrealism* of the assumptions behind IGE is nothing but a symptom of the illness of the theory. Traditional neoclassical economics did not have recourse to these unrealistic hypotheses, but it had to pay a high price instead: the need to specify the factor capital in value terms. Allais's concerns therefore are simply an attempt to state that something is wrong with contemporaneous formulations of neoclassical theory. While the economists critical of neoclassical economics have been insisting on these problems for a long time, the fact that these critiques actually come from one of the "*fathers*", so to speak, of IGE, may contribute to strengthen their importance. I close then by quoting Paul Samuelson (1983, as quoted in Grandmont, 1989: 27), who argued that "Had Allais's earliest writings been in English, a generation of economic theory would have taken a different course".

APPENDIX A: THE QUANTITY OF CAPITAL IN ALLAIS'S OLG MODEL.

This appendix aims to show that the equations of Allais's OLG model (EI: Annexe II) determine a *secular* stationary state.

A1. General setting.

- In each period, there are $2n$ consumers (n young; n old).
- Each consumer lives for two periods.
- The aggregate endowment of labour is equal to X . Each young consumer is endowed with X/n units of labour that are supplied inelastically at the wage rate x . Old people do not work.
- Stationary conditions prevail.

A2. Consumption side.

- All consumers have identical preferences, represented by a logarithmic additive utility function, which depends on the consumption of good A in the first period (A_0) and in the second period (A_1).
- Consumer's demands for A_0 and A_1 are derived from the following maximization problem:

$$\max : U(A_0, A_1) = \ln(A_0) + \alpha \ln(A_1) \quad s.t. : R^i = \bar{a}A_0 + \frac{\bar{a}}{1+i} A_1 \quad (a.1)$$

Where \bar{a} stands for the price of good A , i for the rate of interest, $R^i \equiv x \frac{X}{n}$ for the income of the single individual and $\alpha \in (0,1)$ represents individuals' preference for present consumption.

A3. Production side.

- There are two productive sectors. Sector 1 produces the consumption good (A) and sector 2 the capital good (H)
- A is produced by labour and the circulating capital good, under a twice differentiable production function, homogeneous of degree one: $\bar{A} = f(X_A, H)$, where \bar{A} is the supply of consumption goods and X_A and

H respectively represent the quantity of labour, of the capital good used in the production of A^[178].

- The capital good is produced with the aid of labour only, under a twice differentiable homogeneous of degree one production function $\bar{H} = g(X_H)$, where \bar{H} stands for the supply of new capital goods and X_H is the quantity of labour used in the production of the capital good.

A4. Equilibrium equations.

The equilibrium conditions of the problem are given by the following equations:

$$A_0 = \frac{R}{\bar{a}(1+\alpha)} \quad (\text{A.1}) \quad \bar{H} = g(X_H) \quad (\text{A.7})$$

$$A_1 = \frac{R}{\bar{a}(1+\alpha)} \alpha(1+i) \quad (\text{A.2}) \quad \bar{h}\bar{H}'_x = x \quad (\text{A.8})$$

$$A_0 + A_1 = A = \frac{R}{\bar{a}(1+\alpha)} [1 + \alpha(1+i)] \quad (\text{A.3}) \quad 1+i = \frac{h}{\bar{h}} \quad (\text{A.9})$$

$$\bar{A} = f(X_A, H) \quad (\text{A.4}) \quad X_A + X_H = X \quad (\text{A.10})$$

$$\bar{a}\bar{A}'_x = x \quad (\text{A.5}) \quad H = \bar{H} \quad (\text{A.11})$$

$$\bar{a}\bar{A}'_H = h \quad (\text{A.6}) \quad A = \bar{A} \quad (\text{A.12})$$

Equations (A.1)-(A.2) stand for the aggregate demand for the consumption good of the young (A_0) and the old (A_1) respectively, and $R = \sum_{i=1}^n R^i = xX$ stands for the aggregate income of the young; equation (A.3) is the total demand for the consumption good A. Equations (A.4) and (A.7) respectively represent the supplies of the consumption good (\bar{A}) and of the new capital goods produced (\bar{H}); equations (A.5) and (A.6) respectively equalize, in the consumption-good sector, the wage rate (x) with the value marginal product of labour ($\bar{a}\bar{A}'_x$) (\bar{a} being the selling price of A), the rental price of the capital good (h) with its

¹⁷⁸ In some variants of the OLG model, Allais includes land as a second, non-produced factor of production. And Allais considers different possibilities for the distribution of land property rights, e.g. a case where land is equally distributed to the old generation, and then sold to the young, or a case in which land is publicly owned, and land rents are equally distributed to the young or, alternatively, to the old. Given the limited purposes of this appendix, all this is omitted here since the inclusion of land will not alter the main results, i.e. that Allais's equations determine a secular equilibrium. Allais also considers a variant of the model where there is one more variable to be considered, the amount of debt issued by the Government, which I also neglect.

value marginal product ($\bar{a}\bar{A}'_H$); equation (A.8) equalizes, in the capital good industry, the wage rate with the marginal product of labour used in the capital-good industry ($\bar{h}\bar{H}'_X$); equation (A.9) establishes that, in equilibrium, the rate of interest (i) must be equal to the ratio of the rental price of the circulating capital goods (h) and its selling price (\bar{h}); equations (A.10)-(A.12) establish the market clearing conditions in the labour market, in the capital-good market and in the consumption-good market respectively. Equations (A.1)-(A.12) thus define a system in 12 equations^[179] to determine 13 unknowns:

$$\left\{ \begin{array}{l} \text{prices : } \{ \bar{a}, h, \bar{h}, x, i \} \\ \text{quantities : } \{ A_o, A_1, A, H, X_A, X_H, \bar{A}, \bar{H} \} \end{array} \right.$$

We thus determine relative prices by setting $\bar{a} = 1$. In Allais's model, the equations of equilibrium are numbered by (4)-(5)-(9)-(21)-(22)-(23), 2 equations (24), equations (25)-(28) and 2 equations (31), which respectively correspond to my equations (A.4)-(A.7)-(A.10)-(A.1)-(A.2)-(A.3)-(A.5)-(A.6)-(A.8)-(A.9)-(A.11)-(A.12).

It is to be noticed that the assumption of homogeneity of degree 1 of the production functions $f(\cdot)$ and $g(\cdot)$ allows us to derive the selling price=cost conditions. For instance, we know that $\bar{A} = \bar{A}'_{X_A} X_A + \bar{A}'_H H$; hence, by multiplying equation (A.5) by X_A , equation (A.6) by H and adding we obtain:

$$\bar{a}\bar{A} = xX_A + hH \tag{A.13}$$

Analogously, given that $\bar{H} = \bar{H}'_{X_H} X_H$, by multiplying equation (A.8) by X_H we obtain

$$\bar{h}\bar{H} = xX_H \tag{A.14}$$

¹⁷⁹ Of them only 11 are independent, since (A3) is equal to (A1)+(A2). We thus have 11 independent equations in 12 unknowns since once A_o and A_1 are determined A is determined as well.

A5. The stationary assumption and the quantity of capital.

Note first that conditions (A.4) and (A.11) already show that the economy is stationary because the demand for the endowments of capital goods (H) that are used as inputs in the consumption good sector, and hence already produced in the previous period, is equal to the new capital goods that are being currently produced, \bar{H} . In order to shed some further light on this issue we could replace equation (A.11) by three equations (A.11') and two additional unknowns, H^E and H^I , that would explicitly represent, respectively, the endowment of existing capital goods and the investment demand for new capital goods.

$$\begin{cases} H^E = H \\ H^I = H^E \\ H^I = \bar{H} \end{cases} \quad (\text{A.11}')$$

The first of equations (A.11') equalizes the endowment of existing capital goods with their demand in the consumption-good sector; the second equation explicitly establishes stationariness, since the demand for new capital goods (H^I) must be equal to the existing endowments; finally, the last equation establishes the market clearing condition for the production of *new* capital goods. Of course, the set of equations (A.11') can be reduced to (A.11); their usefulness is that they make explicit the stationary character of the economy. These conditions however, are also compatible with the traditional notion of *static* stationary state.

In what follows I will follow Fratini (2007: section V): not only do I want to show that in the set of equations (A.1)-(A.12) there is implicit a *savings-investment* market, hence a demand for capital in *free form*, but more importantly, that in Allais's model the endowment of capital in value terms is not *ex ante* given but it is a *resultant* of the adjustment process; i.e. it determines a *secular* equilibrium state. In Allais's model however, there is a further aspect needing for discussion. Because only one capital good is produced, it is still possible to determine a static stationary equilibrium –where, *trivially*, the return on the supply prices of the new capital goods will be the same- without having recourse to a value specification of the factor capital: the

physical endowment of the unique capital good could be taken as given and specified in its *own* technical units, so it is not immediately explicit why Allais does not have recourse to the static stationary equilibrium. In order to shed further light on this issue, in the next section I extend Allais' model and consider that the consumption good is produced by labour and two capital goods.

Note first that the supply of savings is only implicitly determined because the budget constraint of the individual is written in its intertemporal form. Aggregate savings (S) are thus determined by the difference between aggregate income in the first period (R) and the aggregate value of the first period consumption (A_0). That is, $S = R - \bar{a}A_0$. Therefore:

$$S = R \frac{\alpha}{(1+\alpha)} \quad (\text{A.15})$$

Let us now introduce equation (A.9) into (A.13) and add equation (A.14). Recall that equilibrium implies: $X_A + X_H = X$ and that $H = \bar{H}$. So, we have:

$$\bar{a}\bar{A} + \bar{h}\bar{H} = xX + (1+i)\bar{h}\bar{H} \quad (\text{A.16})$$

Equation (A.16) says that in equilibrium, gross production, GP (left hand side), is equal to the full-employment level of gross income, GI (right hand side). Now, from the individuals' budget constraint of each period, we know that the full-employment level of gross income is devoted to consumption demand –both of the young and the old- and gross savings.

$$\text{GI} = \bar{a}A_0 + \bar{a}A_1 + S \quad (\text{A.17})$$

This means that,

$$\bar{a}\bar{A} + \bar{h}\bar{H} = \bar{a}A_0 + \bar{a}A_1 + S \quad (\text{A.18})$$

Therefore, recalling that aggregate demand, A , is equal to $A_0 + A_1$, we have that

$$\bar{a}(\bar{A} - A) = S - \bar{h}\bar{H} \quad (\text{A.19})$$

Equation (A.19) implies that, in equilibrium, full-employment gross savings are equal to the gross production of capital goods, which is equal to investment demand, H . Moreover, because of (A.19), equation (A.12) is satisfied *if and only if* the condition

$$S = \bar{h}\bar{H} \quad (\text{A.20})$$

is satisfied. We can thus substitute condition (A.20) for condition (A.12) *without* altering the significance of the system^[180]. It is to be noticed moreover that the equilibrium condition $S = \bar{h}\bar{H}$ determines a secular stationary equilibrium because the quantity of capital in value terms is *endogenously* determined by condition (A.20), which implies zero net accumulation in equilibrium. To determine a *static* stationary equilibrium on the other hand, we can replace the value of gross savings by an *exogenously* given quantity of capital, that is, we change equation (A.20) to

$$K^* = \bar{h}\bar{H} \quad (\text{A.20}')$$

where K^* stands for the given endowment of capital in value terms^[181]. Under this new specification, net savings are *assumed* to be zero whatever prices and distribution^[182]. Let me insist that the assumption of zero net savings is a legitimate approximation due to the slowness of capital accumulation.

A7. The case of two capital goods.

As I have remarked above, the case considered by Allais is rather pathological because, given the stringent assumption that there is only one capital good, it is *still* possible to determine a static equilibrium without having recourse to a value magnitude of the capital endowment, because the

¹⁸⁰ Incidentally, once the savings-investment equation is made explicit, one may plausibly conjecture that the same kind of instabilities and multiple equilibria due to reswitching that have undermined traditional theory are present in Allais's model as well. In fact, Fratini (2007: sections VI and VII) shows the possibility of reswitching and multiple equilibria in an OLG model under secular stationary equilibria.

¹⁸¹ Although we are replacing a stock (K^*) for a flow, gross savings, given that all capital is circulating capital both magnitudes coincide.

¹⁸² As to the distribution of the capital endowment, we may for instance assume that each young individual is endowed with a fraction K^*/n .

endowment of the capital good (H^E) can be taken as given and yet the equilibrium equations will be compatible with a static stationary state. Suppose in fact that in equations (A.11') H^E is no longer an unknown but is given. We would lose one variable. However, once the rental price (h) and the selling price of the capital good (\bar{h}) are given, condition (A9) will be immediately satisfied, and since there is only one *kind* of capital good, then all existing capital goods will yield the same return on their supply prices without any need to specify the capital endowment in value terms (we may drop then equation A.20').

In order to avoid this issue, which arises because of the special assumptions of Allais's model, I consider now that the consumption good is produced by labour, land and two capital goods, H and M . As before, each capital good is produced by labour as input. The new equilibrium equations are:

$$\begin{array}{ll}
A_0 = \frac{R}{\bar{a}(1+\alpha)} & \text{(A.1)} \\
A_1 = \frac{R}{\bar{a}(1+\alpha)} \alpha(1+i) & \text{(A.2)} \\
A_0 + A_1 = A = \frac{R}{\bar{a}(1+\alpha)} [1 + \alpha(1+i)] & \text{(A.3)} \\
\bar{A} = f(X_A, H, M) & \text{(A.4')} \\
\bar{a}\bar{A}'_x = x & \text{(A.5)} \\
\bar{a}\bar{A}'_H = h & \text{(A.6)} \\
\bar{H} = g(X_H) & \text{(A.7)} \\
\bar{h}\bar{H}'_x = x & \text{(A.8)} \\
1+i = \frac{h}{\bar{h}} & \text{(A.9)} \\
X_A + X_H + X_M = X & \text{(A.10')} \\
H = \bar{H} & \text{(A.11)} \\
A = \bar{A} & \text{(A.12)} \\
\bar{M} = j(X_M) & \text{(A.21)} \\
\bar{a}\bar{A}'_M = m & \text{(A.22)} \\
\bar{m}\bar{M}'_x = x & \text{(A.23)} \\
1+i = \frac{m}{\bar{m}} & \text{(A.24)} \\
M = \bar{M} & \text{(A.25)}
\end{array}$$

while the unknowns are

$$\left\{ \begin{array}{l} \text{prices : } \{ \bar{a}, h, \bar{h}, x, i, \bar{m}, m \} \\ \text{quantities : } \{ A_0, A_1, A, H, M, X_A, X_H, X_M, \bar{A}, \bar{H}, \bar{M} \} \end{array} \right.$$

There are therefore five new equations (equations A.21-A.26) to determine five new unknowns $\{ \bar{m}, m, X_M, M, \bar{M} \}$ i.e., respectively, the selling price of the capital good M , the selling price of M , the quantity of labour used in the production of

M , the demand for the existing capital goods M and the current supply of capital goods type M . Equations (A.21)-(A.25) are self-explanatory.

The price=cost condition of the capital good M is derived by multiplying equations (A.23) by X_M .

$$\bar{m}\bar{M} = xX_M \quad (\text{A.26})$$

On the other hand, given that the consumption good A is now produced by labour and two capital goods, equation (A.13) is modified as follows:

$$\bar{a}\bar{A} = xX_A + h\bar{H} + m\bar{M} \quad (\text{A.13}')$$

The reasoning now is the same as before. We introduce (A.9) and (A.24) into (A.13'), and add (A.13') to (A.14) and to (A.26) to obtain the equality between gross production (GP) and the full-employment level of gross income (GI):

$$\bar{a}\bar{A} + \bar{h}\bar{H} + \bar{m}\bar{M} = xX + (1+i)[\bar{h}\bar{H} + \bar{m}\bar{M}] \quad (\text{A.16}')$$

And taking into account that GI is devoted to consumption demand and gross savings, this means that

$$\bar{a}\bar{A} + \bar{h}\bar{H} + \bar{m}\bar{M} = \bar{a}A_0 + \bar{a}A_1 + S \quad (\text{A.18}')$$

Therefore, we have that

$$\bar{a}(\bar{A} - A) = S - (\bar{h}\bar{H} + \bar{m}\bar{M}) \quad (\text{A.19}')$$

And hence,

$$S = \bar{h}\bar{H} + \bar{m}\bar{M} \quad (\text{A.20}'')$$

As before, we can thus substitute condition (A.20'') for condition (12) without altering the significance of the system. We have seen that conditions (A.20 or A.20'') determine a secular stationary equilibrium, because the quantity of

capital is endogenously determined by the condition of zero net savings. Now however, because there are two capital goods instead of one, in order to determine a static stationary equilibrium such that all capital goods yield the same rate of return on their supply price, we can no longer take the endowments of each capital good as given; there is no other alternative than to specify the endowment of capital in value terms (K^*), that is, we should replace (A.20'') for the condition:

$$K^* = \bar{h}\bar{H} + \bar{m}\bar{M} \quad (\text{A.20''})$$

APPENDIX B: THE DEMAND FOR “CAPITAL ORIGINNAIRE” AND THE RATE OF INTEREST.

