

Economic Ontology and Business Cycles Theories:
An inquiry into the capital theory foundations of real business cycles theory

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Biographical note

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In 2014, he enrolled in the PhD program in Economics, also at FEP. In 2017, he spent one term at the Faculty of Economics, University of Cambridge, as a visiting PhD student under the sponsorship of Professor Tony Lawson. There he joined the workshops and other related activities of the Cambridge Social Ontology Group.

Since 2016, he has occasionally lectured Economics and Management undergraduate students at FEP on the European and Portuguese economies as well as on economic growth theory.

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Abstract

The framework that most widely influences business cycles research is the ‘new neoclassical synthesis’, the core of which is the real business cycles model. One of the building blocks of this model is a version of the traditional neoclassical theory of capital. When the real business cycles paradigm was being forged, however, another neoclassical framework for capital was available, developed in the context of general equilibrium theory. Thus, one may be tempted to ask: why was one particular theory of capital preferred in real business cycles theory?

This question leads us further than might *prima facie* be expected. In the ‘60s, roughly twenty years before the emergence of the real business cycles paradigm, a debate on capital theory – the Cambridge controversies, opposing neoclassical to heterodox economists – established that the traditional neoclassical theory of capital had serious flaws. Nevertheless, that theory survived the debate, which seems to have been forgotten by mainstream economists. Thus, despite there being an alternative, the real business cycles paradigm was built on an admittedly discredited theory of capital – a theory that neoclassical economists themselves recognized to be flawed. How can it be that, though an alternative was available, an admittedly flawed theory became central in mainstream business cycle theorising?

In answering these questions, this thesis argues that (1) the lack of impact of the Cambridge capital controversies constitutes a puzzle; and that (2) this puzzle becomes intelligible once the role of implicit and under-articulated ontological presuppositions in the controversies is unveiled. Although neoclassical economists initially recognized the problems in the traditional theory of capital, eventually they dismissed them. The ontological questions behind the controversies are central in accounting for this turnaround, and ultimately, for explaining the survival of the traditional neoclassical theory of capital, hence its widespread influence on the real business cycles paradigm.

Resumo

O paradigma que mais influencia a investigação sobre ciclos económicos é conhecido como a ‘nova síntese neoclássica’, cujo núcleo é o modelo de ciclos de negócios reais. Um dos elementos deste modelo é uma versão tradicional da teoria neoclássica do capital. Contudo, aquando da emergência do paradigma dos ciclos de negócios reais, existia, dentro pensamento neoclássico, uma outra forma para lidar com o capital, que tinha sido desenvolvida no contexto da teoria do equilíbrio geral. A pergunta é, assim, inevitável: havendo mais do que uma teoria do capital disponível, como explicar a escolha feita aquando edificação da teoria dos ciclos de negócios reais?

Esta questão leva-nos mais longe do que se poderia inicialmente suspeitar. Nos anos 60, aproximadamente vinte anos antes do aparecimento do paradigma dos ciclos de negócios reais, um debate sobre a teoria do capital – as controvérsias de Cambridge, que opôs economistas neoclássicos a heterodoxos – estabeleceu que as versões tradicionais da teoria neoclássica do capital apresentavam falhas relevantes. No entanto, essas versões da teoria sobreviveram ao debate, o qual parece aliás ter sido esquecido pelos principais economistas. Assim, apesar de existir uma alternativa, o paradigma dos ciclos de negócios reais foi construído sobre uma teoria do capital reconhecidamente desacreditada – uma teoria que os próprios economistas neoclássicos reconheceram ser defeituosa. Como é possível que, não obstante a existência de uma outra alternativa, uma teoria reconhecidamente problemática tenha sido colocada no centro da teorização neoclássica dos ciclos económicos?

Ao responder a estas perguntas, esta tese argumenta que (1) o fraco impacto das controvérsias de Cambridge constitui um enigma; e que (2) esse puzzle se torna inteligível quando o papel dos pressupostos ontológicos implícitos e não completamente articulados das controvérsias é clarificado. Embora os economistas neoclássicos tenham inicialmente reconhecido os problemas das formulações tradicionais da teoria do capital, estes acabaram por ser desconsiderados. As questões ontológicas envolvidas nas controvérsias são fundamentais para explicar essa reviravolta e assim, para explicar também a sobrevivência das versões tradicionais da teoria neoclássica do capital, bem como a sua influência generalizada no paradigma dos ciclos de negócios reais.

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“It is what you read when you don't have to that determines what you will be when you can't help it”.