This appendix shows that, contrarily to what Allais's believes, the value of $C_{\mu o}$ (i.e. capital demand) may be positively related with the rate of interest, i [¹⁸³]. Let me recall the gist of Allais's (EI: 119-120) argument: consider an Austrian framework, where the net product (A_o) is envisaged as the outcome of dated units of labour and land. The final cost of each unit of dated input X is equal to its rental price plus the addition of interest between rent payment and the moment when A_o comes to the market. An increase in the rate of interest decreases the cost of 'later' inputs relative to the cost of 'early' inputs, and this allegedly induces firms to substitute the later for the former. Allais asserts:

Comme d'une manière générale un facteur de production est d'autant moins employé qu'il est plus cher et que le prix d'un facteur de production (X_{-q}) fourni à l'instant t_{-q} à un prix x_{-q} , est égal à $x_{-q}(1+i)^q$ à l'instant t_o , on voit que le facteur (X_{-q}) sera d'autant moins employé pour la production (A_o) que le taux d'intérêt i sera plus élevé. Il en résulte que, si le taux d'intérêt est élevé, les facteurs de production éloignés seront relativement peu employés par rapport aux facteurs de production rapprochés et inversement. On voit ainsi que... le degré d'emploi du capital seront d'autant moins grande que le taux d'intérêt est plus élevé. (EI : 119-120)

In order to show why Allais's reasoning is not generally valid, consider the following example: there is only one consumption good, A_o , produced under a standard, twice-differentiable, constant-returns-to-scale, production function $f(\cdot)$ given by

¹⁸³ The gist of the argument is taken from Garegnani (1985). Essentially the same claim was put forward in my critique to Hayek (*cf.* chapter 2).

$$A_0 = f(L_0, L_{-1}, B_0, B_{-1}) \quad (\text{B.1})$$

L_m and B_m represent, respectively, the amount of labour and land employed in stage $m=0,-1$. That is, in period $t=-1$, L_{-1} and B_{-1} are employed in the production of the circulating capital goods that, one period later, cooperate with L_0 and B_0 to produce the final output A . In this case, the “original revenue” R_{N_0} , is given by:

$$\sum_{\rho=0}^1 \Delta_{-\rho} = [B_0 + B_{-1}]r + [L_0 + L_{-1}]w \quad (\text{B.2})$$

While, the demand for capital, $C_{\mu\omega}$, is given by

$$\sum_{\rho=0}^1 \rho \Delta_{-\rho} = B_{-1}r + L_{-1}w \quad (\text{B.3})$$

where r and w are, respectively, the period-0 rates of money wages and rents. Cost-minimizing techniques are derived by maximizing:

$$f(L_0, L_{-1}, B_0, B_{-1}) - (B_0r + L_0w) - (B_{-1}r_{-1} + L_{-1}w_{-1}) \quad (\text{B.4})$$

where the price of A_0 is set as the numéraire. The first order conditions of the problem are given by:

$$\begin{cases} f_{L_0} = w \\ f_{B_0} = r \\ f_{L_{-1}} = w_{-1} \\ f_{B_{-1}} = r_{-1} \end{cases} \quad (\text{B.5})$$

Where f_X with $X= L_0, B_0, L_{-1}, B_{-1}$ is the marginal product of factor X ; w_{-1} and r_{-1} stand, respectively, for the rates of money wages and rents paid to those factors employed in period -1. Equilibrium also requires that:

$$\begin{cases} w_{-1} = (1+i)w \\ r_{-1} = (1+i)r \end{cases} \quad (\text{B.6})$$

where i stands for the uniform rate of interest. And by simple manipulations of equations (B.5) and (B.6) we obtain:

$$\begin{cases} \frac{f_{L_{-1}}}{f_{L_0}} = \frac{w_{-1}}{w} = 1 + i \\ \frac{f_{B_{-1}}}{f_{B_0}} = \frac{r_{-1}}{r} = 1 + i \end{cases} \quad (\text{B.7})$$

Finally, the assumption of full employment of productive factors implies that:

$$\begin{cases} \bar{L} = L_{-1} + L_0 \\ \bar{B} = B_{-1} + B_0 \end{cases} \quad (\text{B.8})$$

Where \bar{L} and \bar{B} are, respectively, the given supplies of labour and land. Now, we have seen that, according to Allais, a decrease in the rate of interest induces entrepreneurs to adopt longer process of production because the rental prices of the factors used in the earlier stages decreases relative to the rentals of those factors used in the later stages. Then, his argument goes, the “degree du employ de capital” increases.

With the help of equations (B.7) and (B.8) we may now proceed to show why Allais’s claim that there is a negative relationship between the rate of interest and the employment of capital $C_{\mu\omega} = B_{-1}r + L_{-1}w$ is not generally valid. Notice first that a decrease in the rate of interest implies a decrease in the ratios $\frac{w_{-1}}{w}$ and $\frac{r_{-1}}{r}$; therefore, the quantities of factors L_{-1} , L_0 , B_{-1} and B_0 must vary so as to respect the decrease in these ratios. However, the validity of Allais’s argument presupposes that when the rate of interest decreases, the decrease, e.g. in $\frac{w_{-1}}{w}$ must be accompanied by an *increase* in L_{-1} and, due to equation (B.8), a *decrease* in L_0 ; that is to say, with an increase in the ratio $\frac{L_{-1}}{L_0}$. By the same reasoning, a decrease in $\frac{r_{-1}}{r}$ must take place with a corresponding increase in B_{-1} and a decrease in B_0 .

However, this reasoning would be valid only if the inputs included in the production function are two; when the inputs are more than two, the increase (decrease) in the ratio $\frac{w_{-1}}{w}$ may well take place with a increase (decrease) in $\frac{L_{-1}}{L_0}$. The reason is that, in the case of more than two inputs, the increase in the employment of one input can well *decrease* the marginal product of the other inputs (the only restriction is that the marginal product of at least one of the remaining inputs must increase). So, suppose e.g. that L_{-1} increases. Owing to the principle of decreasing marginal productivity, $f_{L_{-1}}$, and hence w_{-1} , must decrease too. However, if $f_{L_{-1}L_0} < 0$, it can be the case that the decrease in w_{-1} is accompanied by a more than proportional decrease in the marginal product of L_0 , and hence in w . This effect need not be counterbalanced by the subsequent increase in w that will take place when, owing to the restriction imposed by equation (B.8), L_0 decreases. So, in this case, the increase in $\frac{L_{-1}}{L_0}$ would take place with an increase in $\frac{w_{-1}}{w}$, and hence in the interest rate, i . Analogously, the ratio $\frac{B_{-1}}{B_0}$ can be in a direct relation with i too. The implication is that the decrease in the rate of interest requires a decrease in $\frac{w_{-1}}{w}$ and in $\frac{r_{-1}}{r}$; however, L_{-1} , B_{-1} , r and w can well all *decrease*. So, given that capital demand is: $C_{\mu\omega} = B_{-1}r + L_{-1}w$, it can well happen that when i decreases, $C_{\mu\omega}$ also decreases.

APPENDIX C: THE AVERAGE PERIOD OF PRODUCTION UNDER COMPOUND INTEREST.

This appendix shows that, when prices are calculated under the rule of compound interest, the average period does not necessarily contra-varies with the interest rate. I show this by means of Samuelson's (1966) example of reswitching. I recall the example for the convenience of the reader. According to the example, there are two techniques, α and β to produce champagne. Under technique α , 7 units of labour are used in the period $\rho = -1$ to produce 1 unit of

brandy. In period $\rho=0$, brandy ferments by itself and produces 1 unit of champagne. Under technique β , 2 units of labour produce 1 unit of grape-juice in period $\rho=-2$. In period $\rho=-1$ grape-juice ripens by itself and produces 1 unit of wine. Finally, in period $\rho=0$, 6 units of labour co-operate with the unit of wine to produce 1 unit of champagne. Setting the price of champagne as numéraire, the price equations under technique α are

$$\begin{cases} p_{brandy} = 7w \\ 1 = p_{brandy} (1+i) \end{cases} \quad (\text{C.1})$$

where w and i stand for the wage and the interest rates respectively; under technique β , cost-of-production relative prices are determined by the following equations:

$$\begin{cases} p_{grape} = 2w \\ p_{wine} = p_{grape} (1+i) \\ 1 = p_{wine} (1+i) + 6w \end{cases} \quad (\text{C.2})$$

We can now proceed to derive, for each technique, an equation relating the wage and the interest rates. Under technique α , this relation is given by,

$$w = \frac{1}{7(1+i)} \quad (\text{C.3})$$

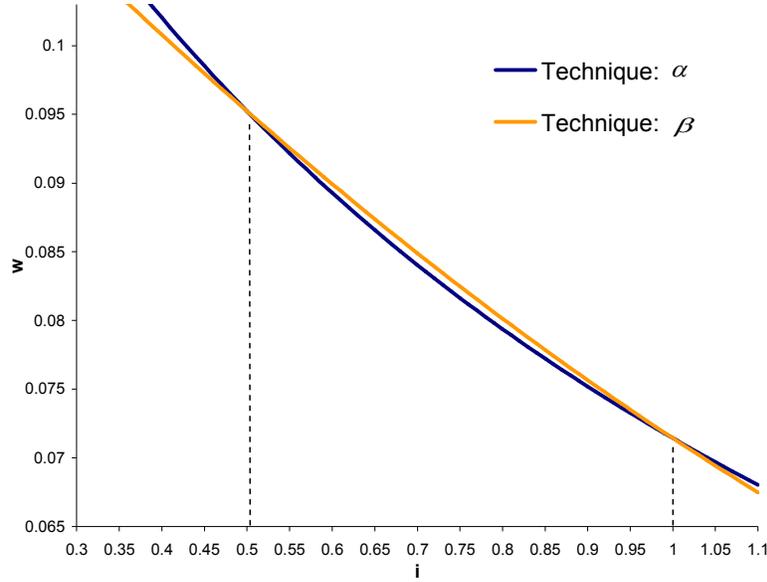
while under technique β , we get,

$$w = \frac{1}{6 + 2(1+i)^2} \quad (\text{C.4})$$

The curves described by equations (C.3) and (C.4) are downward sloping (as the interest rate rises, the wage rate decreases). As is well known, because of competition, the technique that is to be adopted is the one that allows paying the higher wage rate for a given rate of interest. Hence, for values of the interest rate between 0 and 0.5 and higher than 1, technique α is cost-minimizing, while for values between 0.5 and 1, technique β is cost-minimizing. At $i=0.5$

and $i=1$ both techniques coexist. We can now proceed to derive the *wage curve*, i.e. the outer-downward sloping curve that indicates, for each value of the rate of interest, the wage rate that will prevail in equilibrium in the economy according to which technique is cost-minimizing. The wage-curve is depicted in Figure 2.

FIGURE 2: The wage curve.



Now, if we proceed to calculate the average period of each technique according to Allais's measure (equation (2) in the main text), we obtain^[184]:

$$\begin{cases} \Theta_{\alpha} = \frac{(0*0 + 7*1 + 0*2)w}{7w} = 1 \\ \Theta_{\beta} = \frac{(6*0 + 0*1 + 2*2)w}{8w} = \frac{1}{2} \end{cases} \quad (C.5)$$

Then, if the rate of interest increases from $i=0.5$ to $i=0.5+\varepsilon$ (with $\varepsilon > 0$), we obtain the result predicted by Allais's theory, i.e., the rate of interest increases and the average period decreases because we switch from technique α to β . However, if the rate of interest increases further from $i=1$ to $i=1+\varepsilon$, there is a

¹⁸⁴ Note that given that the example assumes that only labour is used as a non-produced factor, the average periods calculated by (C.4) are independent of distribution. However, as we shall see in a moment, because prices are calculated under the rule of compound interest, there is reswitching of techniques and hence the average period of each technique cannot provide a measure of the capital intensity of the techniques under use.

switch back to α , and hence the technique that is (re)adopted has the lowest average period of production. The phenomenon of reswitching therefore undermines the possibility of ordering techniques according to their degree of capital intensity^[185].

We can now proceed to give an alternative argument to the one provided in APPENDIX B that shows that the demand for capital, $C_{\mu\omega}$, does not necessarily contra-varies with the rate of interest. Recall that $C_{\mu\omega}$ is equal to $\sum_{\rho=0}^n \rho \Delta_{-\rho}$; under the assumption that there is only one non-produced input, $C_{\mu\omega} = \sum_{\rho=0}^n \rho w L_{-\rho}$. Then, denoting $C_{\mu\omega}^m$ the demand for capital when technique $m = \alpha, \beta$ is in use, we have:

$$\begin{cases} C_{\mu\omega}^{\alpha} = 7w \\ C_{\mu\omega}^{\beta} = 4w \end{cases} \quad (\text{C.6})$$

Finally, by replacing equations (C.3) and (C.4) into (C.6) we obtain the aggregate demand for capital as the interest rate varies. Recalling that technique α is adopted when the rate of interest is between 0 and 0.5 and also for values higher than 1, while technique β is utilized for values of the rate between 0.5 and 1, we obtain:

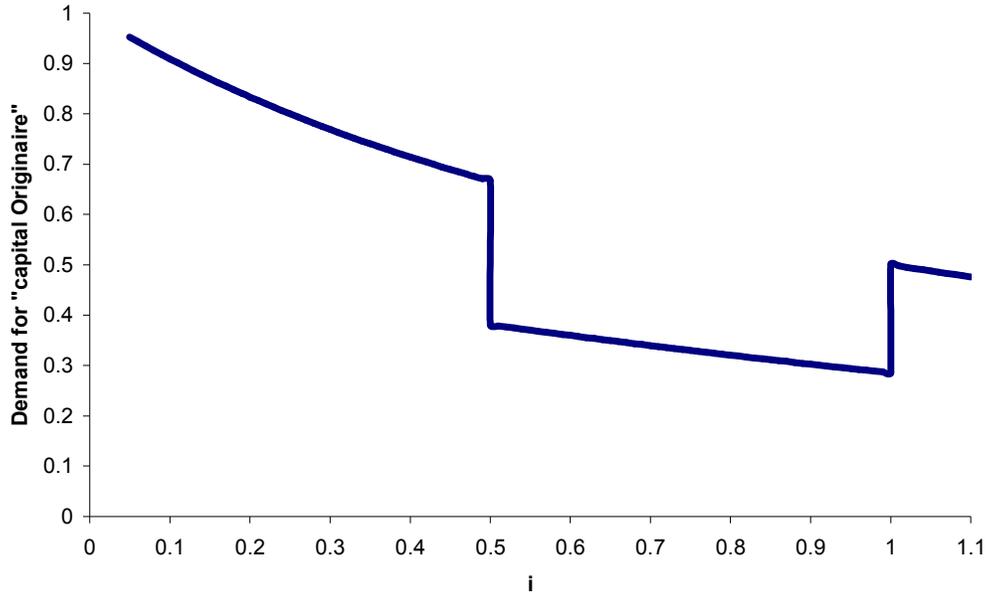
$$C_{\mu\omega} = \begin{cases} C_{\mu\omega}^{\alpha} = \frac{1}{(1+i)} & i \in [0; 0.5] \cup [1; +\infty) \\ C_{\mu\omega}^{\beta} = \frac{4}{6+2(1+i)^2} & i \in [0.5; 1] \end{cases} \quad (\text{C.7})$$

¹⁸⁵ It is worth noting that when prices are calculated under the rule of simple interest, the price of champagne is computed according to equation (6) in the main text, and equation (C.3) thus becomes $w = \frac{1}{4(2+i)}$; the new curves thus intersect only once, at $i = \frac{1}{3}$, implying that reswitching

cannot occur. In this case, technique α is adopted for values of i between 0 and $\frac{1}{3}$, while technique β is more profitable for values of the interest rate that are higher than $\frac{1}{3}$. At $i = \frac{1}{3}$ both techniques are equally profitable. Under the rule of simple interest therefore, as the interest increases, the economy becomes less capitalistic, i.e. the technique that is adopted (β) is the one with the lower average period of production.

The shape of the curve is illustrated in Figure 3 below.

FIGURE 3: The demand for “capital originaire”



The figure shows that, while at the switch point $i=0.5$ the demand for capital moves in the direction predicted by neoclassical theory, that is, cost-minimizing techniques use less capital as the interest rate increases, at the switch point $i=1$ the demand for capital increases notwithstanding the interest rate has also increased, contradicting Allais's claims.

PART IV

THE NOTION OF EQUILIBRIUM AMONG THE SECOND GENERATION OF NEO- WALRASIAN SCHOLARS

LUCAS AND MALINVAUD: NEOWALRASIAN EQUILIBRIA AS CENTRES OF GRAVITATION

INTRODUCTION.

This last part of the dissertation starts by examining the reactions of a group of contemporary scholars who have endowed modern general-equilibrium theory with explanatory-predictive value. Although different in their respective approaches, we shall argue that Lucas and Malinvaud can be considered as some of the most important representatives of this group. There seem to be two main reasons to study the notion of equilibrium that emerges from Lucas's and Malinvaud's contributions. A first important reason is that their works, especially Lucas's, have been highly influential in the development of mainstream macroeconomics in the last decades. Besides, in the case of Malinvaud, the importance of reviewing some of his contributions on equilibrium and capital is that this author is widely acknowledged to be one of the most important contributors to neo-Walrasian price and distribution theory (*cf.* e.g. Malinvaud, 1953). Second, the analyses of both Lucas and Malinvaud will reveal that, although these scholars attempt to ground their work on neo-Walrasian theory, they actually rely on traditional ways of reasoning that stand or fall with the notion of capital, the single magnitude in value terms. Therefore, the assessment of these scholars' contributions will bring further support to one of the central claims of this dissertation: the notion of equilibrium as endowed with explanatory role needs, within neoclassical theory, the factor capital to be specified in value terms.

In regard to Lucas's position, we shall see in section 1 that, while on the one hand this author argues that the economy can be represented as being *always* in full-employment rational-expectations equilibrium, on the other hand he forcedly admits that adjustments take time, because the assumption of rational expectations presupposes that agents have learnt the underlying conditions of the market under a process of trial and error and experimentation. This claim

presupposes persistent data, and in a heterogeneous-capital-goods world, it takes for granted the traditional notion of capital as a single scalar. Albeit implicitly, the notion of capital as a single factor is also behind Lucas's recourse to one-good models to explain the trends in income distribution in terms of well-behaved factor substitution mechanisms, with factors earning their marginal products. In section 2 we shall see that Malinvaud is more cautious and openly acknowledges that adjustments are not instantaneous as neo-Walrasian theory presumes; however, he argues that over relatively long periods, the assumption that neo-Walrasian paths sufficiently approximate actual paths is an acceptable simplification because, although sluggish in their action, the supply-and-demand forces are always at work in the factors markets. As we shall see, Malinvaud's justification presupposes the traditional value notion of capital in its demand side role: he will argue that over long periods of time labour demand is sufficiently elastic with respect to the wage rate, and investment demand is fairly elastic with respect to the interest rate because, e.g., a decrease in the wage-interest ratio induces profit maximizing firms to adopt more labour-intensive techniques of production.

To an important extent therefore, a line of continuity can be traced between this group of scholars and the first generation of neo-Walrasian authors reviewed in the second part of this dissertation. Those authors, let us recall, by specifying a given vectorial endowment of capital goods among the data in the problem of prices and distribution, implicitly changed the kind of equilibrium traditionally determined by marginalist theory to what I have labelled a Market Nash Equilibrium (MNE), a solution concept that must abstract from the issue of how equilibrium comes about. However, I have also argued that the aim of these authors to use economic theory to explain real world phenomena forced them to be *inconsistent* with this latter notion of equilibrium, and to continue thinking in terms of a centre of gravitation. Moreover, we have also seen that, to argue in these terms, these authors were forced to rely, in some way or another, on a value conception of the factor capital. We will accordingly see in this chapter that Lucas's and Malinvaud's developments also take for granted both this value conception of capital and the traditional neoclassical tendencies that stand or fall with this notion. Therefore, like the first generation of neo-

Walrasian scholars, both Lucas and Malinvaud *illegitimately* attempt to present traditional reasonings as justified by modern general equilibrium theory^[186]. The main conclusion that will come out from our discussion of both Lucas's and Malinvaud's contributions is that, despite their claims to the contrary, neither Lucas nor Malinvaud have any theory *at all*, since their results are supported neither by modern theory, which cannot deal with real-time adjustments due to the lack of persistence of its data, nor by traditional theory, marred by the untenable notion of capital as a single factor, measured in value terms.

1. LUCAS ON EQUILIBRIUM AND CAPITAL

1.1. Friedman and the Philips Curve.

A complete description of Lucas's methodological and theoretical research programme amply exceeds the aim of this section^[187]. I will limit myself to describe the main features of Lucas's conception of equilibrium by analysing some of his contributions to the theory of cycles during the 1970's and 1980's. In this sense, Lucas's work can be interpreted as an attempt to firmly ground Friedman's contributions on the Phillips Curve on neo-Walrasian general equilibrium tenets. As is probably well known, Friedman (1968, 1977) argues that the trade-off between labour unemployment and inflation that shapes the Phillips Curve can only hold in the short-run, since any attempt of the monetary authorities to decrease the unemployment rate by increasing the money supply will sooner or later make workers -who have *adaptive* expectations in Friedman's construction- realize that the price level will eventually rise proportionally in the future and, therefore, workers will inevitably end up by raising their demand for higher money wages so as to leave their real wage unaltered. Therefore, Friedman's argument goes, while effective in the short-run, the increase in money supply -and hence in money wages and inflation-

¹⁸⁶ However, there is an essential difference between the authors reviewed in the previous chapters on the one hand, and Malinvaud and Lucas, on the other. The contributions made by Hayek, Hicks, Lindahl and Lange took place *before* the results of the Cambridge Controversies regarding the logical problems behind the notion of value capital were definitely established and admitted by the neoclassical side of the debate, and hence these authors can be to some extent excused for holding their belief that the economy would in the long run behave *as if* a given quantity of value capital could be specified independently of distribution and could be treated like a technical factor in the substitution mechanisms; on the contrary Lucas's and Malinvaud's contributions come out *after* these negative results.

¹⁸⁷ For a thorough review of Lucas's methodological and theoretical approach the reader is referred to Vercelli (1991).

cannot exert any permanent effect on the unemployment rate (money is said to be *neutral* in the long run), which must return to its long-run “natural rate”; in other words, Friedman argues that the long-run (or equilibrium) level of employment is determined by the *real* forces of supply and demand. So, Friedman (1977: 14) naturally concludes that when the increase in prices continue, “perceptions will adjust to reality”, and therefore

Ultimately, employment will be back at the level that prevailed before the assumed unanticipated acceleration in aggregate nominal demand. (Friedman, 1977: 14)

Now, it must be noticed that, while Friedman (1968:8) argues that “the natural rate of unemployment” is grounded on “Walrasian theory of general equilibrium equations” (Friedman does not bother to distinguish between Walras’s and neo-Walrasian general equilibrium theories), his analysis remains fully traditional, since the natural rate is assumed to be reached after sufficient time, by trial and error and experimentation, that is to say, after a process of learning having taken place. In this connection, the traditional nature of Friedman’s analysis is explained by Friedman (*id.*: 7-8) himself, who points out that the ‘natural rate of unemployment’ is the *mirror-image* of the natural rate of interest, a traditional concept used by Wicksell to reflect the behaviour of the marginal product of capital, and that can differ from the rate of interest set by the monetary authorities only temporarily. In the long run, as Wicksell argument goes, the Central Bank will eventually adjust the market rate to the natural interest rate to avoid cumulative inflation or deflation. Essentially, Friedman maintains that the same kind of distinction can be traced in the labour market between the observed (market) and the natural rates of unemployment. He writes:

Thanks to Wicksell, we are all acquainted with the concept of a ‘natural’ rate of interest and the possibility of a discrepancy between the ‘natural’ and the ‘market’ rate. The preceding analysis of interest rates can be translated fairly directly into Wicksellian terms. The monetary authority can make the market rate less than the natural rate only by inflation. It can make the market rate higher than the natural rate only by deflation... This analysis has its close counterpart in the employment market. At any moment of time, there is some level of unemployment which has the property that it is consistent with equilibrium in the structure of real wage rates. (Friedman, 1968: 7)

And in analogy with the behaviour shown by the market and the natural rates of interest in Wicksell’s analysis, Friedman argues that the long run outcome of

both workers' and firms' behaviour is that the natural rate of unemployment will set the pace of the market rate. So, it seems that Friedman's distinction between a market and a natural (non-stationary) rate of real wages faithfully reflects traditional neoclassical thought and, accordingly, so do his conclusions regarding the long-run effects of money supply expansion on the level of output and employment.

1.2. Lucas: Rational expectations, instantaneous adjustments and the notion of contingent claim equilibrium.

In his seminal contribution, "*Expectations and the neutrality of money*" (1972), Lucas replaces the hypothesis of adaptive expectations with the assumption of rational expectations (henceforth RATEX), and he introduces the "new" (*id.*: 104) notion of RATEX equilibrium. As Lucas further clarifies in a subsequent article, "*Methods and problems in real business cycles theory*" (1980), the RATEX equilibrium is allegedly grounded on the notion of Arrow-Debreu 'contingent claim' equilibrium. In this framework, Lucas explains, uncertainty is introduced by indexing goods both "by the date on which they are to be exchanged and by the (perhaps stochastically selected) 'state of nature' contingent on which the exchange is to occur", and equilibrium is defined as a set of relative prices and quantities that clears current and future state-contingent markets. These prices and quantities are assumed to come about through an auctioneer-guided tâtonnement (*cf.* e.g. Lucas, 1980: 711); the auctioneer "operates so rapidly that he is not noticed" (*id.*). Now, Lucas notes that while "individual traders may assess the probabilities of the occurrence of future states of nature", if one assumes complete markets in contingent commodities "the issue of price expectations does not arise" (*id.*). Lucas thus introduces expectation functions into the analysis by formalizing the intertemporal equilibrium with contingent commodities as a sequence of temporary equilibria (i.e. only spot markets clear), and adds the assumption that individuals know the correct probability distribution of future prices and quantities because they have rational expectations. He explains:

[t]hink of a contingent-claim equilibrium as being determined via a sequence of 'spot' markets, in which current prices are set given certain expectations about future prices. On this second interpretation, one needs a principle to reconcile the price distributions implied by the market equilibrium with the distributions used by agents to form their

own views of the future. John Muth noted that the general principle of the absence of rents in competitive equilibrium carried the particular implication that these distributions could not differ in a systematic way. His term for this latter hypothesis was rational expectations. (Lucas, 1980: 707)

Despite the assumption of RATEX, Lucas's model does capture a short-run positive relationship between inflation and the level of output – Lucas (1972:103) argues that this relationship is “essentially a variant of the well-known Phillips Curve”- but only because individuals have access to *limited or imperfect information* (Lucas, 1972, 1981), and hence they “cannot discriminate perfectly between real and monetary demand shifts” (Lucas, 1972: 116); so monetary changes have real effects because the variation in only one variable, the average price level, reflects two different kinds of ‘shocks’, a ‘monetary’ and a ‘real’ shock, and individuals are unable to properly disentangle them. The implication is that, although agents base their production and investment decisions on perceived (expected) relative prices (that only depend on the ‘real’ characteristics of the economy), and those who want to work do work so there is no involuntary unemployment, when there is a ‘monetary shock’ that causes the general level of prices to increase, agents

[t]end temporally to mistake a general increase in all absolute prices as an increase in the relative price of the good they are selling, leading them to increase their supply of that good over what they had previously planned... The increase of output above what it would have been occurs whenever this period's average economywide price level is above what agents had expected it to be on the basis of their previous information... The hypothesis of rational expectations is being imposed here: agents are assumed to make the best possible use of the limited information they have and to know the pertinent objective probability distributions. This hypothesis is imposed by way of adhering to the tenets of equilibrium theory. (Lucas and Sargent 1981: 307)

However, the theory predicts that monetary authorities cannot exploit this fact: since in this framework the “choice of a monetary policy is equivalent to the choice of a density function f governing the stochastic rate of monetary expansion” (Lucas, 1972: 119), and since RATEX individuals are assumed to know this density function, any “systematic activist policy” (Lucas and Sargent 1981: 307) pursued by the Central Bank will only result in a higher level of current prices, while leaving perceived relative prices unaltered; hence, agents do not have any incentive to increase current production by modifying their optimal leisure-consumption patterns, i.e. no real effect takes place in the economy, not even in the very short run. Lucas thus concludes: “Effectiveness

[in monetary policy], then, rest on the inability of private agents to recognize systematic patterns in monetary and fiscal policy” (Lucas and Sargent, 1981: 309-310).

It is also important to stress that the Lucasian economy is in rational expectations equilibrium even when the positive relationship between inflation and employment does take place, because this relationship is the consequence of *objective* informational limitations of the economy and not of *subjective* mistakes made by the agents that trade in markets, and that are assumed to be eliminated over sufficient time. In this connection, Lucas concludes his 1972 article with the following remark:

The Phillips curve emerges not as an unexplained empirical fact but as the central feature of the solution to a general equilibrium system. (Lucas, 1972: 122)^[188]

For our purposes, the following point is worth noting: unlike traditional neoclassical scholars, Lucas *does not* explain economic fluctuations in output and employment as disequilibrium *deviations* with respect to a ‘norm’ that the economy tends to realize after a process of learning (error-correction) on the part of agents having taken place. In Lucas’s model agents never learn because the density of the ‘shocks’ is known, they do not commit any mistake even in the very-short run^[189]; and the assumption of the auctioneer, which amounts to assuming *instantaneous equilibration*, implies that even in a neo-Walrasian setting that includes physically heterogeneous capital goods among the data of the equilibrium –whose quantities and forms in disequilibrium can be very quickly altered if so decided-^[190] agents’ optimal decisions can be mutually implemented without any delay. In short, the business cycle, represented by the sequence of rational expectations equilibria, is now conceived as an “equilibrium cycle”, namely, prices and quantities “are taken to be always in equilibrium” (Lucas, 1980: 709). As a result, the traditional distinction between market (disequilibrium) and normal levels of employment, and the claim that,

¹⁸⁸ Recall that Lucas re-interprets the Phillips curve as depicting a positive relationship between inflation and the level of output.

¹⁸⁹ As Lucas (1981: 307), argues, the errors agents commit “are unavoidable given their limited information”.

¹⁹⁰ Lucas extends the 1972 paper to model the production of capital goods and hence considers investment decisions in Lucas (1975) (see section 1.3.1. below).

over several market rounds and by trial and error, the former will tend to coincide with latter, are simply erased from economic analysis.

1.3. Lucas as a traditional neoclassical scholar.

We have seen that within the approach developed by Lucas, the sequence of RATEX equilibria is directly identified with observable, empirical, variables. At *each point in time*, Lucas affirms, the economy is assumed to be in equilibrium.

One essential feature of equilibrium models is that all markets clear, or that *all observed* prices and quantities are viewed as outcomes of decisions taken by individual firms and households. In practice, this has meant a conventional, competitive supply-equals-demand assumption... (Lucas and Sargent 1981: 310-311, emphasis added)

Where by equilibrium models, Lucas means RATEX equilibrium models. However, the alleged identification between the sequence of RATEX equilibria and observable magnitudes appears to be hardly defensible: by relying on the extremely stringent hypotheses of rational expectations and the auctioneer, Lucas simply avoids a thorough discussion of the problem of how each RATEX equilibrium is supposed to come about in actual economies. It seems hard to disagree with Hahn (1990a: 241, *cf.* chapter 8) when he argues that, within the rational-expectations approach, equilibrium is essentially established as an “axiom”.

But is it really the case? I.e., does Lucas actually envisage an economy that it is *always* in RATEX equilibrium? Lucas sometimes justifies this claim on the grounds that “Any model that is well enough articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently ‘unreal’” (Lucas, 1980: 696) and, as Vercelli (1991: 130) aptly remarks, Lucas “defends himself by asking to be judged not on the realism of the hypotheses but on the usefulness of his assumptions”. In Lucas’s (*id.*: 697) view, a good model “will not be exactly more real than a poor one, but will provide better imitations [of reality]”. In a subsequent contribution, Lucas (1988) also attempts to defend himself by arguing that, if the notion of rational expectations equilibrium is extended over the infinite future, the economy would eventually reach a position of steady growth. And Lucas goes on to argue that it is this final equilibrium, and not necessarily the rational equilibrium path itself, the position around

which actually observed magnitudes tend to gravitate “most of the time” (cf. Kurz and Salvadori, 2003: 231). He writes:

What of economies that begin off the balanced path - surely the normal case? Cass showed - and this is exactly why the balanced path is interesting to us - that for any initial capital $K(0) > 0$, the optimal capital-consumption path $(K(t), c(t))$ will converge to the balanced path asymptotically. That is, the balanced path will be a good approximation to any actual path ‘most’ of the time. (Lucas, 1988: 11)

However, for the reasons addressed in the first chapter (section 4), and then in my evaluation of Allais (chapter 6), this second attempt to defend the plausibility of the RATEX path is devoid of solid justification. As I discuss immediately below, I also believe that the claim that the economy can be modelled as being always in RATEX equilibrium can be justified on more plausible bases than the rather superficial ‘Friedmanite’ kind of argument presented above and often used by Lucas (however, as we shall see below, this justification is devoid of legitimacy because it presupposes the traditional notion of capital as a single factor).

Indeed, it is not usually noticed that in some of his writings Lucas *does* admit that adjustments take time to assert themselves fully, and hence that the data that determine equilibrium prices and quantities must be sufficiently persistent if the possible mistakes and other causes of disequilibria are to be eventually corrected and compensated over time. Consider for instance what Lucas writes in “*Equilibrium search and unemployment*” (1974) with respect to the assumption of rational expectations:

The economic interpretation of this assumption of rational expectations is that agents have operated for some time in a situation like the current one and have therefore built up experience about the probability distribution which affects them. For this to have meaning, these distributions must remain stable through time. Mathematically, this means that we will be concerned with stationary distributions of demand and workforce and with behaviour rules under these stationary distributions. (Lucas, 1974: 190)

And then he observes that the probability distributions of the variables under examination

[a]re learned by processing observed frequencies in some sensible fashion... which has the property that the ‘true’ distributions become ‘known’ after enough time has passed. (Lucas, 1974: 204)

Lucas thus explicitly recognises that the hypothesis of correct expectations in equilibrium presupposes a time-consuming process of learning on the part of agents that must take place under unchanging underlying conditions, so that individuals can gain the necessary experience about the conditions ruling on the market. This same view of the equilibration process as the outcome of a trial-and-error adjustment process arises in a subsequent and little-noticed article^[191] “*Adaptive behaviour and economic theory*” (1986). In that same article Lucas (*id.*: 411-413) documents Vernon Smith’s contributions (Smith, 1962, 1982) in which, through a series of experiments with a group of individuals, this latter author attempts to show how equilibrium in a single market is to be reached by trial and error and experimentation after several market rounds. Relying on Smith’s positive results on the stability of the equilibrium, Lucas argues that stability issues can be discussed without actually assuming an implausible auctioneer-guided tâtonnement, since Smith’s setting

[s]hifted the task of adaptation from the auctioneer to the same agents whose preferences determine the equilibrium, and permitted trades to be consummated whenever mutually agreeable, just as they are in actual free markets. (Lucas, 1986: 413)

According to Lucas, “The mechanism [Smith] proposed has the virtues of being concrete, of relying on simple adaptive capacities, and of being, under a wide range of circumstances, stable” (*id.*). Actually, the positive results on stability obtained by Smith, and that Lucas approvingly quotes to argue the supply-and-demand explanation of prices and distribution in “actual free markets”, are not surprising: as Lucas (1986: 412) himself notes, in Smith’s set-up individuals are faced with the *same* data in successive market rounds (Smith, 1962: 114-115)^[192]. But then, Lucas is not authorized to extend Smith’s positive conclusions regarding the stability of the supply-and-demand equilibrium to a neo-Walrasian setting (the alleged micro foundation of the RATEX approach),

¹⁹¹ Vercelli (1991) is an exception.

¹⁹² Smith convincingly justifies the assumption of constant data as follows: “The experimental conditions of supply and demand are held constant over several successive trading periods in order to give any equilibrating mechanisms an opportunity to establish an equilibrium over time. Real markets are likely to be continually subjected to changing conditions of supply and demand. Marshall was well aware of such problems and defined equilibrium as a condition toward which the market would move if the forces of supply and demand were to remain stationary for a sufficiently long time. It is this concept of equilibrium that this particular series of experiments is designed, in part, to test.” (Smith, 1962: 115). As shown in the first chapter, this view is fully traditional.

because the constancy of the data relative to the vectorial endowment of capital goods cannot be plausibly justified. In other words, Lucas must accomplish an impossible task: he must reconcile the admitted fact that adjustments are time-consuming with his so-called micro foundation, the theory of intertemporal equilibrium, that is condemned to assume an instantaneous equilibration process owing to the lack of persistence of the datum relative to the vectorial endowment of capital goods.

1.3.1. The traditional conception of capital in Lucas's framework.

In the previous section we have shown that Lucas is thinking of mechanisms that take sufficient time to assert themselves fully, and that are incompatible with the theory of intertemporal equilibrium, the so-called micro foundation of the rational expectations approach.

Before pursuing the argument further, it is important to note that Lucas's claims regarding time-consuming adjustments could be plausibly accommodated within a traditional neoclassical framework: granted the notion of capital as a single scalar, this magnitude has the sufficient persistence so that, if stability can be assumed, the process of equilibration can be safely considered as taking place in 'real time'. In view of this consideration it is worth examining whether or not the framework of intertemporal equilibrium is the so-called micro foundation of the rational expectations approach, as Lucas declares. In other words, has the traditional notion of capital as a value magnitude actually disappeared from Lucas's framework? As I argue in the remaining of this section, the answer appears to be negative. Consider, for instance, Lucas's article "*An equilibrium model of the business cycle*" (1975)¹⁹³ (cf. also e.g. Lucas, 1988). There, Lucas develops a one-sector model where the consumption-capital good is produced by labour and itself under a well-behaved, constant returns to scale production function (Lucas, 1975: 1115).

¹⁹³ As noted in fn. 190, in the 1975 article Lucas attempts to extend the 1972 paper results to a framework where the production of and the demand for capital goods (investment) is explicitly considered.