(attributed to) Oscar Wilde

1 Introduction

Reading a book can change one's life journey. Back in 2009, I read the “*The New Paradigm for Financial Markets: The Credit Crisis of 2008 and What It Means*” by George Soros (2008). At the time the book was written, the subprime crisis was at its climax, fostering the idea, both among the public and among some economists, that economics had failed (Colander et al., 2009). For it was incapable of predicting, and of prescribing convincing remedies for, the emergent crisis. Soros criticized what he understood to be the state of economics, especially financial theory, and sketched a way forward. Thus, my endeavour to understand the crisis exposed me to a critique of mainstream economic theory. The pieces of economic theory that had been presented to me up until that point had always seemed so plain and logical that I had never dared question them. For a naïve student of economics, Soros's critiques and arguments were as shocking as they were inspiring.

That book changed me. Not only did it arouse my curiosity about criticisms of economic theory, but it also fuelled my interest in the study and explanation of crises. Parallel to my formal study of economics as an undergraduate student, I started to read economists absent from the curricula of the courses I was being taught. Authors in the Austrian tradition, especially Ludwig von Mises and Friedrich von Hayek, which were to be discussed only later in my undergraduate degree, in the History of Economic Thought course, were the ones that I found most persuasive: in particular, the Austrian business cycle theory and the Austrian theory of capital. Still as an undergraduate student, I came across Tony Lawson's “*Economics and Reality*”, which was part of the reading list of History of Economic Thought. I found this book highly persuasive in its diagnosis of mainstream economics. It was then that I was first introduced to a form of study especially concerned with the nature of reality. Ontology, as that form of study is called, is often not systematically addressed, or indeed completely neglected, but it remains, so the book's argument goes, essential for a relevant economics. Since reading Lawson's book,

ontology has not only been at the heart of my interests in economics, but it has also changed the way I read or interpret economic texts. Quite often, I find myself wondering about the ontological beliefs that inform the author I am reading, even if I am doing it in a loose manner.

It was with this mindset that I read the writings of Piero Sraffa. First, I looked at his critiques of Hayek's early business cycle theory (Sraffa, 1932), but then I discovered his critiques of Marshallian economics (Sraffa, 1926), and later his contributions to capital theory (Sraffa, 1960). All these readings were not part of a purposefully defined reading list whose outcome would be an idea that would help me in my doctoral thesis. Yet, as it happens, those readings did trigger the reflections that would eventually lead me to the topic of this thesis.

My thesis focuses on our understanding of what is conventionally referred to as business cycles: the succession of periods of relative prosperity and recession, or even depression, while, on balance, the material conditions of humankind steadily grow over the long run. Currently, the theoretical framework that most widely informs, shapes, and contributes to what we know about cycles is the so-called 'new neoclassical synthesis'¹. Its core is the 'basic neoclassical model' proposed in the early 80s as part of the theory of real business cycles. This basic model underpins all mainstream² research in the field and is adapted according to the purpose at hand: either to study a particular aspect of the business cycle or to be used as a 'laboratory' for policy analysis, for instance. Despite there being dozens, if not hundreds, of different variants of this basic neoclassical model, its fundamental structure is always preserved. It is precisely on that invariant structure that I focus.

If we subject the basic neoclassical model to 'reverse engineering', it is easy to see that its two main building blocks are the neoclassical consumption, or decision, theory – which is not my focus in the present text – and the traditional neoclassical theory of

¹ This expression was coined by Marvin Goodfriend and Robert King (1997). For a more recent update on the elements of this synthesis, see Michel Woodford (2009).

² For the purposes of this thesis the labels 'mainstream' and 'neoclassical' are synonyms, used interchangeably. In other contexts that might not be so (see Lawson, 2013).

capital (often loosely referred to as ‘the supply side’), the original formulation of which can be located in the writings of John Bates Clark, Irving Fisher, and Frank Knight³.

Briefly, capital theory integrates the analysis of production into an otherwise pure exchange theory. In neoclassical thought, the theory that emerges from that integration takes the preferences of economic agents, the techniques of production and the endowment of factors of production (i.e., capital, labour and land) as given. Then, the theory determines equilibrium, which corresponds to a system of relative prices, the associated level of output/income and its distribution.

In the traditional neoclassical theory of capital, capital is conceived as a single factor of production, whilst the capital endowment consists of many physically distinct capital goods. Given free competition, the workings of supply and demand guarantee that the system features a uniform rate of profit – or interest rate⁴ – over the supply price of capital goods. That uniform rate of profit is achieved through an endogenous, though still implicit, process, through which the less profitable capital goods are relatively less demanded (and so, less supplied) than the relatively more profitable ones. In this way, capital changes its form: its physical composition adjusts. Since the new capital goods are produced using the same economic resources that had produced the old ones, this change in form brings no change in value whatsoever. At the end of the adjustment process, every capital good ‘earns’ the same rate of profit (or interest) – thus the idea of a uniform rate of profit (or interest). A uniform rate of profits assures that the specific capital goods in existence are the most profitable ones (given the available technology and the preferences of consumers). This way, the theorist can reason about capital in terms of a single and homogeneous quantity of value, i.e., as a single factor of production, with the rate of profit (or interest rate) as its income.

The propositions of this theory establish the relations among the earnings of factors, their employment, and the level of output. There have been very many different versions of this theory. Its results and main assumptions have always been preserved, though. In recent mathematical models, for instance, the productive sector is modelled by

3 The most relevant texts are: Clark (1888, 1899), Fisher (1906, 1907, 1930), and Knight (1933, 1934, 1936a, 1936b).

4 The difference between interest rate and rate of profit is a recurrent confusion in capital theory. For the most part, I treat them as synonyms in this thesis. I shall return to this issue in chapters 3 and 4, however.

specifying a production function that takes as one of its arguments the quantity of capital, which is, in turn, a homogeneous substance. It is precisely this version of the traditional neoclassical theory of capital that appears in real business cycles theory⁵.

Although in real business cycles theory the causes of business cycles are exogenously generated random shocks, it is through the production function and, therefore, through the machinery provided by neoclassical capital theory, that those shocks affect the economy and that cycles are ‘understood’. Clearly, then, the way capital is treated is crucial for mainstream business cycle theorising.

Surprisingly perhaps, there are, within the realm of the mainstream, alternative ways of dealing with the concept of capital. Instead of treating or conceiving it as a homogeneous factor of production – as is done in the real business cycles theory – one could use a physical, disaggregated heterogeneous concept, which goes back to Léon Walras⁶. This conception of capital re-emerged in neoclassical literature with Sir John Hicks’s *Value and Capital* (1939) and became influential in the second half of the 20th century in the context of general equilibrium theory (see Arrow and Debreu (1954); Arrow and Hahn (1971)).⁷ In itself, this is something that deserves to be examined: why did a particular theory of capital become the preferred choice in real business cycles theory whilst neoclassical alternatives were available?

It is not hard to come up with a few partial answers to that question. First, when compared to the physical, disaggregated heterogeneous concept of capital and its associated framework, the neoclassical theory of capital which takes the production function as the central concept constitutes a technically easier alternative to implement in mathematical models. It also facilitates the kind of empirical analysis that real business cycles theorists envisioned, i.e., calibration followed by simulation.

Second, real business cycles theorists sought to study fluctuations through a framework coherent with the regularities and the propositions of the theory of long-run

⁵ The label ‘traditional’ refers to the subset of the neoclassical theories of capital that conceive of capital as a single factor of production, either as a quantity of homogeneous value encrusted in heterogeneous capital goods, or as a physically homogeneous factor of production.

⁶ In this case, the assumption of a uniform rate of profit is dropped. More on this in chapter 3.

⁷ Other economists worked on capital theory along the path set out by Walras (see Hennings, 1990). The synthesis provided by Hicks (1939) is, however, usually credited for the re-emergence of this concept of capital.

growth. The neoclassical optimal growth model was, by the late '70s and early '80s, when the real business cycles approach was being forged, the dominant framework for understanding growth. It consists of Robert Solow's (1956) model augmented by the neoclassical choice-theoretic framework, with the inclusion of a stochastic component to model the technological parameter proposed by Solow. The neoclassical optimal growth model is thus an offspring of the Solow model, which is in turn built on the basic, traditional neoclassical theory of capital. Real business cycles theorists 'chose' to use the traditional version of the neoclassical theory of capital over possible alternatives, indirectly, through their option to resort to the neoclassical optimal growth model for the study of fluctuations.

These and similar answers are, however, insufficient. In the '60s – roughly twenty years before the emergence of real business cycles theory – the neoclassical production function with aggregate or homogeneous capital had been proven to be theoretically (and even mathematically) *flawed*. This occurred in the context of a fierce debate on capital theory, opposing (essentially) heterodox economists from the University of Cambridge, UK, to mainstream economists based at the Massachusetts Institute of Technology (MIT), Cambridge, USA⁸. This debate came to be known as the 'Cambridge controversies in the theory of capital'⁹. This dispute, which was wide-ranging and technically sophisticated, started in the '50s. In the late '60s, contributions to it were already in sharp decline. The case against the production function or, more generally, against the traditional versions of the neoclassical theory of capital, was first fully articulated by the Cambridge side and later accepted, not before a huge struggle to prove it technically invalid, by the MIT side - namely by its figurehead, Paul Samuelson. In other words, there *was* a recognition, by *both* sides of the debate, of problems with the traditional neoclassical theory of capital and that was taken as an advance in the understanding of capital.