The point I wish to make is that the model developed in the 1975 paper is, *essentially*, a Ramsey (or Solow) model^[194]; so, as Petri notices, the single capital good has “the same role as the single ‘capital’ of traditional long period equilibria [since] is taken to be a summary index of the heterogeneous capital endowment of the economy, that remains unchanged (if net savings are zero) when the composition of the capital endowment in the real economy changes due to changes in relative factor prices” (Petri: 2004: 326). Under these premises, it is readily seen that each rational-expectations temporary equilibrium that is determined in the 1975 model presents essentially the same features as a traditional neoclassical long-period equilibrium. The sufficient persistence of this equilibrium gives plausibility to Lucas’s claims: first, given that the data of the equilibrium change relatively slowly, it can be reasonably expected that, if stability can be assumed, through a process of trial and error individuals can eventually learn the underlying conditions of the market. Second, it can be reasonably argued that, at a first level of approximation, the auctioneer is a valid simplifying device that can be dispensed with: in any event, granted the stability of the equilibrium (ensured by the correct working of the factor substitution mechanisms, see immediately below) after sufficient time the economy will gravitate around the full-employment growth path determined by theory.

Moreover, given that in the 1975 article Lucas assumes a one-sector model and hence a ‘regular technology’, he derives well-behaved and sufficiently elastic factor demand curves for capital and labour (*id.*: 1116), and he can therefore capture the action of the *same* persistent forces of ‘marginal productivity’ and ‘thrift’ as those traditionally envisaged by neoclassical scholars to explain the distribution of income among the social classes over sufficient time. In other words, Lucas’s one-good model is stringent enough to exclude those kinds of ‘capital paradoxes’ such as reswitching or reverse capital deepening that can arise with many capital goods, and that by themselves undermine the traditional

¹⁹⁴ In the 1975 contribution, Lucas does not model the behaviour of the household sector explicitly. However, following the standard practice of Ramsey kind of models, he suggests that consumers’ demands can be derived from a representative consumer’s well-behaved intertemporal-utility maximizing problem. The assumption that the household sector behaves *as if* there was a single consumer who maximizes an infinite-horizon utility function is also found in, e.g. Stokey and Lucas (1989) and Lucas (1988). As we shall see in the following chapter, Hahn will harshly criticise this assumption.

neoclassical belief that the rate of interest would bring into equilibrium investment decisions with full-employment savings, hence depriving of plausibility the assumption that the economy follows on average a full-employment growth path. In the light of these considerations, it seems legitimate to conclude that, far from being a simplifying assumption, the hypothesis of one capital good, which, since in real economies capital is heterogeneous, amounts to assuming that one can treat capital as a value factor, the traditional conception of capital emerges as a distinctive feature of Lucas's construction, and hence the plausibility of his results stands or falls with the conception of capital as the traditional homogenous magnitude^[195].

A conclusion seems to emerge from our discussion of Lucas's work: while on the one hand Lucas argues that the notion of intertemporal contingent equilibrium is the so-called micro foundation of the rational expectations approach, his contributions, on the other hand, seem to *closely* follow traditional neoclassical theory, much more than what Lucas is willing to recognize explicitly. In fact, unlike the intertemporal framework, that as a matter of logic must assume instantaneous equilibration due to the lack of persistence of its data, Lucas acknowledges that individuals must learn from experience the underlying conditions of the market, and hence that the process of equilibration is necessarily time-consuming. And once the need for this kind of adjustments is admitted, the assumption that the economy follows a full-employment rational-expectations equilibrium path can be only argued along traditional neoclassical lines, namely on the grounds that this path aims to describe –and can only describe– a position that the economy tends to realize over sufficient time. We have then defended the plausibility of this interpretation on the basis of Lucas's own class of models: these are one-good models, and therefore, the explanation of income distribution that emerges from them relies on the same persistent forces at work as those envisaged by traditional neoclassical scholars, the factor substitution mechanisms that stand or fall with the traditional conception of capital as a single factor. As a result of

¹⁹⁵ Petri aptly argues in this connection: “Neoclassical analyses based on one-good models are not ‘simplifications’ of neo-Walrasian disaggregated analyses, on the contrary, they embody a ‘vision’ which is prior to, and the only possible foundation of, the belief that neo-Walrasian analyses may have any connection with reality” (Petri, 1999: 47).

these considerations one seems authorized to conclude that behind Lucas's arguments there is a logical jump: an *illegitimate* attempt to present one-good reasonings as justified by neo-Walrasian equilibrium theory.

2. MALINVAUD ON EQUILIBRIUM AND CAPITAL.

I do not intend here to provide a thorough examination of Malinvaud's contributions, I will just limit myself to examine Malinvaud's appraisal of the problem of equilibrium and capital in some of his most important contributions –e.g. *Theory of Unemployment reconsidered* (1977), *Mass unemployment* (1984), *Diagnosis unemployment* (1994) and *Macroeconomic theory* (1998a, 19998b) - and also to analyse how he attempts to reconcile this equilibrium notion with modern general equilibrium theory.

2.1. Adjustments take time.

In his recent *Macroeconomic theory* textbook, Malinvaud (1998:50) argues that the central organizing equilibrium concept in macroeconomics is the notion of temporary general equilibrium^[196]. But we shall see below that Malinvaud is unwilling to accept without discussion the assumption of continuous market clearing that underlies this concept of equilibrium. In fact, in *Mass unemployment* (1984), Malinvaud admits that the claim that the economy follows a sequence of full-employment temporary equilibria is only an “idealization” (*id.*: 17) that assumes that, e.g., when there is disequilibrium in the labour market, the required change in the wage rate will “go as far as is required for maintaining equality between supply and demand” (*id.*: 18). However:

It is clear that the labour market does not operate in this way. Wages are not flexible in the short term in the way assumed by this form of the law of the supply and demand. They are not completely insensitive to pressure on the labour market, but they adjust

¹⁹⁶ Unlike Lucas, who argues that his notion of equilibrium is directly to be found in Arrow-Debreu's notion of equilibrium, Malinvaud does not discuss Arrow-Debreu equilibrium in the works we will examine, and directly considers the temporary-equilibrium versions of the neo-Walrasian approach. The justification for bypassing the Arrow-Debreu equilibrium is to be found, for instance, in Malinvaud's *Macroeconomic theory* (1998a: 49), where the French author argues that some salient features of Arrow-Debreu equilibrium, e.g. perfect foresight and complete future markets, do not exist in reality and hence the “so called Arrow-Debreu theory, is not adequate to treat most macroeconomic questions”. And therefore, “this is why macroeconomics uses a different concept: the temporary equilibrium” where complete future markets are missing and expectations can be mistaken (*id.*: 50).

much less than would be required for permanent market clearing. (Malinvaud, 1984: 19)^[197]

While on the one hand Malinvaud admits that the representation of the economy as following a sequence of continuous-market-clearing temporary equilibria is hard to digest, it is important to notice that, on the other hand, he *does not* deny the action of the forces of supply and demand on the factor markets. Malinvaud limits himself to remark that these forces “adjust much less than would be required for permanent market clearing” owing, for instance, to the existence of trade unions and regulations that may well hamper short-run supply-and-demand equilibrium in the labour market^[198], but also and more fundamentally because, as we shall see below (section 2.3), changes in labour demand caused by changes in the level of wages will generally call for a modification in the existing equipment, and this process may take considerable time. Therefore, to fully assert their effects, the supply and demand forces must be given sufficient time. “To conclude”, Malinvaud writes,

[I]et me say that the law of supply and demand is not completely inactive in the labour market, but that its influence is slow and, therefore, quite limited in the short term. Large quantity adjustments then have to occur: hours work changed, recruitments are accelerated or stopped, or lay-offs of greater or lesser numbers of workers decided upon. (Malinvaud, 1984: 20)

As he later summarizes in his *Macroeconomic theory (1999b)* textbook:

The law of supply and demand acts, but rather *slowly*... (Malinvaud: 1999b: 912, emphasis added)

I must thus insist on this important point: while on the one hand Malinvaud argues that real adjustments are certainly not instantaneous as modern general equilibrium theory presumes, he does *never* attempt to object to the explanation of prices and distribution in terms of supply and demand forces that underlies the determination of those equilibria. The author’s belief in the correctness of the supply and demand explanation of prices and distribution is indeed evident

¹⁹⁷ Here, Malinvaud’s considerations refer to the labour market, but he also extends the admission that markets are sluggish to the produced commodity markets.

¹⁹⁸ “The description of wage determination should take into account the legislation and regulations that exist everywhere in order to protect workers against exploitation and to reduce the frequency of industrial conflicts... Legislation and trade union power vary from one country to another, so that the impact of disequilibria in the labour market also varies, but is nowhere strong, except perhaps in the long term.” (Malinvaud, 1984: 20)

if we notice that, from the very outset of his 1984 work, Malinvaud envisages unemployment as a *disequilibrium* phenomenon that reflects an excess supply of labour, an assertion that presumes that the forces of supply and demand are always at work. He writes, e.g., that

[u]nemployment is a disequilibrium, revealing an excess supply of labour. (Malinvaud, 1984: 35)

Indeed, if the forces that determine the trends in distribution are other than those of supply and demand, there would be no solid grounds to argue that an excess supply of labour would eventually tend to be eliminated by plausible (non drastic) reductions in the wage rate, i.e. that unemployment is a transitory, rather than a permanent phenomenon. On the contrary, Malinvaud does stress the temporary nature of the unemployment phenomenon, and moreover, at the end of his 1984 book, he exhorts policy makers to think in 'long-run' terms, and hence to take this temporary-nature of unemployment into account when deciding about the best economic policies to pursue.

[m]ost economists agree that, even though it will still last for some time, mass unemployment will also some day cease to be the general problem that it now is. No formal proof of this exists... but its intuitive validity is supported by reference to economic history, which shows that past periods of mass unemployment end. When deciding about present economic policies, we should, then, not restrict attention to the present situation but also, sometimes, look beyond, especially when the measures contemplated have a structural character or concern long-range decisions. Nothing in our economic institutions is absolutely irreversible, but we know by experience that Keynes's famous sentence about the long run is dangerous when it leads to the neglect of everything except the proximate future. (Malinvaud, 1984: 100)

In this passage Malinvaud mentions the tendency not of unemployment, but of "mass" unemployment, to be eliminated after sufficient time, and hence one could argue that, in Malinvaud's view, unemployment will indeed persist even in the long-run. But no neoclassical economist who, like Malinvaud, realistically accepts that the supply and demand forces are sluggish, would deny that, owing to the effect of accidental events, social pressures, trade unions, informational and matching problems, etc. there would be always some positive level unemployment in the economy that can be considered as 'normal'. This normal level is called "frictional unemployment" by Malinvaud (1977: vii-viii) and

corresponds, e.g., to Friedman's natural level of unemployment^[199]. Therefore, in the previous passage Malinvaud seems to be simply embracing the traditional neoclassical claim that, with sufficient time, sufficient changes in the wage rate will allow the economy to gravitate towards a position of full-employment growth. Now, no doubt, Malinvaud's assertions are at least disputable, since had they been true, one would thus expect that, at least on average, the economy would be close to a full-employment level of output; yet, positions of full employment seem to be the exception rather than the norm.

2.2. The plausibility of the full-employment growth path.

Now, the point I wish to make is that, while on the one hand Malinvaud believes that the representation of the economy as a sequence of (full-employment) temporary equilibria does some violence to reality due to its continuous-market-clearing assumption, and wages may be better modelled as *fixed* in the short-run -i.e. within a general temporary equilibrium framework "with rationing" (cf. Malinvaud, 1998a: 51; 1998b: 880)- on the other hand it would seem that Malinvaud still believes that the sequence of temporary equilibria with flexible (market clearing) prices does essentially capture the action of the fundamental forces at work for the determination of long-run growth, since given sufficient time, the economy will gravitate around a position of full-employment growth anyway. This is the idea that, for instance, seems to emerge from Malinvaud's *Macroeconomic theory* textbook:

Knowing whether a price must be taken as fixed or flexible will obviously depend on the sort of phenomenon studied and on the length of the lags that it will be permissible to disregard. When one examines long-run growth, prices seem to be flexible since they can be viewed as evolving according to regular trends with a roughly constant degree of market slack. On the contrary, when studying short-run phenomena, one will often prefer to treat many prices as fixed, or rather as moving exogenously, or still better, as reacting with a lag to the sequence of equilibria whose determination one will study. (Malinvaud, 1998a: 51)

¹⁹⁹ As Malinvaud (1999b: 854) explains in his discussion of short-run fluctuations: "What matters for macroeconomists is to recognize that a part of unemployment, as revealed by statistics, cannot be assigned to a global disequilibrium between the aggregate supply of labour and the aggregate demand for it, but to the costs of search and difficulties of adjustment which prevent perfect and immediate matching between supply and demand at the microeconomic level... But the main concern of the macroeconomic theory here discussed is to explain variations in the remaining part [that part of unemployment that can be explained in terms of supply and demand forces]".

According to the author, this dichotomy between short-run rigidity and long-run flexibility is not necessarily contradictory. “Someone is not necessarily schizophrenic”, he asserts,

[i]f he or she accepts price and wage flexibility in microeconomic theory in order to explain long-run changes in the relative prices of various goods, or in the relative wage rate of various professions, but at the same time accepts also stickiness of the average nominal or real wage rate in the macroeconomic theory in order to explain short-run changes in unemployment (Malinvaud, 1998b: 873)

And when analysing the determinants of the real wage, Malinvaud (*id.*: 641, emphasis added) declares: “In our attempt to understand the underlying factors that determine the level of the real wage, we may rely upon the equality that *tends to appear*” between supply and demand for labour by sufficient changes in the wage rate. While he admits that the argument is “too simple” (*id.*), because it ignores short-term rigidities, etc., he adds:

But, when studying growth, many economists consider it legitimate to pass over the difficulties of knowing how [equilibrium] is achieved at each point in time. They choose to keep only those equations which would hold under full ‘market-clearing’, i.e. under the hypothesis that supplies and demands would be continually in equilibrium in all markets. The equations thus obtained *are supposed to be valid for long-term trends*, even though they are repeatedly contradicted in short-run adjustments. We shall accept this view for the moment and in most of the subsequent developments of this chapter. (Malinvaud, 1998b: 642) [200]

Note that Malinvaud explicitly declares that the neo-Walrasian general equilibrium equations that describe the economy as being in continuous equilibrium, should not be taken literally, since these equations, although “repeatedly contradicted in short-run adjustments”, are actually “supposed to be valid for long-term trends” only. In this connection, we have in fact seen in the previous section that Malinvaud (1984:100) believes that, although no “formal proof exists”, economic history “supports the intuitive validity” of the claim that periods of mass unemployment eventually come to a halt, i.e. only frictional unemployment remains. In sum, the content of all these passages strongly

²⁰⁰ The same view emerges in his *The theory of unemployment reconsidered* (1977): “It is fair to say that, according to the prevailing view, the fixed price hypothesis would be appropriate for dealing with short-run phenomena such as unemployment, whereas the pure flexible-price hypothesis would suit the study of growth and long-run tendencies. Prices and wages would, indeed, be sticky but would, all the same, move in reaction to sustained excess demands or supplies. A more or less pure Keynesian theory provides us with the required foundations for the first type of phenomenon, and a more or less pure neoclassical price theory for the second type. I am not going to dispute this view... I am even going to accept that a fully appropriate flexible-price theory may be built for the study of growth, and that this theory need not consider short short-term fluctuations except in their purely qualitative features. (Malinvaud, 1977: 92)

suggests that Malinvaud firmly believes that, although ‘rigid’ in the short run, wages and prices are sufficiently responsive to excess demands in the long-run so as to render it legitimate to model the economy as following a full-employment growth path^[201].

2.3. Capital-labour substitution.

Having objected to the assumption of continuous market-clearing but, at the same time, having accepted that “even though they are repeatedly contradicted in short-run adjustments” (Malinvaud, 1999b: 642) the hypothesis that “supplies and demands would be continually in equilibrium in all markets [is] supposed to be valid for long-term trends” (*id.*), Malinvaud must face the difficult task of elucidating why this is the case; that is to say, he must explain why, although “*slowly*”, the forces of supply and demand do “*act*” in the labour market, as Malinvaud (1999b: 912; 1977: 101) repeatedly claims. As we discuss in this section, the author will meet deep troubles in accomplishing this task, because he will have recourse to the traditional factor substitution mechanisms that presuppose the illegitimate notion of capital as a single scalar of variable form.

Malinvaud remarks that, in order to understand why adjustments are not “quicker or more satisfactorily oriented” (Malinvaud, 1984: 50) a proper analysis of unemployment “must pay some attention to mobility of labour and capital from contracting to expanding industries” (Malinvaud, 1984: 50-51). And later, he explains:

A relative cost of labour with respect to capital induces firms to choose a less capital-intensive technique of production and, therefore, to have a larger labour demand for any given amount of the aggregate demand for their product. The impact of a reduction of wages on unemployment then depends on two opposing factors: a decrease of aggregate demand, but an increase of labour requirement per unit of output. Which factor

²⁰¹ It would seem that Hicks is the precursor of Malinvaud’ ideas. As we may recall from our examination of *Value and capital* (chapter 4), in his discussion of the stability of the temporary equilibrium Hicks admits that wages are fairly rigid in the short-run, and hence he acknowledges that the assumption of market-clearing along each temporary equilibrium is “highly unrealistic” (Hicks, 1939: 265). It is only after sufficient time, Hicks continues, that unemployment will eventually be eliminated. In this regard, in his 1977 book Malinvaud explicitly admits to have borrowed the notion of fix price from Hicks: “Given the short-run rigidities that actually exist, the theory under consideration is justified in assuming full price rigidity, i.e. in working with models in which prices and wage rates are exogenous. This is going to be a ‘fix price’ theory according to the denomination proposed by Hicks” (Malinvaud: 1977, 11-12).

dominates depends, in particular, on the degree of capital-labour substitutability. There is now good documentation on the theory according to which this substitutability is quite small in the short term, for work on given equipments with a given organization of production, but quite significant in the long term, when equipments are built or replaced, and when methods of production are reorganized. We can then imagine that the responses of employment to lower wages will be negative in the short term but positive in the long term. (Malinvaud, 1984: 64-65)

So, although Malinvaud argues that the general equilibrium equations that are supposed to be valid for “long run trends” (and give justification to the assumption that the economy follows a full-employment growth path) are the neo-Walrasian, temporary-equilibrium equations, in this passage he justifies the negative inclination of the labour demand curve by *illegitimately* relying on traditional neoclassical adjustments in terms of labour and capital, the single factor in value terms, a concept that has no room within neo-Walrasian theory, because it takes as datum of the equilibrium the vectorial endowment of capital goods. Moreover, note that, albeit implicitly, Malinvaud also relies on the traditional notion of capital as a single factor of *variable form* when he accepts that, given that most of the existing physical equipment is highly specific in the short-run (i.e. technology is of the putty-clay type), when a persistent excess supply of labour causes real wages to decrease, sufficient time is needed for “methods of production [to be] reorganized”, i.e. for profit-maximizing firms to “build” new capital goods, or to “replace” the existing forms of those capital goods in order to adopt more labour intensive techniques that in turn allow aggregate employment to increase. In other words, Malinvaud is very close to admitting here that a plausible explanation of prices and distribution in terms of supply and demand forces *needs* treating the capital endowments as endogenous variables, e.g. determined by the conditions of (long-period) equilibrium, with the implication that, within neoclassical theory, capital must be unavoidably measured as a single factor. Little wonder then that Malinvaud conjectures that “the responses of employment to lower wages will be negative in the short term but positive in the long term”, i.e. an excess supply of labour will eventually decrease wages and this will induce an increase in employment demand. And in fact this is, Malinvaud finally declares,

[t]he conclusion to be drawn from the dynamic responses that have been studied within a simple model in which technology has the putty-clay feature. (Malinvaud, 1984: 64-65)

(in this “simple model”, it must be noted, capital is treated as a single factor of variable form). And in Malinvaud (1998a: 448) it is further argued that “evidence shows that in the short run, the effects of changes in real factor costs on factor demands are very small; but in the long run, they appear to be *sizable and significant*. A simple rationalization of these findings would be given by the putty-clay technology.” And again, in this “simple rationalization” Malinvaud treats capital as a single factor of variable form.

In *Diagnosing unemployment* (1994), Malinvaud insists on the fact that the sufficient negative wage-rate elasticity of the labour demand curve “is intimately linked to investment” (*id.*: 83), i.e. presupposes that, when the wage rate decreases, a long period of time is needed for firms to change the form of the existing capital stock. He argues:

The wage gap has a natural definition; it is the difference between the current real wage rate and a value that would be consistent with a satisfactory level of the demand for labour by firms, assuming the demand for goods would otherwise be satisfactory. The hypothetical value of the real wage rate that is considered would permit an equilibrium of the labour market, while the current value is claimed to prevent it. Clearly, the contemplated equilibrium is not meant to be implemented in the short run, but after several years at least, so that productive capacities are themselves consistent with it. Now, all the measures [of the wage gap] so far proposed assumed the capital stock to remain fixed at its current state; they then considered a hypothetical increase of the demand for labour working on this capital stock. This assumption removes from the representation of the demand for labour what I consider to be its main proximate medium-term determinant besides the demand for goods, namely adaptation of the capital stock. (Malinvaud, 1994: 127-128)

The passage confirms Malinvaud’s traditional neoclassical view of the working of the factors markets: the sufficient elasticity of the labour demand curve needed to determine a plausible equilibrium real wage requires an *endogenous* determination of the composition of the capital stock. The implication is that, when deriving the labour demand curve, the capital goods endowments cannot be treated as distinct factors of production as in Malinvaud’s alleged micro-foundation, the TGE framework; instead, the capital goods must be forcedly conceived as embodiments of somehow a single factor of production of variable form. It is not surprising then that in the 1994 contribution Malinvaud (1994: 118) specifies a very traditional production function with labour and capital, the single factor, as arguments. To conclude then, Malinvaud has recourse to the traditional notion of capital as single factor not only to justify the *correct*

working of the factor substitution mechanisms in capital goods producing economies, but also to derive *sufficiently* elastic factor demand curves that gives plausibility to the supply and demand explanation of distribution and employment over sufficient time^[202].

3. CONCLUDING REMARKS.

Our assessment of Lucas and Malinvaud can be summarized as follows: with some differences, both scholars strongly believe that the path traced by temporary equilibrium prices and quantities indicates actual paths with sufficient approximation, and hence that neo-Walrasian theory can still have a correspondence with observation, and can be also used to derive policy prescriptions. As we have seen, their reasonings presuppose i) persistent data, which in a heterogeneous capital-goods world amounts to specifying the factor capital as a single factor, measured in value terms: sufficient time is needed for agents to learn from their past mistakes and to know the underlying conditions of the market (Lucas), and also for firms to change the physical form of the existing capital stock when distribution changes, which means that the endowments of capital goods must be treated as endogenously determined variables (Malinvaud); ii) well-behaved factor substitution to derive sufficiently elastic factor demand curves that give plausibility to the supply and demand explanation of distribution, and the tendency towards the full employment of resources (Lucas and Malinvaud). In short, their claims to the contrary notwithstanding, Lucas and Malinvaud's arguments are fully traditional: they illegitimately reason *as if* things would continue working in the ways justified by the traditional conception of capital, the single factor measured in value terms. In this sense then, these two authors are important representative scholars of a broader group of authors who, as Garegnani (1970: 349) has argued, have fallen in the illegitimate *temptation* "to use the short-period approach to provide some rationalisation of results which derived their plausibility only from a long-run

²⁰² It must be noticed however that Malinvaud, unlike Lucas, at least is conscious of the potential criticism based on the problems of reverse capital deepening and reswitching. In some contributions Malinvaud (1986, 2008) has attempted however, to minimize the importance of these phenomena on the grounds that their probability of occurrence is small. I will not discuss the limits of Malinvaud's position on this issue here. In general terms, I agree with the position of Garegnani (1990a), Ciccone (1996) and Petri (2011b).

analysis” [203]. To conclude, the main outcome of my discussion of Lucas’s and Malinvaud’s contributions is that, their claims to the contrary notwithstanding, neither of them *has any theory at all*: their results are supported neither by modern theory, which cannot deal with real-time adjustments due to the lack of persistence of its data, nor it can justify the sufficient negative elasticity of the factor demand curves, needed to plausibly argue the stability of the neoclassical equilibrium; nor their claims can be justified by traditional theory, marred by its illegitimate notion of capital.

²⁰³ But it would seem that Lucas and Malinvaud are only the tip of the iceberg. For instance, as Petri (1999, 2004: chapter 8) convincingly argues, in most empirical applications, neoclassical economists still rely on a value conception of capital to derive their conclusions and their policy prescriptions. The issue is also admitted by Arrow (1989): while he dismisses the relevance of reverse capital deepening and reswitching for neo-Walrasian theory by arguing that “I do not think that any true Walrasian can learn much from it, because he or she never assumed that there is only one kind of capital good”, he proceeds to note that these problems are however “important for the applied Walrasians” (Arrow, 1989: 154), because they reach conclusions that ultimately rely on models where capital is treated as a single factor of variable form. But as will be shown in the next chapter, Arrow too reaches conclusions that ultimately rely on the traditional notion of capital as a single factor.

8.

HAHN: NEO-WALRASIAN EQUILIBRIUM AS MARKET NASH EQUILIBRIUM

INTRODUCTION.

This final chapter critically examines Frank Hahn's considerations on the notion of neo-Walrasian equilibrium. The main reason to focus on Hahn's contributions is that his reflexions on this topic appear to be rather *unique*: this author is very aware that due to the problems of stability and impersistence, and also due to the lack of complete markets and perfect foresight, neo-Walrasian equilibrium, or the sequence of such equilibria, cannot be conceived as a centre of gravitation of actually observed magnitudes. And yet, Hahn attempts to rehabilitate the notion of neo-Walrasian equilibrium, in particular, the Arrow-Debreu notion of equilibrium. The aim of this last chapter is to critically assess this attempt.

In order to introduce the reader to Hahn's position on equilibrium, the first section documents his critiques to the position shared by authors like Lucas or Malinvaud (*cf.* chapter 7). On the one hand I will argue that Hahn's critiques are fully justified: he takes these authors' view at their face value, and consistently shows that their results cannot be derived from their alleged micro foundation, pure neo-Walrasian theory. On the other hand however, I will argue that those critiques show that Hahn in fact seems to grasp neither that underlying those authors' claims there actually emerge traditional neoclassical reasonings, nor that the main weakness of those claims is that they completely lack plausibility once it is acknowledged that they are marred by their reliance on the traditional notion of capital as a single factor of variable form. So, the main conclusion that will come out of this first part is that Hahn's critiques are a *manifestation* of his lack of familiarity with those traditional neoclassical gravitational modes of explanation, and with the central, albeit implicit, role played in those arguments by the untenable notion of capital as a single factor.

Section two critically discusses the reasons adduced by this author to rehabilitate the notion of intertemporal equilibrium in the form developed by Arrow and Debreu (1954) and Debreu (1959). As will be seen, Hahn envisages intertemporal theory as a MNE, a *solution concept* that only limits itself to show the mere *possibility* that agents' plans could be mutually compatible; i.e. he *disentangles* the notion of equilibrium from the issue of how this equilibrium plausibly emerges in real economies, as if these were two *independent* issues that bear no relation to each other. To defend this interpretation of the equilibrium concept, Hahn develops three different but related arguments. First, he provides a very particular reading of the history of economic analysis: he claims that neo-Walrasian theory in its intertemporal form gives an "abstract" answer to a "two-hundred-year-old", "important" but "abstract" question that dates back to Adam Smith, namely, can equilibrium among self-seeking agents be "logically possible"? Second, Hahn goes on to argue that however abstract it may be, Arrow-Debreu equilibrium is in any case a useful theoretical device: it can be used as a "reference point" to be compared against actual economic states. Third and finally, Hahn maintains that "it was always understood" by the most rigorous modern general equilibrium theoreticians that intertemporal equilibrium is not meant to be a description of reality.

The main conclusion that will come out of the discussion of Hahn can be summarized as follows: on the one hand, both Hahn's clearer perception that Arrow-Debreu equilibrium has not explanatory-predictive power and his inability to understand the logic behind traditional neoclassical gravitational ways of reasoning gives him the capacity to take authors like Lucas at *his words* and point out the illegitimacy of their reasonings. On the other hand, not only does this same inability to grasp traditional gravitational modes of explanation in part explain the reasons adduced by Hahn to defend the notion of intertemporal equilibrium, but it also seems to prevent him from fully grasping the far-reaching consequences of his destructive critiques to the arguments put forward by those authors who attempt to endow modern general equilibrium theory with explanatory-predictive value: while we shall see that Hahn eventually argues that in order to be used for explanation and description equilibrium must be stable under "acceptable", i.e. time-consuming,

adjustments, he does not seem to realise what this implies, namely, that the theory must *necessarily* determine the endowments of capital goods *endogenously*, and once the illegitimacy of the value conception of capital is accepted, this entails the complete rejection of the neoclassical approach as a positive theory.

1. HAHN'S CRITIQUES OF NEO-WALRASIAN EQUILIBRIA AS CENTRES OF GRAVITATION.

In order to introduce the reader to Hahn's position on modern general equilibrium theory, in this second section I present some of Hahn's main objections to Lucas's and his followers' attempt to endow the path traced by neo-Walrasian equilibria with an explanatory-descriptive value. Essentially, Hahn takes rational-expectations scholars at their words and denounces the salient contradiction that emerges between the claim that rational-expectations theory can be used for explanation and prediction, and the alleged claim that Arrow-Debreu equilibrium is the micro foundation of that approach. Hahn does this on two main grounds: first he maintains that RATEX equilibria will generally not be unique, and the RATEX approach does not provide a *plausible* theory of equilibrium selection, that is, a *mechanism* that justifies which of the multiple equilibrium paths could be plausibly followed in real economies. Second, even assuming the uniqueness of the intertemporal path, Hahn harshly questions the resort to auctioneer-based adjustments to justify how equilibrium comes about.

1.1. Objections to the assumption of uniqueness of the full-employment path.

In order to obtain well-determinate results, rational expectations models introduce what Vercelli (1991: 100) calls a "succession of *ad hoc* assumptions"; in particular, these authors declare that the economy follows a unique full-employment path. But this result does not follow from standard Arrow-Debreu theory, on which rational expectations scholars have argued to ground their work. In "*Expectations*" (1990a), Hahn comments on Lucas (1972) contribution and declares:

Lucas believed real expectations equilibria to be unique, which is not generally the case... Certainly it is well known that Arrow-Debreu economies may have many (although, in general, denumerable) real equilibria. The same will a fortiori be true of rational expectations sequence economies. (Hahn, 1990a: 237)^[204]

And he adds that, when complete markets are missing, there might exist even a continuum of equilibria (*cf.* Geanakoplos and Mas-Colell, 1989). So, Hahn argues, even if one were to accept that rational expectations paths are susceptible of indicating actual paths with sufficient approximation^[205], in order to predict which among the possible paths will be actually realized, a theory of equilibrium selection that plausibly explains how expectations are formed and coordinated among agents becomes necessary:

It is not easy to see how a theorist can make predictions unless he provides a theory of expectation formation, i.e. a theory of how agents' expectations come to be formed. Certainly knowledge on the part of the agent of the 'real' structural equations may be quite insufficient for calculating the 'right' expectations. He would have to know something concerning the expectations of others. (Hahn, 1990a: 237)

In other words, once multiple equilibria are possible, there is no reason to assume that expectations among individuals should be shared. Therefore, if the theory is to be used for prediction and description, there must be a *plausible* reasoning that justifies how individuals coordinate their expectations so that the actual economy can follow one of the RATEX full-employment growth paths determined by theory. But these reasonings, Hahn continues, are actually missing in rational expectations literature, since to solve the indeterminacy caused by multiple equilibria, this school introduces "*ad hoc*" models with a "very special functional form" (Hahn, 1984[1982a]: 309) that eliminate the possibility of multiple equilibrium paths by assuming that the economy behaves *as if* ruled by a single omniscient consumer, the so-called 'Central Planner':

Many of the rational expectations macroeconomists have sought to sidestep these problems by supposing that the (single-good) economy behaves 'as if' a single infinitely lived agent made optimal choices in full knowledge of the real production and consumption possibilities in the economy. It is known that in this case there will, in general, be a unique optimum path to pursue. Since there is only one individual he need not consider expectations of others... It seems to me, however, that this is not so much sidestepping the problems as turning one's back on them (Hahn, 1990a: 238)

²⁰⁴ Bu 'real' equilibria, Hahn follows Pigou (1929) and means equilibria which are independent of any "psychological component", namely, independent of individuals' expectations.

²⁰⁵ As we shall see in section 1.2 Hahn disputes this claim as well.

Finally, I wish to address the following point raised by Hahn: by relying on the ‘equivalence principles’ between equilibrium and planning, rational expectations theorists reinterpret the optimal intertemporal program achieved by the Central Planner as a competitive equilibrium, and hence they argue that, “under fairly broad assumptions” (*cf.* e.g. Stokey and Lucas, 1989: 22) also free-market economies, allegedly described by the sequence of RATEX equilibria, follow an optimal growth path^[206]. However, Hahn stresses that only if there are complete markets the sequence of RATEX equilibria will be Pareto efficient:

It must by now be very widely understood that unless there are sufficient insurance (contingent) markets the resulting rational expectations equilibrium will not be Pareto efficient. (Hahn, 1990a: 245) ^[207]

And therefore, given that “Completeness of markets in actual economies seems highly doubtful” (*id.*: 246), Hahn goes on to argue that inefficient RATEX equilibrium paths due, e.g., to ‘sunspots’ or ‘self-fulfilling prophecies’ cannot be excluded (*cf.* also Shell, 2008: 85), therefore leaving “vast scope for Government intervention in the market.” (Hahn 1990a: 246)^[208]. Hahn therefore concludes that, in a world of missing markets, the reinterpretation of the Central Planner’s efficient solution as a position that the actual economy tends to realize is misleading:

All this means that we should not proceed ‘as if’ the economy were guided by a Ramsey social-welfare function being maximize subject to usual constraints if there are missing markets (Hahn, 1990a: 246).

²⁰⁶ Here I reproduce the whole passage by Stokey and Lucas in their well-known textbook: “[so far] we were concerned exclusively with the allocation problem faced by a hypothetical social planner. In this section we show that the solutions to planning of this type can, under appropriate conditions, be interpreted as *predictions about the behaviour of market economies*. The argument establishing this is based, of course, on the classical connection between competitive equilibria and Pareto optima. These connections hold under *fairly broad assumptions*.” (Stokey and Lucas, 1989: 22, emphasis added)

²⁰⁷ As an authoritative scholar like Becker writes in this connection: “Establishing the formal equivalence between optimal accumulation models and their equilibrium counterparts in many capital goods models requires the equilibrium economy to impose a transversality condition on itself, just as in the one-sector case. The general question is how is the initial price determined so that the equilibrium price profile satisfies the conditions for achievement of a Ramsey-style central planning problem. This is the crux of the Hahn problem. The modern perfect foresight interpretation is that this problem is solved whenever a transversality condition obtains as necessary for an equilibrium. This requires the household sector to be forward looking over the infinite horizon, and markets to operate on all dates for all commodities”. (Becker, 2008: 672)

²⁰⁸ The accuracy of Hahn’s claim regarding the “vast scope for Government intervention” is discussed below.

On the one hand, it seems difficult to disagree with Hahn: if Lucas and his school are taken at their words, their alleged micro foundation (Arrow-Debreu theory) does not authorize them to reason as they do, and argue a correspondence between the sequence of RATEX equilibria and observation. First, the extremely implausible and yet indispensable hypotheses required to ensure the existence of an Arrow-Debreu equilibrium (in particular, complete markets) do not even guarantee the uniqueness of the equilibrium path. And this problem will a fortiori affect RATEX equilibria too; so, lacking of a *plausible* mechanism that justifies which equilibrium path could be plausibly followed in real economies (i.e. a mechanism that does not unrealistically assume that the economy behaves *as if* ruled by a single omniscient consumer), the claim that a well-determined RATEX equilibrium will be eventually established in actual economies is devoid of legitimacy. Second, Hahn is also right in denouncing the illegitimacy of relying on the so-called ‘equivalence principles’ to argue that the actual path allegedly described by the sequence of RATEX equilibria is Pareto efficient: when complete markets do not exist (as one must realistically admit), the theoretically determined path may well be Pareto inefficient.