After this recognition, the MIT side focused on trying to find neoclassical ways of treating capital that were immune to the Cambridge critique, and multiple contributions

⁸ Although economists from both camps of the debate could be said to be from Cambridge, this thesis reserves the labels 'Cambridge side' or 'Cambridge economists' to the economists of the University of Cambridge, or otherwise belonging to that side of the controversy.

⁹ Later, as more economists joined the debate, this opposition between the University of Cambridge and the MIT becomes less obvious. However, the debate remains known as the 'Cambridge Controversies in the Theory of Capital', a label coined by Geoffrey Harcourt (1969) in his famous narration of the debate.

appeared. It was in this context that, as I mentioned above, a distinct, ‘Walrasian’ way of treating capital gained prominence, enhancing the relevance of general equilibrium theory. To many, this ‘new’ theory seemed to overcome the difficulties identified during the controversies.

In other words, there is a theory of capital that is defeated by the criticisms of the Cambridge side, a defeat recognised by its own proponents. Partially as a response, a different theory of capital was brought to the fore. Yet, despite this alternative, the paradigm that would emerge in the early 1980s to study business cycles *was built on the unquestionably flawed theory of capital*.

Now, if the neoclassical theory of capital in its traditional versions is admittedly flawed, the fact that it was even contemplated as a possible choice by neoclassical economists is perplexing – perhaps even more than the fact that it was actually chosen, and this is not addressed by any of the arguments mentioned so far. So, those arguments, though perhaps necessary, *are not sufficient* to explain the choice of the particular version of the neoclassical theory of capital that we find in real business cycles theory.

Why has a discredited theory of capital survived? In other words, how can it be that, despite there being an alternative, a concept, and indeed a whole theory, *recognized to be flawed* remained in use, and was even pushed to the core of economic theorising, including, specifically, business cycles theorising?

The aim of this thesis is to offer a possible explanation for this puzzle. It does so by proposing a different interpretation of the Cambridge controversies in the theory of capital. The majority of the contributions to this debate, if taken at face value, seem to address theoretical or technical issues only. The economists from Cambridge attacked the neoclassical theory of capital on, *prima facie*, those sorts of grounds, and the economists at the MIT engaged in a defence of the neoclassical theory at the same level. However, if one looks at the debate from an ontological viewpoint, or with a certain degree of ontological awareness, a different, more illuminating understanding can be achieved. Theoretical or technical disagreements constitute only part of the Cambridge critique and are surface reflections of implicit, or at least under-articulated, conflicting visions of the nature of the economic world.

Mainstream economists did realize that the neoclassical theory of capital and some of its core concepts were technically flawed. Nevertheless, or so I argue, given their implicit ontological commitments, the main results reached in the Cambridge controversies were ultimately irrelevant to them. And so, eventually, they devalued the results achieved in that debate. This is the gist of my account of why its impact turned out to be minimal. As we shall see, all this is backed by substantial and, I believe, convincing evidence.

It follows from the previous paragraphs that my thesis is at the border between the history of economic thought, economic ontology and macroeconomics. The breaking up of the puzzle presented above allows for the classification of my work as an ontologically informed contribution to the history of economics. The fact that I analyse current business cycles theories places my work within modern macroeconomics. Both exercises exemplify how ontology can be useful to the academic economist and central for a relevant economics.

Chapter by chapter, the thesis is organized as follows. In chapter 2, I construct my interpretative framework. I start by studying the *possibility* and *usefulness* of ontology, notably when the object is the social world. The strategy is to establish, first, the kind of problems ontology deals with, and then to document the emergence of ontology as a legitimate and fruitful form of study. This strategy further provides the necessary material to develop the interpretative framework to be used for the analysis of the capital theory debate. In this regard, the notion of ‘ontological commitment’ is particularly important because, as hinted above, the main argument in this thesis is that the ultimate source of disagreement between the two sides of the Cambridge capital controversies is related to the ontological commitments characterizing each side.

Specifically, this chapter endeavours to establish the following propositions:

P2.1 Ontology is a feasible form of study within the economics discipline.

P2.2 Ontology is fruitful in the sense that economics can benefit from its systematic study.

By establishing how economics can benefit from ontology, the second proposition encompasses the following two sub-propositions:

P2.2.1 Ontology can help in accounting for the historical evolution of economics, while diagnosing and explaining tensions or conundrums in the discipline, in specific schools, or in specific theories.

P2.2.2 Ontology can provide a way forward for substantive economic theorising;

In Chapter 3, I provide an exposition and discussion of modern mainstream business cycles theories – real business cycles theories – in their own terms. The emphasis of the exposition is on the foundational structure of real business cycles theory. That foundational structure is, as stated above, the basic neoclassical model, a model which took over macroeconomics in the early 1980s when economists started to look for micro-founded macroeconomic models. This chapter scrutinizes the capital theory foundations of the basic neoclassical model, underscoring that it is built on the traditional neoclassical theory of capital. This theory of capital was not, as observed, the only available alternative, and this chapter also expounds the essentials of the alternative theory. In short, this chapter establishes the following two propositions:

P3.1 Real business cycles theory crucially relies on a specific, traditional version of the neoclassical theory of capital.

P3.2 Within neoclassical thought there is an alternative theory, or way of dealing with, capital, viz. ‘Walrasian capital’.

As the second proposition raises the question as to why the traditional version of the theory of capital was the selected framework to integrate capital into real business cycles theory, chapter 3 also systematizes possible explanations for that.

Chapter 4 deals with the debate on capital theory – the ‘Cambridge controversies in the theory of capital’ – which took place in the ‘50s, ‘60s and early ‘70s. This debate focuses on the notion of capital and the associated use of production functions, on the relationship between capital intensity and the rate of profit or rate of interest and, eventually, on the merits of general equilibrium theory. As stated above, one of the most interesting results of the debate was that, from a mathematical viewpoint, the production function and the conception of capital in the traditional neoclassical theory were flawed. This chapter reproduces the relevant mathematical demonstrations, along with some, less formal, arguments, all formulated by the Cambridge side of the debate. Contributions

from the MIT side are also carefully surveyed. This chapter, then, is concerned with the following propositions:

P4.1 The traditional version of the neoclassical theory of capital is pervaded by inconsistencies.

P4.2 The neoclassical efforts to rehabilitate the traditional version of the neoclassical theory of capital turned out to be unsuccessful, leading to the neoclassical side conceding defeat.

P4.3 The initial defeat led neoclassical economists to develop a new theory of capital and this spurred a feeling of victory for the neoclassical side.

Taken together, chapters 3 and 4 point, as remarked earlier, to a puzzle: admittedly flawed concepts – the production function and the associated conception of capital – became a cornerstone of macroeconomic theory in general, and real business cycles theory in particular. This puzzle is the subject of chapter 5. Chapter 5 thus builds on the results of all previous chapters in order to answer the following question: how can it be that a theory *recognized to be flawed* remained in use, and was even pushed to the core of economic theorising, including, specifically, business cycle theorising?

The aim of the chapter is to prove, firstly, that

P5.1 The capital theory debate had little, if any, impact in mainstream circles after the '70s, including in real business cycles theory, even though, during that debate, the neoclassical side recognized the flaws of its capital theory.

In order to explain this, chapter 5 looks at the Cambridge controversies in the light of recent advances in the philosophy of science. Ontology, though not directly touched upon during the debate, turns out to be at the core of the controversy and enables a better understanding of the puzzle with which I am concerned. The chapter seeks to establish the following propositions:

P5.2 The Cambridge capital controversies can be reinterpreted as a clash ignited by the belief that neoclassical theory was unable to address reality as it really is.

P5.3 Since the essence of the Cambridge opposition was ontological but the debate never explicitly addressed ontology, the Cambridge criticisms were

not deadly, which accounts for the survival of the traditional neoclassical theory of capital.

Chapter 6 concludes this thesis by drawing attention to various directions in which the argument could be expanded. In particular, it explores the relevance of some threads of the argument for a variety of topics; and it attempts to articulate – in very broad terms, of course – a general framework for the theory of capital, drawing on the Cambridge debates, on non-mainstream theories of capital, and on contributions to social ontology.

“A curious thing about the ontological problem is its simplicity. It can be put in three Anglo-Saxon monosyllables: ‘What is there?’ It can be answered, moreover, in a word – ‘Everything’ – and everyone will accept this answer as true. However, this is merely to say that there is what there is. There remains room for disagreement over cases; and so, the issue has stayed alive down the centuries.”

Willard van Orman Quine (1948b)

2 Fitting Ontology in Economics: Methodological Reflections

“What you saw was the materialization of your conception of her.” This is how Dr. Snaut clarifies the rather odd event Kelvin had just experienced. The passage is taken from Andrei Tarkovsky’s 1972 film, *Solaris*. ‘Solaris’ is the name of a distant, strange planet where a manned space station was sent to. Once there, a series of mind-blowing events happen on board of the station and a further scientist, Kelvin, is sent there to investigate what is going on. When Kelvin arrives, he observes the unexpectedly weird behaviour of the crewmen of the station. On some occasions, he believes he glimpses the presence of people beyond the crewmen. One day, Kelvin sees his late wife, who had died years before. In the opening passage of this paragraph, Dr. Snaut explains that her appearance is a product of Kelvin’s thoughts. To some extent, her being was defined by Kelvin’s memories.