On the other hand I wish to make the following point, which is not often noticed: the claim that on average the economy follows a well-determined full-employment path as in the standard Ramsey problem was not really justified on the basis of a presumed coincidence of RATEX paths and Arrow-Debreu paths; albeit implicitly, it was justified on a basis which would have been much more plausible, if old neoclassical modes of explanation could be accepted. In fact, we have seen in the previous chapter that Lucas and Malinvaud do not deny that agents do not have perfect foresight. They admit that individuals make mistakes in their forecasts, and accept that they adapt their behaviour by learning and experimentation under time-consuming adjustments; their claim that the Solow-Ramsey full-employment growth path is a position that the actual economy tends to realize actually relies on traditional neoclassical ways of reasoning that in a heterogeneous-capital world must presume the conception of capital as a single scalar (measured in value), which is what ultimately justifies the action of the forces of supply and demand in the factor markets, and hence the tendency towards the full-employment of resources. Under these

bases, and disregarding possible anti-neoclassical income effects, the factor demand curves will be sufficiently elastic so that the claim that the full-employment growth path is unique and stable acquires plausibility.

Moreover, a neoclassical author who, like Lucas or Malinvaud, accepts that agents are likely to make mistakes in their forecasts, would not disagree with Hahn on the fact that, owing e.g. to expectational mistakes, inefficient outcomes are possible. However, this scholar would have good reasons to dismiss the relevance of this issue: given that over sufficient time the action of the supply and demand forces would eventually assert itself fully, the possible inefficiencies will be gradually corrected in the light of experience, exerting a relatively minor negative effect on growth. And precisely because the tendency towards the full-employment of resources would be constantly at work, this scholar would be on strong grounds to object to Hahn's (199a) claim that the absence of complete future markets "leaves *vast scope* for Government intervention": she would argue that, granted the tendency towards the full-employment of resources, the scope for Government intervention would actually be quite narrow. Of course this reasoning presumes persistent data and well-behaved capital-labour substitution, hence the indefensible notion of capital in value terms. However, the point is that granting the notion of capital as a single factor that, incidentally, Hahn (1982: 370) does consider to have some 'heuristic' value^[209], the results obtained by neoclassical theory and defended by Lucas and Malinvaud acquire plausibility. We see here why the conception of capital as a single factor (and hence in value terms) is not a "simplification" as Hahn (*id.*) has argued, but is a necessary requisite for the plausibility of the entire supply and demand approach.

1.2. Hahn's rejection of the auctioneer.

²⁰⁹ In his well known article "neo-Ricardians" (1982), Hahn (*id.*: 370) "doubts" that one-capital good models are actually "useless" (*id.*), and then he adds, "we use simple models (e.g. macroeconomics) to gain insights of a certain kind. Simplification is never without cost and the cost is sometimes loss of rigour. It remains to be shown that the cost is too high in this instance." So, even Hahn seems to hesitate to give up the picture of economic growth derivable from one-goods models; still he is not as confident as Lucas or Malinvaud –he is careful not to say that one-good models work, although he does not feel like inquiring himself into whether "the cost is too high in this instance", a revealing attitude, since it would seem to suggest that he would like to be able to accept the main insights of neoclassical theory.

Hahn also raises what seems to be a more fundamental point: it is contradictory to argue that rational expectations equilibria are grounded on neo-Walrasian theory and to claim that rational expectations paths describe actual paths with sufficient approximation, because there are no solid theoretical bases to believe that neo-Walrasian equilibrium prices and quantities –in any of their forms- will ever come about under a *realistic* adjustment process. Hahn writes:

Suppose for instance it is possible for an egg to stay standing on its tip until it is disturbed. We should not attach great practical significance to this equilibrium in the egg until we were told some causal story of how it comes to be in that state. In exactly the same way, the proposition that, in certain circumstances, there is a set of prices which ensures equality between demand and supply in all markets tells us nothing on whether these prices will indeed be established by a market economy. On this central question neither economic theory nor evidence is at all satisfactory (Hahn, 1984[1982c]: 124)^[210]

Hahn extensively discusses the problems of stability and persistence that undermine neo-Walrasian adjustments processes in an earlier article, “*Some adjustment problems*” (1970). There he first points out that sufficient conditions for stability are quite stringent conditions that, as a matter of fact, only constitute a “collection of anecdotes” (*id.*: 2); second, Hahn also notices that “the exact form of the adjustment process is important” (*id.*), that is to say, stability results strongly depend on the *kind* of adjustment process postulated by the theorist. But more important is that Hahn notices that “even when convergence can be demonstrated, the speed of convergence may *be very slow*” (*id.*: 2, emphasis added). Therefore, “even if final clearing of all markets were achieved, it would represent an equilibrium constrained by the debris of the actual groping process” (*id.*: 4). Hahn thus concludes:

[i]f one is to take the [adjustment] process as somehow representing real events, the possibility of slow convergence in the docile cases, when it can be established, is awkward for the simple reason that the no transaction requirement becomes very strained. (Hahn, 1970: 6)

The “no transaction requirement” Hahn refers to implies the assumption of the famed *auctioneer* who prevents ‘false’ production and trading from taking place, a hypothesis that amounts to assuming instantaneous equilibration. But in any case, he continues, “in recent years all these problems have been avoided by

²¹⁰ Note that by ‘theory’ Hahn clearly means neo-Walrasian theory, as if this theory were the only theory.

simply supposing the economy to be in equilibrium at every moment of time” (*id.*: 6). In other words, Hahn (1990b: 241) denounces the common practice of assuming “that in ordinary models we can treat market clearing as an axiom”. He correctly concludes,

To that extent then we seem to be prepared to live on faith. (Hahn, 1970: 6)

Hahn’s concerns seem to be well justified, since the options available to the neo-Walrasian approach are certainly not very promising. Either the neo-Walrasian analysis of stability must be carried under the unpalatable assumption of the auctioneer and hence the stability results are simply devoid of implications for real world phenomena; or, alternatively, “*prepared to live on faith*”, the theorist must directly postulate the “*axiom*” that the economy is always in equilibrium. To the extent that the correspondence between neo-Walrasian theory and observation is postulated on the basis of such fragile foundations, one cannot but agree with Hahn when, in “*What markets can and cannot do*” (1992), he argues that

It is a triumph of wishful thinking that a number of economists –mostly American- are prepared to take perfect foresight or its slightly more complex brother, rational expectations, as descriptively satisfactory. (Hahn, 1992: 5)

On the one hand then, Hahn takes modern general equilibrium theory seriously and hence he correctly argues that as long as rational expectations theorists assume the auctioneer or directly postulate the “axiom” of continuous market clearing (and perfect foresight), they are actually “prepared to live on faith”. On the other hand, we have noticed in chapter 7 that underlying e.g. Lucas’s assumption of the auctioneer there is in fact a presumption that adjustments are time-consuming and stable relative to an economy whose data actually change *relatively slowly*, and hence there emerges a faith in the connected time-consuming traditional neoclassical adjustment mechanisms and hence in the traditional notion of capital in value terms. So, while rational expectations theorists, and also Malinvaud, do have faith in the descriptive properties of general equilibrium theory, it is not because they think that the economy is constantly in equilibrium, but rather because they believe that the path traced by an intertemporal equilibrium is what the economy approximately follows because of the traditional (time-consuming) neoclassical gravitational

mechanisms. Therefore, they still have faith in those *same* persistent forces and mechanisms –whose plausibility stands or fall with the notion of capital as a single factor – as those used by the founders of the marginalist approach to argue that the economy would follow on average a full-employment growth path (with factors earning their marginal products). Now, it is true that, e.g. Lucas, the actual target of Hahn’s critiques, never admits that this is how he reasons, so Hahn is justified in accusing him –taking his statements at face value- of “being prepared to live on faith”; but Hahn never seems to realize what faith this author relies on, and what is wrong with that faith: its implicit reliance on the indefensible notion of capital as a single value magnitude.

At the risk of repetition, I wish to summarize the main conclusion of this section: while on the one hand it is difficult to disagree with Hahn that the attempts of Lucas and his school (and also of Malinvaud) to give modern general equilibrium theory an explanatory-predictive value are devoid of justification, on the other hand Hahn’s critiques to those attempts show that he does not perceive the hidden (albeit illegitimate) defensive belt: Hahn does not seem to realise that behind Lucas’s (and Malinvaud’s) arguments there actually emerge traditional neoclassical modes of explanation, whose plausibility stands or falls with the illegitimate conception of capital as a single scalar, measured in value terms. So, while Hahn is right, the fact that he proves unable to understand the logic behind Lucas and Malinvaud’s arguments suggests a lack of familiarity on the part of this author with traditional gravitational ways of reasoning.

2. HAHN’S ASSESSMENT OF NEO-WALRASIAN EQUILIBRIA.

This section examines Hahn’s reasons to rehabilitate the notion of intertemporal equilibrium. I attempt to show that the reasons he puts forward to defend this equilibrium notion do not accurately represent the history of the theory of prices and distribution, and also confirm that the author is unfamiliar with traditional neoclassical gravitational ways of reasoning, which, as will be further argued here, *still* survive in the works of some prominent contemporary neo-Walrasian scholars. It will be seen that, while Hahn is clear that neo-Walrasian equilibrium, or the sequence of these equilibria, cannot have the

explanatory-predictive role adduced by Lucas or Malinvaud, he does not envisage this issue as undermining the whole neo-Walrasian construction: neo-Walrasian equilibrium, so Hahn argues, is a Market Nash Equilibrium, a solution concept that *only* attempts to describe the mere possibility that individuals' optimal plans could be mutually compatible and hence, unlike a centre of gravitation, this equilibrium notion does not *necessarily* call for an assessment of the tendency towards equilibrium. Hahn defends his interpretation of neo-Walrasian equilibrium on three main grounds: i) that the theory of intertemporal equilibrium solves an "important" but "abstract" "two-hundred-year-old" problem that dates back to Adam Smith; in other words, that it allegedly answers essentially the *same kinds* of questions that classical and early neoclassical authors were addressing, but were unable to answer in a rigorous, satisfactory way; ii) that abstract as the intertemporal equilibrium notion is, it can in any case be used as a "reference point"; iii) that given the abstract nature of the problem, it "was always understood" by the most prominent general equilibrium theorists that intertemporal equilibrium was never meant to provide a faithful description of economic reality. To further highlight Hahn's rather peculiar position on equilibrium, the section begins with an assessment of Bliss's (1975) reaction to this issue, because in that work this author also attempts to disentangle the definition of equilibrium from the problem of the tendency towards it as if they were two isolated issues. But as we shall see, in Bliss's case the 'disentanglement' is never complete. A clear indication of this is that in his latest writings, and without justification, this author argues that the intertemporal path traced by neo-Walrasian equilibria describes actual paths with sufficient approximation.

2.1. Bliss on neo-Walrasian equilibrium.

In his book *Capital theory and the distribution of income* (1975) Bliss acknowledges that the equilibrium position determined by neo-Walrasian theory is neither necessarily stable nor persistent. He says:

[t]he stability of equilibrium is known to be problematic –there is certainly no guarantee that every, or even any, economic equilibrium will be stable. Furthermore, even if equilibrium were to be stable there might not be enough time within the space of a 'week' for prices to adjust to an equilibrium, for firms to give effect to their production plans and for exchange to be effected. (Bliss, 1975: 28)

Bliss thus suggests that, “In the face of all the foregoing problems”

[i]t may seem more sensible to simply assume that equilibrium will prevail and to thus confine our investigations to the equilibrium state. We could regard the object of our investigations not as ‘the economy’ but as ‘economic equilibrium’ (Bliss, 1975: 28)

The passage is interesting because we find Bliss arguing that due to the problems of persistence and stability, it “may seem more sensible” to postulate *ab ovo* that equilibrium prevails. The reader must notice that, in so doing, Bliss *validates* the separation between the specification of the equilibrium and the issue of the tendency towards it as if they were two isolated problems. But in any event, one seems authorized to wonder how ‘sensible’ actually it is to proceed as Bliss does, unless one attributes some usefulness to the study of equilibrium states; however, this issue is only very briefly discussed by the author later on in this book when he explains that intertemporal equilibrium theory

[d]oes not serve to represent reality and that is not its purpose. Where the simple model of an intertemporal economy with all the forward markets functioning can prove useful is as a point of departure, as a guide to which concepts are central and fundamental and which peripheral, and as a reminder that time and capital make an important difference because and only because the system of forward markets... are not in fact extant. (Bliss, 1975: 301)

While Bliss here admits that the Arrow-Debreu model “does not serve to represent reality”, he is on the contrary very vague on the supposed usefulness of the Arrow-Debreu approach: he does not explain why or how this construction can be useful as “a point of departure”; nor does he explain why this framework, admittedly unable to deal with reality, can be a guide to “fundamental” and “peripheral” concepts. Confirmation of the failure of the theory to shed light on this issue is manifested in the fact that even Bliss, one of the most attentive modern general equilibrium theorists, fails to grasp the “important difference” caused by “time and capital” in neo-Walrasian theory. The author does not seem to understand that, unrealistic as it is, the assumption of complete future markets is only a “peripheral” problem, i.e. a *consequence* of the actually *fundamental* drawback of the treatment of capital in the theory of intertemporal equilibrium: the inclusion of the capital goods’ endowments as part of its data. As we have explained in the first chapter, it is the rapid changes that the arbitrarily given endowment of capital goods will experience in the not-

very-far future, and hence the entailed changes in many relative prices –a fact that cannot be ignored by individuals- the reason why neo-Walrasian theory must explain how future price changes affect agents’ current decisions; the assumption of complete future markets is an unsatisfactory attempt to treat these expectations in an objective manner –and hence to avoid the indefinite results due to introducing subjective expectations among the data. Bliss’s failure to distinguish between fundamental and peripheral ‘theoretical choices’ may explain why he does not bother to wonder why traditional neoclassical authors did not need to rely on the unpalatable assumption of complete future markets when dealing with the issues of “capital and time”.

In any case, such vague justifications clearly indicate a continuing faith of the author in a strong usefulness of the study of equilibrium, which can only derive from a continuing faith in the neoclassical approach as indicating the forces that determine income distribution. Little wonder that, although in his earlier contributions Bliss admitted that the path trace by neo-Walrasian equilibria “does not serve to represent reality”, in one of his latest contributions, “*The theory of capital: a personal overview*” (2005), the author writes:

Simple models seem to be inadequate, although they may throw light on some important issues. We can glimpse more convincing models of the supply of capital via saving. They are quite complicated, and have sometimes not been developed explicitly. To be really insightful, it is imperative that a model should be disaggregated, although no usable model can be as disaggregated as realism would dictate. However various influences are weighted, it will be the case that saving rates, demographics and technical progress will be the grand forces driving the long-run interest rate. There is no reason to suppose that the same would not be true for better and more complicated models (Bliss, 2005: xxv)

Here the “simple models” Bliss refers to are Solow (or Ramsey) models where capital is specified as a single factor of production. The acceptance of the standard neoclassical approach to distribution is evident in the claim that the rate of interest, once the state of technical knowledge is given, depends on the savings accumulated up to them, i.e. on the supply of capital, and on demographics i.e. on the supply of labour, which evidently determine the marginal product of capital and savings. These models, Bliss argues, are useful constructions that “throw light on some important issues”, ignoring or forgetting that once the illegitimacy of the value specification of the factor capital is admitted, the whole plausibility of the neoclassical explanation of the

trends followed by prices and quantities in actual economies is at stake. So, notice that although Bliss is very aware that those “more complicated” neoclassical models where capital is “disaggregated” can only study the “economic equilibrium” and hence they must renounce to study actual (disequilibrium) economic states, he nonetheless argues without *any justification* that “there is no reason” why the predictions derived from those ‘simple’ neoclassical models, e.g., that the economy will follow on average a full-employment growth path, and hence that factor endowments and technology will be the ‘engine of growth’, will not remain valid in those ‘more realistic’ models that specify the factor capital in purely physical terms (*cf.* also Petri, 2007). In a nutshell, it emerges from the previous passage that, like Lucas and Malinvaud, also Bliss ends up envisaging the intertemporal path traced by neo-Walrasian equilibria as indicative of actual paths with sufficient approximation; in this sense, he too *falls in the temptation* to use neo-Walrasian equilibria to reach illegitimate conclusions, since these results can be plausibly derived only from traditional neoclassical ways of reasoning.

2.2. HAHN’S POSITION ON EQUILIBRIUM.

2.2.1. Neo-Walrasian equilibrium as Market Nash Equilibrium.

Let us now proceed to analyse Hahn’s position on neo-Walrasian equilibria. Hahn’s understanding of the theory of relative prices as dealt with by the neo-Walrasian approach can be summarized in three main different, but related propositions. First, we shall see that this author (Hahn 1973a, 1973b, 1984[1981], 1984[1982c]) argues that intertemporal equilibrium theory answers an “important” but “abstract” question that can be traced back to Adam Smith: “Can a decentralised economy relying only on price signals for market information be orderly?” (Hahn, 1984[1981]: 75). Second, Hahn argues that, while the theory of intertemporal equilibrium is a “false theory of how the world is like”, it can be used as a “*benchmark*” (1970, 1973a, 1973b, 1984[1982a]). Third and finally, the author asserts that the fact that Arrow-Debreu equilibrium never attempted to be a description of actual economic states “*was always understood*” (1973b). These three claims are critically discussed in the following sections.

2.2.1.1. The “abstract” problem of equilibrium relative prices.

As a first step to defend the notion of intertemporal equilibrium, Hahn notes:

That a society of greedy and self-seeking people constrained only by the criminal law and the law of property and contract should be capable of an orderly and coherent disposition of its economic resources is very surprising. Marx called such a society anarchic and so it is. Yet, ever since Adam Smith, economists have been concerned to show that such anarchy is consistent with order and indeed with certain desirable outcomes. (Hahn, (1984[1982c]): 111)

In a different article, Hahn goes on to affirm that Smith “posed an obviously important question” (Hahn, 1984[1981]: 72), and with general equilibrium theory in the version developed by Arrow and Debreu (1954) and Debreu (1959) we have “certainly arrived at an orderly destination” (Hahn, 1984[1981]: 72). In fact, Hahn continues, this theory provides

[a]n abstract answer to an abstract and important question. Can a decentralized economy relying on price signals be orderly? The answer of General Equilibrium Theory is clear and definitive: one can describe such an economy with these properties. But this of course does not mean that any actual economy has been described. (Hahn, 1984[1981]:75)

Essentially the same claim emerges in Hahn (1984[1982c]): it is there argued that the intertemporal equilibrium is a “major intellectual achievement” because

[i]t establishes the astonishing claim that *it is logically possible* to describe an economy in which millions of agents, looking no further than their own interests and responding to the sparse information system of prices only, can none the less attain a coherent disposition of resources... let me nonetheless emphasise the phrase ‘logically possible’. Nothing whatever has been said of whether it is possible to describe any actual economy in these terms (Hahn, 1984[1982c]: 114, emphasis added)

Although Arrow and Debreu do not describe “any actual economy”, Hahn affirms that they show “how the world would have to look like” if equilibrium among self-seeking individuals is to be “logically possible”; their solution apparently solves a problem that can be traced back to Adam Smith and involves a “two-hundred-year-old tradition” (Hahn, 1973b: 324).