Tarkovsky’s film seems to invite us to embark on a metaphysical meditation about existence. What is reality made of? Is it the product of our thoughts and thus mind-dependent? Does existence precede awareness, or is existence defined by awareness? Is the world we live in just an illusion of some sort? These questions are, as Willard van Orman Quine above implies, age-old. They are systematically addressed within the intellectual discipline of metaphysics, defined long ago by Aristotle: “[it] is a science which investigates being as being and the attributes which belong to this in virtue of its own nature”. Metaphysics is thus the study of the existential dimension of the world

through multiple angles: the nature of existence, including the general characteristics of being, the existents or entities that there are, and what all things that exist have in common. The study of being can, however, be viewed in a slightly different, but crucial, perspective, which is that of ontology (from the Greek words *onto* which means ‘being’, and *Logos*, which means the ‘study of’). In its philosophical branch, ontology is a department of metaphysics: while metaphysics concerns itself with all being, all reality, ontology focuses on that part of being, of reality, which constitutes the object of science¹⁰. This notion of ontology I shall call *philosophical ontology*. Besides this philosophical dimension, there is also a *scientific ontology*, which refers to the study of specific categories, or existents framed by science (Lawson, 2015a, pp. 19–20). As such, scientific ontology is, broadly speaking, less abstract and less general compared to philosophical ontology, which is mainly concerned with the most general features of being, i.e. with reality’s ultimate structure (Lawson, 2015a, pp. 19–20).

The word ‘ontology’ thus refers to a form of *study* concerned with the fabric of the object of scientific inquiry. As such, it asks Quine’s question “*what is there?*”. ‘Ontology’ can, however, have a different connotation. It can mean the results of such study, i.e., the answer to Quine’s question. “Everything” thus stands as a theory of ontology¹¹, or simply put, as an ontology¹².

Such a theory of ontology is, as Quine hints in the opening quotation, hardly satisfactory. And so, the question remains: What is there? Of course, attempting to answer, in a single chapter, such an old and controversial question would be futile. As it stands, however, the question ‘What is there?’ immediately begs another one; one that is the backbone of this chapter’s topic: how to learn about or examine what is there? Or, putting it differently, how to arrive at, or derive, an ontology? Hidden behind these questions, if they are to be taken as meaningful at all, is the idea that ontology is worth our while. But what exactly is the point of ontology, especially in economics? It is these methodological questions that this chapter seeks to illuminate.

¹⁰ Consider, for example, inquiries about the nature and existence of God. These unambiguously belong to metaphysics, as such Entity is part of religious or theological knowledge, and thus not part of the scientific object. Discussions about God thus stay outside the ontology research menu.

¹¹ This means that to exist is to be one of everything. For details on this, see (Salmon, 1987).

¹² Of course, whenever the sense of the word ‘ontology’ is found not to be evident from the context, I shall explicitly clarify it.

This chapter thus focuses on the *feasibility* and *usefulness* of ontology, in addition to the ways in which it can be *practised*. I shall endeavour to establish two propositions: P2.1: ‘Ontology is a feasible form of study within the economics discipline’; and P2.2: ‘Ontology is fruitful in the sense that economics can benefit from its systematic study’.

The first proposition, ‘Ontology is a feasible form of study within the economics discipline’, demands, firstly, an understanding of the type and nature of problems that ontology, the field of study, deals with. Those problems are bundled together under the umbrella notion of ‘ontological problem’. At a sufficiently high level of abstraction, the ‘ontological problem’ has essentially the same nature both in philosophy and in social science (and in economics in particular), and that is the problem of determining what exists. Importantly, however, this problem emerges in a crucially different manner within the *practice* of social sciences, thanks to the type of material with which social science is concerned. More than inventorying what (type) entities there are – as the philosopher would do – ontology of the social world tends to focus on the specific ways in which the things that there are shall be conceived.

Having thus established what kind of problems ontology deals with, this chapter can move on to the first proposition. In order to establish the feasibility of ontology, this chapter follows a twofold strategy. Heuristically, this twofold strategy can be interpreted as consisting of an *ex-ante* and an *ex-post* case on the matter. Showing the feasibility of ontology in an *ex-ante* manner consists in arguing that it is a *conceptually* and *methodologically* possible form of inquiry. This is done through the study of the emergence of ontology as a legitimate field of study, first in 20th-century philosophy, and then in economics, during the 1990s. Indeed, by virtue of being regarded with distrust by some philosophers, and as a complete novelty in the case of economics, the first proponents of ontology felt the need to explicitly elaborate the ways in which ontology could (legitimately) be pursued, and by doing so they have shown that ontology is *feasible*. This chapter takes advantage of this and studies Willard Quine, Uskali Mäki, and Tony Lawson, including, briefly, the intellectual context from which these projects emerged. Among other things, the *conceptual case* that each of these authors makes for the *feasibility* of ontology is documented. Briefly, both Quine and Mäki, though in rather different ways, argue that scientific theories – from natural science in the case of Quine, and from economics in the case of Mäki – are legitimate entry points for ontological

inquiry. Lawson, in turn, contends that observed human practices enable ontological inquiry in the social world. Thus, the *conceptual case* amounts to the idea that there is no epistemological barrier for addressing ontological issues.

Additionally, through the study of these authors, their approaches and methods, this chapter gathers a set of useful conceptual tools for doing ontology in economics. In particular, this chapter details the concept of transcendental reasoning, the related notion of ontological commitment, and it also discusses the nature of ontological propositions. These reflections make up the *methodological case* for the *feasibility* of ontology, for by specifying how ontology can be done, this chapter is, *a fortiori*, showing that ontology is a *feasible* form of study.

The above paragraphs detail what is developed in this chapter in terms of the *ex-ante* case for establishing the *feasibility* of ontology. The *ex-post* strategy consists simply in documenting substantive contributions to economic ontology. Here the range of authors considered is expanded, something that allows for a more comprehensive overview of the field of economic ontology. As it happens, these contributions are also relevant for the discussion of proposition P2.2: ‘Ontology is fruitful in the sense that economics can benefit from its systematic study’, as they illustrate how ontology can be useful for economics.

The study of the second proposition also involves a more conceptual analysis. That analysis consists in reasoning whether the ‘resolution’ of the ontological problem is of any profit to, especially, economics. This chapter addresses this by, again, studying the authors mentioned above, with a special emphasis on Mäki and Lawson. Indeed, while pushing the case for ontology in economics, their argumentative strategy encompasses also statements about what the implications of the results of economic ontology inquiries can be.

Taken together, the review of existing substantive contributions to economic ontology and the conceptual analysis allow the articulation of the following two sub-propositions on the benefits of serious and systematic concern with ontology in economics:

P2.2.1 Ontology can help in accounting for the historical evolution of economics while diagnosing and explaining tensions or conundrums in the discipline, in specific schools or in specific theories;

P2.2.2 Ontology can provide a way forward for substantive theorising;

The rest of this chapter is organized as follows. Section 2.1 deals with the ontological problem. In a sense, elaborating upon the notion of an ‘ontological problem’ details what ontology is about. And knowing what ontology is about is necessary to question, and then establish, its *feasibility*. Also, knowing what the ontological problem is furnishes some valuable material to understand whether its ‘resolution’ is of any profit to, especially, the economics discipline. As such, defining the ontological problem is a necessary step before directly tackling the two propositions this chapter deals with.

The study of these propositions starts, strictly speaking, with section 2.2. This section begins with some reflections from philosophy. Quine’s work is reviewed, and the focal point of the analysis is his case for the legitimacy of ontology and his vision on the way ontology can be practised. The story of the emergence of ontology within philosophy is completed by a (very) brief description of the intellectual environment in which the works of Quine appeared.

In section 2.3, the focus turns to economics. Before reviewing the works of Mäki and Lawson, this section pictures the state of economic methodology before ontology became a relevant topic of research. It then reviews writings of Mäki and Lawson on ontology as a field of study within economics.

Building on this last section, section 2.4 reflects upon concepts previously identified in the works of Quine, Mäki, and Lawson: the transcendental reasoning, ontological commitment, and the properties of ontological propositions. Section 2.5 documents some contributions to social ontology and section 2.6 concludes the chapter.

2.1 The Ontological Problem

For reasons beyond my control, I am writing these lines in an office at the temporary facilities of the Faculty of Economics. Although there is, so to speak, a ‘single landscape’ around this office, I can observe it from a few *different windows*. I can look,

for instance, through the biggest window which allows me to see an old sycamore tree which is not too far; but I can equally look through the small window just in front of my desk. This window, however, is of frosted glass: if it happens to be closed, I cannot actually see the sycamore tree. Indeed, at most, I can understand whether it is daytime or not. But the old sycamore tree is still in its place.