As it can be readily seen from these passages, behind this defence of the notion of intertemporal equilibrium there seems to emerge a very *particular* reading of the history of value and distribution theory. And this reading is what seems to have prompted Hahn to interpret intertemporal equilibrium as the “abstract answer” to an “abstract and important question” that involves “a two-

hundred-year old tradition”. Little wonder then that in “*On the notion of equilibrium in economics*” (1973a) and “*The Winter of Our Discontent*” (1973b), Hahn envisages intertemporal equilibrium as a *solution concept* that only describes a situation where “the intended actions of rational individuals are mutually consistent and can therefore be implemented” (Hahn, 1973a: 3), and hence he does not find it disturbing to admit that the theory of intertemporal equilibrium

[m]akes no formal or explicit causal claims at all. For instance it contains no presumption that a sequence of actual economic states will terminate in an equilibrium state. (Hahn, 1973a: 7)[²¹¹]

In the light of these considerations, it seems sufficiently clear that, like Bliss (1975), Hahn envisages intertemporal equilibrium as a MNE, and hence he *disentangles* the problem of *equilibrium* from the study of the *causal mechanisms* that may plausibly bring this position about, as if they were two isolated issues. This is made particularly explicit in Hahn’s following remark, where he notes that general equilibrium

[i]s strong on equilibrium and very weak on how it comes about. (Hahn 1973b: 327)

The claim that neo-Walrasian theory is “strong on equilibrium” is certainly false, since in order to provide a “definite” answer to the problem of equilibrium, this theory relies on extremely unrealistic and yet indispensable assumptions such as complete futures markets. But unlike Bliss (2005) –who as seen above ends up accepting without justification that intertemporal equilibrium describes with sufficient approximation actual paths – Hahn at least attempts to be consistent with his own considerations documented in the previous section: Hahn is aware that neo-Walrasian theory cannot make causal claims at all, in particular, it is certainly unable to prove that “actual economic states will terminate in a [neo-Walrasian] equilibrium state” (Hahn 1973a: 7)[²¹²].

²¹¹ Cf. also Hahn (1972: 8), where it is argued that intertemporal equilibrium theory “has no causal significance”.

²¹² There is another passage, also cited by Garegnani (1990a), which also reveals Hahn’s methodological position regarding the relation between equilibrium and equilibration. Hahn writes: “I have said that neither Sraffa nor his followers have made anything of reswitching. By this I mean that they have continued to believe that it is damaging to neoclassical equilibrium theory which it is not and have neglected various neoclassical adjustment theories which are certainly at risk”. (Hahn: 1982b, 373).

A critical assessment of Hahn's considerations regarding the lack of any necessary relation between equilibrium and equilibration should start by stressing that behind Hahn's claims there seems to emerge a big initial misunderstanding: Hahn misinterprets both classical and early neoclassical authors' aims and scopes. In effect, for these scholars the problem of equilibrium prices^[213] was never addressed as an 'abstract question' looking for an 'abstract answer'; contrary to Hahn's claims, these scholars posed *concrete* questions, i.e. they attempted to explain the working of actual economies. And given that in actual economies production and consumption decisions take place through the market, a *concrete* answer to these concerns necessarily called for a theory of actual price formation. On sufficient time stretches, those authors argued, empirical variables would gravitate around those prices and quantities determined by theory. They were clear therefore that, for the equilibrium to be capable of having the role of a centre of gravitation of empirical (disequilibrium) magnitudes, there must be an *actual* tendency towards equilibrium.

The implication of this discussion is that, early marginalist scholars, but also the most prominent neoclassical theorists up to the 1930's, would have felt certainly troubled by Hahn's claims: They could never have envisaged modern general equilibrium analysis as being "strong on equilibrium", precisely because, as Hahn himself openly recognizes, this theory "makes no explicit causal claims at all", i.e. it does not –it cannot, actually- address the issue of the *actual* tendency towards equilibrium. In short, given that all these scholars envisaged the problem of equilibrium and the tendency towards it as two faces of the same coin (*cf.* chapter 1), they implicitly took it for granted that a *weakness* of the theory on how equilibrium comes about, if not correctible, would *necessarily* imply that the dominant forces or mechanisms selected by theory to determine equilibrium prices were not the correct ones, and that a different theory of prices and distribution must be put forward^[214].

²¹³ The reader must have in mind that for 'factors', e.g. labour, the classics had no idea of equilibrium prices.

²¹⁴ Garegnani (1990a) summarizes this vision when he objects to Hahn that he has overlooked the "essential link that exists between the analysis of equilibrium and that of the manner in which such and equilibrium is supposed to come about. It is the link which had long been taken for granted, between the relevance of the variables determined by the theory, and their nature as

It would thus seem that Hahn's first line of defence of the theory of intertemporal equilibrium is based on a big initial misunderstanding that leads him astray, and induces him to significantly change the question that the theories of prices and distribution developed by both classical and traditional neoclassical authors originally attempted to answer, namely "how do actual economies work?". Hahn's peculiar interpretation of the history of economic analysis makes him significantly change the problem and argue that intertemporal equilibrium theory provides an answer to the abstract question: "which conditions ensure the *mere possibility* that, in market economies, individuals' production and consumption plans are mutually compatible?" – which of course leaves the concrete and relevant question "are these production and consumption decisions actually implemented in real economies? And if not, which ones are?", unanswered^[215].

2.2.1.2. Intertemporal equilibrium: a false theory that can be used as a reference point.

Hahn openly admits that the extremely implausible assumptions needed to determine an intertemporal equilibrium (existence of complete futures markets, absence of increasing returns, price taking behaviour, etc) imply that Arrow-Debreu theory "*is false* as a theory of what the world is like" (Hahn, 1973b: 324, *cf.* also Hahn, 1973a:9-11). However, he does not suggest abandoning the theory, allegedly because this equilibrium notion is still a useful concept: it can be interpreted as a "*benchmark*" (Hahn 1984[1982a]: 308) or a "*reference point*" (*id.*: 309) to be compared against actual economic states. In other words, because the conditions required to determine Arrow-Debreu equilibrium are no doubt extremely restrictive, we can gain useful insights as to *why* there is no intertemporal equilibrium in reality. In this sense, Hahn for instance argues:

The Arrow-Debreu equilibrium is very useful when for instance one comes to argue with someone who maintains that we need not worry about exhaustible resources because they

centres of gravitation towards which the actual variables would tend" (*id.*: 73) Garegnani thus concludes that "Equilibrium theory can hardly continue to be separated from adjustment processes" (*id.*: 74).

²¹⁵ In a recent contribution, Eatwell (2012: 3) seems to have reached a very similar conclusion when he notices that "Unable to answer Smith's question", i.e. unable to determine prices that can act as a centre of gravitation of actually observed magnitudes, intertemporal equilibrium "has simply changed the question" (Eatwell, 2012: 3), i.e. "intertemporal equilibrium is simply the name attached to the solution of a set of equations." (*id.*)

will always have prices which ensure their ‘proper’ use. Of course there are many things wrong with this contention but a quick way of disposing of the claim is to note that an Arrow-Debreu equilibrium must be an assumption he is making for the economy and then to show why the economy cannot be in this state. The argument will here turn on the absence of futures markets and contingent futures markets and on the inadequate treatment of time and uncertainty by the construction. This negative role of Arrow-Debreu equilibrium, I consider *almost* to be sufficient justification for it... (Hahn, 1973a: 14-16, emphasis added)

Now, in the first place, the claim that intertemporal equilibrium can be used as a reference point *presupposes*, if the conditions that ensure the existence of intertemporal equilibrium (e.g. complete markets) could be somehow satisfied in reality, that this equilibrium notion could be used to describe actual economic states. But due to the other problems that affect Arrow-Debreu equilibrium, e.g. the “impermanence problem”, the “substitutability problem” or the negative results on stability, there seems to be no reason to believe that an intertemporal-equilibrium path can indicate actual paths with sufficient approximation^[216]. In the second place, notice that even Hahn here admits the weakness of this line of defence, since he admits that the benchmark role is only “*almost* sufficient justification” for the theory of intertemporal equilibrium, i.e., it is *not* a fully sufficient justification for it. The implication is that Hahn must still find another *independent* reason that justifies the usefulness of the theory. But Hahn does not provide any other valid justification. Indeed, Hahn’s final argument appears to be the following:

Debreu and others have made a significant contribution to the understanding of Keynesian economies just by describing so precisely what would have to be the case if there were to be no Keynesian problems. (Hahn, 1973a: 34)

But the claim is surprising because incorrect and liable to objection on the basis of Hahn’s own previous statements in that and other articles. Hahn’s claim makes sense only if one takes it for granted that, if the assumptions of Arrow-Debreu theory are satisfied, intertemporal equilibrium will *be effectively* established in the economy. And yet, in the 1973a article Hahn has insisted that Arrow and Debreu (1954) only showed the conditions under which an

²¹⁶ The claim that an intertemporal equilibrium can be used as a benchmark has been also criticised by Currie and Steedman (1989) and Srepanti and Zamagni (1995) among other scholars on the grounds that, extremely stringent as they are, the conditions assumed to have an intertemporal equilibrium are *sufficient*, yet *not necessary* conditions. So, e.g. the fact that in actual economies production usually exhibits increasing returns to scale, or that individual preferences are not convex, does not in *itself* imply that equilibrium will not exist.

intertemporal equilibrium *exists* (cf. e.g. Hahn, 1973a: 8). But existence of a vector of prices and quantities such that all present and future markets clear does not in the least rule out ‘Keynesian problems’, since these concern stability above all: when criticising the neoclassical approach, Keynes does not attempt to deny that there might be a position of full employment ‘equilibrium’; granting its existence, Keynes objects to the alleged capacity of the forces of supply and demand to enact a tendency towards such equilibrium. The quote is also interesting because it evidences that even for Hahn, who constantly attempts to disentangle the problem of equilibrium from the issue of how it comes about, it is sometimes difficult to do so^[217].

2.2.1.3. Was it really “always understood” that Arrow-Debreu is not a description of reality?

The third argument put forward by Hahn to defend his particular vision of intertemporal equilibrium as a MNE with no explanatory-predictive value is that

[i]t was *always understood* [that] the equilibrium of Arrow-Debreu is not a description of an actual economy. (Hahn (1973b: 329, emphasis added)

I.e. intertemporal equilibrium was *never* meant to be a centre of gravitation of actually observed variables. Hahn ironically wonders in this connection:

Now one of the mysteries which future historians of thought will surely wish to unravel is how it came about that the Arrow-Debreu model came to be taken descriptively (Hahn, 1984[1982a]: 308-309)

But is it really the case that the most attentive general equilibrium theorists “always understood” that the notion of intertemporal equilibrium is not a description of reality? Let us try to unravel this apparent “mystery”. In the first place our examination of the first generation of neo-Walrasian scholars seems to firmly contradict Hahn’s claim, since we have seen that all these authors do

²¹⁷ A similar slip appears in the following statement by Hahn: “When the claim is made... that a myriad of self-seeking agents left to themselves *will lead* to a coherent and efficient disposition of economic resources, Arrow and Debreu show what the world would have to look like if the claim is to be true.” (Hahn, 1973b:323, emphasis added). Note that here again there seems to emerge the idea that intertemporal equilibrium theory provides, not only an answer to the problem of the ‘existence’ of equilibrium, but it also shows the conditions under which self-seeking individuals “left to themselves”, i.e. interacting in markets, *will eventually reach* this equilibrium.

have recourse to neo-Walrasian general equilibrium theory in a positive way, that is to say, to explain how real economies actually work. Moreover in chapter 7 we have also shown that even one of the most important exponents of the paradigm, Edmond Malinvaud, does not share Hahn's view (and the same thing could be said about Bliss, *cf.* section 2.1). But closer inspection seems to reveal that even Arrow, and to some extent Debreu, have both envisaged their construction as a description of the fundamental forces at work in reality. For instance, in a recent contribution, “*Existence*” (1998), Debreu has argued:

The theory of general equilibrium is centered on the concept of a price vector satisfying this condition [market clearing], fulfilling one of the essential functions of prices by equating demand and supply on every market. It represents that concept as an explanation of the prices observed in an economy at a given time. (Debreu, 1998: 21)

It seems clear enough that Debreu sees general equilibrium theory as much more than an abstract benchmark, since in his view explains those prices “observed in an economy at a given time”. As a matter of fact, Debreu goes on to remark that “if the model that has been specified requires strong assumptions to guarantee existence of an equilibrium price vector, the explanatory power of the theory will be low” (*id.*), and in this sense, he concludes:

The simple question initially raised in this chapter has received an answer in the form of a theorem asserting the existence of a general equilibrium for a model of the economy. The weakness of the assumptions of the theorem is in direct relation to the power of the model in explaining economic reality. How weak these assumptions might turn out to be was not anticipated. (Debreu, 1998: 33)

So, given that Debreu believes that the assumptions of the Arrow-Debreu approach are sufficiently weak, the model is accordingly powerful in explaining reality. In so arguing, Debreu ignores or forgets that, besides the mythic auctioneer^[218], equilibrium in production assumes the existence of complete future markets, which do not exist in reality. In the light of these remarks it thus seems clear that, Hahn's claims to the contrary notwithstanding, for Debreu neo-Walrasian equilibrium provides a ‘powerful explanation’ of how real market economies work.

²¹⁸ Incidentally, it is Debreu himself and Arrow who, in their 1954 contribution, appear to have introduced the auctioneer in an explicit way probably for the first time in economic theory. In that work, the auctioneer is called the “market participant”, who is a “fictitious agent that chooses prices”, and whose behaviour attempts to mimic the “classical law of supply and demand” by increasing (decreasing) the prices of goods in excess demand (supply) (Arrow and Debreu, 1954: 274-275).

The claim that for the most prominent neo-Walrasian scholars intertemporal equilibrium can be used in a positive way can be strengthened further by examining some passages of Arrow's "*General Economic Equilibrium: Purpose, Analytic Techniques, Collective Choice*" (1974). In this contribution, while on the one hand Arrow admits that

The balancing of supply and demand is far from perfect. Most conspicuously, the history of the capitalist system has been marked by recurring periods in which the supply of available labor and of productive equipment available for the production of goods has been in excess of their utilization, sometimes, as in the 1930's, by very considerable magnitudes. (Arrow, 1974: 253)

He on the other hand asserts:

Nevertheless, when all due allowances are made, the coherence of individual economic decisions is remarkable. As incomes rise and demands shift, for example, from food to clothing and housing, the labor force and productive facilities follow suit. Similarly, and even more surprising to the layman, there is a mutual interaction between shifts in technology and the allocation of the labor force. As technology improves exogenously, through innovations, the labor made redundant does not become permanently unemployed but finds its place in the economy. It is truly amazing that the lessons of both theory and over a century of history are still so misunderstood. On the other hand, a growing accumulation of instruments of production raises real wages and in turn induces a rise in the prices of labor-intensive commodities relative to those which use little labor. All these phenomena show that by and large and in the long view of history, the economic system adjusts with a considerable degree of smoothness and indeed of rationality to changes in the fundamental facts within which it operates. (Arrow, 1974: 253-254)

The previous passages are particularly relevant for two main reasons. First, because it is clear enough that the equilibrium, which no doubt Arrow identifies with Arrow-Debreu equilibrium, *does play* an explanatory role. Second and perhaps more surprisingly, because Arrow, in order to explain how the "remarkable coherence of individual economic decisions" actually takes place, envisages time-consuming adjustments, and explains the tendency towards equilibrium in the factor markets by having recourse to the direct and indirect factor substitution mechanisms in terms of labour and "instruments", the latter clearly seen as embodiments of quantities of a single factor since he speaks of "accumulation of instruments" for what on the contrary generally represents a nearly total change in the types of capital goods existing. He speaks of this accumulation of 'instruments' as raising the marginal product of labour, and he speaks of labour intensity of products as if ascertainable through the simple ratio of labour to 'instruments' and independent of prices. The avoidance of the word 'capital' (probably due to the fact that at that time the results of the

Cambridge controversies had already taken root in a considerable part of the economics theorists) is not enough to hide the fact that the logic of the argument implies the possibility of treating the different capital goods as representing embodiments of a single factor capital of variable ‘form’, whose ratio to labour can be treated as ascertainable independently of income distribution, and whose increase allows one to speak of ‘accumulation’. In other words, the argument assumes the traditional conception of capital, which is also what authorizes the admission of time-consuming adjustments tending to definite results. Like Lucas and Malinvaud (*cf.* chapter 7) then, Arrow still believes in the old neoclassical adjustment mechanisms based on capital the single factor, and it is this belief that makes him attribute to general equilibrium a positive, descriptive role, showing in all this less clarity than Hahn on the difficulties of justifying those mechanisms on the basis of the kind of general equilibrium he has formalized^[219].

Consider also what Arrow writes when he attempts to describe the policy implications of the Second Welfare Theorem. He says:

General competitive equilibrium above all teaches the extent to which a social allocation of resources can be achieved by independent private decisions coordinated through the market. We are assured indeed that not only can an allocation be achieved, but the result will be Pareto efficient. But, as has been stressed, there is nothing in the process which guarantees that the distribution be just. Indeed, the theory teaches us that the final allocation will depend on the distribution of initial supplies and of ownership of firms. If we want to rely on the virtues of the market but also to achieve a more just distribution, *the theory suggests* the strategy of changing the initial distribution rather than interfering with the allocation process at some later stage. (Arrow, 1974: 269, emphasis added)

Clearly there is here the presumption that the spontaneous action of the market forces of supply and demand will allow the equilibrium corresponding to the new distribution of endowments to be established and, moreover, that this more “just” equilibrium will only depend on the new configuration of the data, that is to say, that it will be independent of the details of disequilibrium productions and transactions. This is, in Arrow’s view, what “the theory suggests”. This is a clear refutation of Hahn’s statement: even in the years when Hahn was putting

²¹⁹ In this respect, Petri (2004, chapter 5, appendix 5.A.2) has noticed that in his discussion of the problem of price taking (Arrow, 1959), Arrow in fact does not seem to be very clear that traditional ways of reasoning are illegitimate when applied to modern versions of general equilibrium theory.

forward that claim, it *was not* always understood, not even by famous neoclassical theoreticians, that Arrow-Debreu equilibrium was not meant to be a representation of actual economic states.

The main conclusion that comes out of this section is that, in view of the evidence here provided, Hahn's claims that i) intertemporal general equilibrium is only an "abstract answer" to an equally "abstract question" that has a "two-hundred-year-old tradition" and that ii) Arrow-Debreu equilibrium must be seen as a "benchmark", reveal, at a general level, a clear inability on the part of Hahn to understand the history of price and distribution theory. But at a more concrete level, they also reveal a lack of understanding of the aims of those prominent contemporary neoclassical economists who develop general equilibrium theory during the second half of the 20th century (e.g. Debreu, Arrow and Malinvaud); contrary to what Hahn declares, these authors do develop the theory of intertemporal equilibrium to explain the trends in prices and quantities, namely, they attempt to determine a centre of gravitation of actually observed variables. Moreover, by ignoring or overlooking passages like those by, e.g., Arrow, Hahn also misses the opportunity to notice the central role, in those passages, played by the traditional notion of capital as somehow a single factor of variable form.

2.2.2. Equilibrium requires "reachability".

I wish to make a final qualification regarding the relation between equilibrium and equilibration in Hahn's work. When Hahn comes to indicate what kind of equilibrium would have the explanatory, positive role that is wrongly attributed to Arrow-Debreu equilibrium, one finds in him a very interesting admission. In a passage of "*History and economic theory*" (1991), Hahn writes:

[e]quilibria which cannot be reached from historically given initial conditions by an acceptable process of learning should, I contend, be ruled out. What that means is that the equilibrium definition *should include* the requirement of *reachability*. This is not an extravagant proposal. An egg may just have an equilibrium standing on edge but that is not of interest to the grocer (although I do not deny that it may be of mild interest to magicians and physicists)... All this may be summed up by saying that economic theory

should deal with equilibria which are *stable* under some *acceptable* process. (Hahn, 1991: 70-71, emphasis added)[²²⁰]

I completely agree with Hahn here: equilibria which are not reachable by “acceptable”, i.e. realistic, adjustments “should be ruled out”. But, contrarily to what Hahn apparently believes, his proposal is not “extravagant” at all. It seems enough to recall that this is precisely what Wicksell (1954, 1934) and Walras (1954) had both argued 100 years before Hahn. Yet, given that Hahn now claims that “the equilibrium definition should *include* the requirement of reachability”, one would also expect Hahn to reconsider his identifying Arrow-Debreu solution as a proper ‘equilibrium’ solution. More generally, one would expect Hahn to abandon the attempt to determine equilibrium states on the basis of a *given* endowment of capital goods. Indeed, any “acceptable process” of adjustment must be time-consuming and therefore the position to which the adjustment converges cannot be defined and considered ‘stable’ on the basis of a vector of capital endowments known before the adjustment has been completed. In other words, as openly admitted by traditional neoclassical scholars, the vectorial endowments of capital goods must be endogenous variables if equilibrium is to be reached by acceptable, time-consuming adjustments. But given that in neoclassical theory this condition can be only satisfied by specifying the capital endowment of the economy as a *given* amount of value[²²¹], which is unacceptable, the solution to the problem of equilibrium necessarily calls for a different theory of prices and distribution than the neoclassical one.

Hahn however seems unable to conceive of an equilibrium notion where the endowments of capital goods are not given but determined by the conditions of

²²⁰ In Hahn (1970) it is already admitted that “it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about.” (Hahn, 1970: 1). A very different position is taken for instance by Arrow (1974b) when discussing the notion of intertemporal equilibrium: “In its most formal statement, we simply use for analysis the equilibrium conditions of the individual agent and of the market, without inquiry as to how they come to hold. Yet even these statements turn out to yield revealing insights in the workings of resource allocation. Why have medical costs risen so rapidly relative to other prices since 1967? The upward shift in demand due to Medicare and Medicaid with a price-inelastic supply of physicians and hospitals provides a simple straightforward answer”. Moreover, this statement further disproves Hahn’s claims regarding the status of Arrow Debreu equilibria, namely, that this notion was thought to be useful for explanation of real word events.

²²¹ The reasons why the quantity of capital of the economy cannot be also endogenously determined if the adjustment process is to be “acceptable”, as Hahn requires, have been explained in the first chapter.

equilibrium. And this may help us understand why, as we have first seen when we assessed Hahn's critiques to the RATEX school in section 1, and we more thoroughly discuss in the remainder of this section, he also proves unable to grasp the relation that *necessarily* emerges between the notion of equilibrium as a centre of gravitation in neoclassical theory and the traditional conception of capital, the single factor in value terms. This apparent inability emerges in several instances of Hahn's work. E.g., both in his "*Revival of political economy: the wrong issues and the wrong argument*" (1975) and then in "*The neo-Ricardians*" (1982), Hahn mistakenly identifies those prices that yield a uniform rate of return on the supply prices of the capital goods with the constancy of prices over time within an intertemporal framework, i.e. with prices that yield the same own-rates of profit (*cf.* Garegnani 2011, appendix II for a detailed analysis of the notion of own-rate of interest and its relationship with the traditional notion of uniform rate of profits). Hahn thus says:

General equilibrium theory is general and so can discuss the equilibrium of an economy whatever its 'initial conditions', *e.g.* outfit of goods inherited from the past. For most such specifications it will not be the case that the equilibrium price of a good for future delivery in terms of the same good for current delivery will be the same for all goods. (Hahn, 1975: 360)

As is well known, only under the stringent condition that relative prices do not change over time the equilibrium price of a good for future delivery *in terms of the same good*, i.e. its own-rate of interest, will be the *same* for *all* goods. That is why Hahn concludes:

The crudest empirical observations will convince one that there is no unique rate of profit to be observed in the economy. Do we conclude from that that competition is functioning badly? Answer: No. Consult any general equilibrium text. Why is 'the rate of profit' an interesting unknown? Ask the neo-Ricardians. If we knew all relative prices from now to doomsday, could we ask more? (Hahn, 1975: 361)

Note first that in this passage Hahn betrays a contradiction. While he insistently argued that Arrow-Debreu theory can only have a *negative* role (*cf.* section 2.2.1.2 above), we now find him relying on intertemporal equilibrium theory to reach *positive* conclusions: this theory would be able to provide an explanation of why the actually observed own-rates of profits are not necessarily uniform in the different businesses, without this fact implying the absence of free competition. In any case, it is true that "crudest empirical observations" reveal that own rates of interest are not uniform: first, because the constant accidental

events that may affect the economy at any given time imply that empirically observed rates of profits are disequilibrium rates; second, because the underlying given conditions will change over time and hence, even granting the correctness of neoclassical theory, these equilibrium relative prices will be modified accordingly. However, it is precisely the impossibility to know “all relative prices from now to doomsday”, that is to say, to determine with sufficient approximation how market prices will evolve over time in the further future, what seems to have prompted traditional neoclassical authors to determine as equilibrium prices normal relative prices, i.e. prices that yield a uniform return on the capital goods’ supply prices. On the one hand, these magnitudes are sufficiently *persistent* with the further consequence that it is possible to neglect future price changes without necessarily condemning the theory to studying secular stationary positions, or the equivalently unrealistic positions of steady growth. On the other hand, the error involved when assuming constant relative prices over time, neoclassical –and also classical– authors correctly argued, is actually *negligible*. For instance, if we concentrate on normal prices in traditional neoclassical theory, these theoretical magnitudes will change very slowly over time due to different growth rates of labour supply and of the value endowment of capital. In a nutshell, the relative persistence of normal prices –and hence of the assumed causes that determine them– helps clarify Hahn’s concern, i.e. why the uniform rate of profit in traditional theory actually *is an interesting* unknown.

Now, in order to be satisfied, the uniformity-on-the-supply-prices-of-capital-goods condition^[222] requires an endogenous determination of the composition of the capital stock and within a neoclassical framework, a value specification of the capital endowment. Essentially, then, this is the main theoretical reason why Wicksell and the bulk of neoclassical authors up to the 1930’s relied on a value endowment of capital, a reason that appears to be independent of the alleged aim to determine a uniform own-rate of profit, as Hahn mistakenly believes. The uniformity of the own-rates of interest emerges only as a *consequence* of the

²²² In a context of changing relative prices, the relevant equilibrium condition is the uniform effective return on the supply prices of the capital goods (*cf.* Garegnani, 2003 appendix, 2005 for the notion of uniform rate of return on the supply prices of the capital goods when capital goods’ prices are changing over time).

persistence of the equilibrium determined, since this persistence allows abstracting from the possible changes that equilibrium prices may experience over time.

But Hahn proves to be equally unable to grasp the other roles played by the endowment of capital in value terms. This is clear when he writes:

Why do people balk only at aggregation of machines and not of people? (Hahn, 1975: 364)^[223]

Independently of the above-discussed reason regarding the uniform return on the supply prices of the capital goods (and hence the determination of a persistent, not stationary, equilibrium), the insistence by the critics on the need to conceive the several endowments of capital goods as embodiments of a single quantity involves a second reason: as explained in the first chapter, the principle of substitution that constitutes the foundation of the elasticity of the factor demands curves, and hence, of the plausibility of the explanation of distribution in terms of supply and demand forces, necessarily needs a given endowment of capital in value terms capable of changing form without changing quantity. When each capital good is considered as an independent factor, the possibilities of substitution will certainly be very narrow, and hence an implausibly low level of wages, probably zero, can emerge in equilibrium (or possibly so high a wage as to reduce all other gross incomes to nearly zero). This issue, no doubt, will likely jeopardize the supply-and-demand explanation of distribution by depriving it of plausibility. As Hahn himself acknowledges in his 1971 work with Arrow, the equilibrium must be economically “sensible”. And they explain:

By ‘sensible’, of course, we can mean all sorts of things. Certainly, though, we should not be much interested in an equilibrium with a zero real wage. (Arrow and Hahn, 1971: 354-355, emphasis added)

²²³ The same misunderstanding appears in Bliss (1975: 147), when he argues: “The conditions for general capital aggregation are identical to the conditions for the aggregation of labour, or of output”. However, in the very beginning of the book Bliss (*id.*: 8) does admit that “capital cries out to be aggregated”. It would have been very useful that Bliss had attempted to justify this claim.

3. GENERAL CONCLUSION: NEO-WALRASIAN EQUILIBRIUM AS MNE, OR THE GRADUAL ABANDONMENT OF TRADITIONAL GRAVITATIONAL WAYS OF THINKING.

In this last chapter I have examined Hahn's position on neo-Walrasian equilibrium and have compared it with the views of Lucas and Malinvaud (and although more briefly, with Bliss's, Arrow's and Debreu's), who argue that modern general equilibrium can be used for explanation and prediction of real world events. Hahn's position is rather unique: perhaps more than any other neo-Walrasian scholar, Hahn seems to be very much aware that due to the problems of stability and persistence (and non-existence of futures markets) the intertemporal path traced by neo-Walrasian equilibrium prices cannot be conceived as a centre of gravitation of actual paths (section 1). Hahn concludes that this equilibrium must be interpreted as a Market Nash Equilibrium: a solution concept that *only* shows the conditions under which optimal plans among self-seeking individuals can be mutually compatible; in so doing, his argument goes, it only provides a benchmark, an "abstract answer" to an equally "abstract question". Hahn defends himself by arguing that i) Arrow-Debreu theory solves a two-hundred-years-old problem, i.e. it solves relevant problems and questions that e.g. Smith, Ricardo and Marx, but also early neoclassical scholars were all raising, but were unable to answer in a rigorous, satisfactory way; and that ii) the certainly narrow aims and scopes of intertemporal equilibrium theory were "always understood" by the most attentive general equilibrium theorists (section 2).

However, Hahn's position on the notion of equilibrium appears to be hardly defensible, and reveal a lack of familiarity with the logic of traditional neoclassical theory, and with its aims and scopes. In fact, in his attempt at rehabilitation of modern general equilibrium theory, Hahn *disentangles* the relation between equilibrium and equilibration that was largely taken for granted by traditional neoclassical scholars in order to defend the plausibility of the marginalist approach to prices and distribution (*cf.* chapter 1). But we have argued that Hahn's arguments also misrepresent the aims of modern general equilibrium theorists. In this connection, we have shown in previous chapters that neo-Walrasian theory was built and erected during the 1930's and 1940's in

the attempt to explain the trends observed in actual economies. So, among the ‘first generation’ of neo-Walrasian scholars, Hayek (chapter 2) does have recourse to the intertemporal path with the aim to explain how real economies work; but also Lindahl (chapter 3) and Hicks (chapter 4), who are apparently more careful and e.g. admit that neo-Walrasian theory must rely on the “highly unrealistic” assumption that the economy is *always* in equilibrium (and point out the possible *indefinite* outcomes owing to the inclusions of expectation functions among the data in the TGE versions of the theory), in any case fundamentally accept that the sequence of prices and quantities traced by neo-Walrasian equilibria would describe with sufficient approximation the average path followed by actual economies. Even Lange (chapter 5), who uses the notion of temporary equilibrium in an attempt to object to the tendency to full-employment, never attacks the neo-Walrasian notion of equilibrium on the grounds of being an unrealistic construction; recall for instance that, unlike Hicks, Lange does not feel the need to assume instantaneous equilibration. True, Lindahl, Hicks and Lange essentially study the economy through sequences of temporary equilibria, which do not carry with them the unrealistic hypotheses of perfect foresight or complete future markets. But like perfect-foresight or intertemporal equilibria, temporary equilibria cannot do without the equally absurd assumption of *instantaneous* equilibration that Hahn so fiercely objects to. The only author among the scholars examined all along this dissertation who seems to more firmly object to the intertemporal equilibrium as a realistic and hence acceptable representation of reality is Allais (chapter 6). But this is precisely why the French scholar ends up abandoning the neo-Walrasian framework and attempts to explain the main determinants of prices and distribution on a different basis, i.e. by relying on the notion of secular equilibrium. Finally, and perhaps more importantly, we have indeed shown in this last chapter and also in the previous one (chapter 7) that, besides Lucas, general equilibrium specialists like Malinvaud, Bliss, Debreu and Arrow all appear to envisage modern general equilibrium theory as a plausible device for assessing a correspondence with observation. This overwhelming evidence clearly contradicts Hahn’s reconstruction of the history of economic theory.

Closer inspection reveals that Hahn's mistaken reconstruction of the history of the theory of prices and distribution appears to be only a symptom of his particular, rather unique position on the notion of intertemporal equilibrium, that can be summarized in the following way: on the one hand, both Hahn's clearer perception that Arrow-Debreu equilibrium is a MNE and his inability to understand traditional gravitational ways of reasoning give him the capacity to take contemporary scholars like Lucas at their words and point out the illegitimacy of their reasonings. On the other hand, while Hahn (1991) eventually admits that any sensible notion of equilibrium must "*include* the notion of reachability", i.e. must "be stable under some acceptable process", where "acceptable" implies getting rid of the auctioneer, etc., this same impossibility to grasp traditional reasonings impedes him to fully understand the far-reaching consequences of his destructive critiques: equilibria that are reached by "acceptable processes" *require* the endowments of capital goods to be determined by the conditions of equilibrium; the implication is that an *essential* and *unavoidable* link emerges in neoclassical theory between the notion of equilibrium as a centre of gravitation and the value specification of the capital endowment. Given that the notion of capital in value terms is actually indefensible on theoretical grounds, Hahn does not understand that the implication of the requirement of "reachability" is that the forces that determine prices and distribution in actual economies must be other ones than those postulated by neoclassical theory, i.e. that a positive theory of prices and distribution forcedly requires the neoclassical approach and its supply-and-demand logic to be rejected and replaced by an alternative theory.

Considering that Hahn seems unable to realize that the capital goods must be endogenous variables if equilibrium is to be reached by time-consuming adjustments, it is easier to understand why he also proves unable to grasp the nature of the relationship between equilibrium and the value endowment of capital in neoclassical theory. Confirmation of this comes from the fact that not only does Hahn ignore or forget the roles that the value-conception of the capital endowment played in traditional neoclassical theory, but more important for our present purposes, he also overlooks the essential role still played by this notion in the theoretical constructions examined in this

dissertation. In this connection, we have seen that, while in the modern versions of general equilibrium theory the capital endowment of the economy is specified as a collection of capital goods, the notion of capital, the single quantity of variable 'form', still reappears as *a flow* in the savings-investment market: decreases in the rate of interest brought about by an increase in savings are still believed to induce the adoption of more capital-intensive methods of production, hence the tendency of investment to adapt to the full-employment level of savings (the notion of capital reappears also in the *results* of the flow: more flow of savings means that afterwards, labour will have a higher marginal product –not a guaranteed result unless more value of savings means in some sense more capital afterwards in spite of the changed capital goods). On the same footing, we have also seen that the bulk of contemporary neoclassical scholars still accepts that when labour supply increases, the gradual fall in wages over the sequence of neo-Walrasian equilibrium prices will induce entrepreneurs to adopt techniques that require more labour per unit of output without need for additional savings. In other words, underlying these kinds of reasonings there is an *illegitimate* attempt to justify traditional results on the basis of modern general equilibrium theory. Had Hahn been able to grasp the continuous re-emergence of the value notion of capital in neo-Walrasian general equilibrium theorists, and why this must necessarily be the case in order to conceive of neo-Walrasian equilibria as indicative of actual paths, he would also have realized why, contrary to his opinion, it *was not* always understood that neo-Walrasian theory was only aimed to be a simple benchmark, and hence why the first generation of neo-Walrasian scholars, but also the second generation, who finds in Arrow, Debreu, Malinvaud and Lucas some of its most representative scholars, still seems to share the belief that the sequence of equilibrium prices and quantities determined by modern versions of general equilibrium theory are able to describe the average trends of actually observed variables.

Our examination of Hahn's reflexions on the notion of equilibrium reveals that this author appears to be the final outcome of a process of gradual abandonment and forgetfulness of traditional gravitational modes of explanation, and it is because of this that he is unable to continue to believe that

behind the neo-Walrasian equilibria there are, all the same, traditional neoclassical gravitational processes that render these equilibrium paths indicative of the actual paths.

The overall conclusion appears to be the following: the gradual acceptance of the implications of the new MNE theoretical framework brings one inevitably to Hahn: to the admission, on the one hand, that neo-Walrasian equilibria are MNEs with no explanatory-predictive value and, on the other hand, to the rediscovery of the fact that one needs to determine *reachable* positions of rest in order to have a theory with explanatory-predictive value— but without a clear understanding of what this implies because the history of theory is by now misunderstood and the traditional notion of ‘normal’ or ‘long-period’ position is lost.

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