Similarly, and contrary to what may be suggested by the intriguing events in the Solaris space station – where thoughts seemingly create things outside the mind – I do believe *there is* a world out there whose existence is largely independent of our awareness of it. More precisely, there is a world whose existence is largely independent of, and even prior to, our *representations* of it. Representations of the world enable the consistent apprehension of reality. They stand between the mind and the world thus allowing the former to make sense and conceptualize the latter. Of course, while the world is largely independent of our representations, the reverse must not be true. Reliable representations of the world should bear some relation to it. It does not follow, however, that the representations we create are blind to the ideas and thoughts previously held, or that representations are uninfluenced by our perceptual capacities. On the contrary, representations of the world hinge on the way the world is, which in turn is filtered by our ways of thinking and perceiving. Clearly, thus, representations (always fallible) of the same (independent) reality can be multiple, and different representations may be useful for different purposes. The way the world is creates some constraints regarding the possibly acceptable representations of it but does not logically entail what and how those representations must be.

In other words: I am a realist. I could even go further and argue that everyone is a realist: anyone who believes in the existence of some kind of mind-independent entity or being is a realist (Lawson, 1997, p. 15). Yet, one can be a realist in many different senses (U Mäki, 1998b). Postulating the existence of an independent reality is clearly not the point of ontology, though. The subject-object distinction is, however, what grants meaning to the type of problem ontology addresses¹³. But what exactly is that problem?

¹³ Furthermore, failure to properly ascertain that distinction may lead to a fallacy, often referred to as ‘epistemic fallacy’ (see Lawson, 1997, pp. 34-35).

The nature and significance of the (let me use the proper term) *ontological problem* appear most clearly revealed – not surprisingly – in the very many metaphysical, and for that matter, ontological, disputes that philosophers have engaged in. Take, for illustration, the quarrel between the Platonists and the Nominalists. The defining characteristic of Platonists is that they hold the proposition that abstract (i.e., non-physical and non-mental) entities exist. The Platonist would argue, for instance, that the sycamore tree outside of my office has the property of being a tree, say *treewood*. More importantly, *such property exists independently of this, or indeed any other, tree*. In addition to trees, there is also the property of being a tree. The property of being a tree, or *treewood*, is, of course, non-material: technically, it may be called a *Universal*, instantiated by certain objects, *particulars*, in this case by a sycamore tree. Contrary to this Platonist outlook, the Nominalist holds that only material (i.e., physical or mental) objects exist. The argumentative strategy of Nominalists is vast (as is that of Platonists), and it is largely unimportant here. The point is that we are facing two different accounts about what (types of) things exist. This is an example of what Quine refers to as a ‘disagreement over which specific things exist’. The ontological problem is ‘what is there?’ – this is the archetypal question in ontology – but it can be rephrased or re-conceptualized as, and indeed instantiated by, the challenge of studying which side, the Platonist or the Nominalist, is right.

Now, it is more or less evident that when one is specifically concerned with the ontology of the social world, the ontological problem emerges in a crucially different manner. By ‘social world’ I mean that part of the world whose existence results from and is dependent upon human action. This is the standard understanding of that category, and it is what defines the object of social science. If the object of social science is thus defined, then social scientists, by virtue of dealing with human action, deal with the beliefs and intentions of human agents in the context of their actions. As Hayek puts it, “when we have to explain human behaviour towards *things*; *these things* must [...] not be defined by what we may find out about *them* by the objective methods of science, but in terms of what the person acting thinks about *them*” (1942, p. 91 italics in the original). This is so, again, because social phenomena, the object of social science, are the result of human action, which depends on the conceptions held by acting agents (Lawson, 2015a, pp. 30–31). What follows from this, so far as the current concern goes, is that, for the most part,

at least, the explanatory categories of social science (say, markets, commodities, money, institutions, capital, systems of production) are known to exist before deep scientific inquiry:

“whilst the entities of (or posited within) natural science (e.g., super strings, quarks, tanon-neutrinos, black holes) are at first unfamiliar, being the objects of conceptions formulated within scientific work in the course of explaining observed phenomena, resolving theoretical contradictions and the like, and so in principle discoveries, the explanatory categories of social science, including economics, are typically already known (and agreed upon), at least under some description, prior to the work of science.”

(Lawson, 2015a, p. 30)

Contentions arise, however, about the specific ways those explanatory categories are to be understood. Thus, the point of social ontology is not, essentially, to establish what exists in the social world; instead, the problem in social ontology has to do with the proper ways in which explanatory categories should be conceived and articulated (Lawson, 2015a, p. 31). The *social ontological problem*, in other words, is not the discovery of explanatory categories for social science, but the study of their being, of the way they exist, and eventually, of how they came into being¹⁴.

Solving the (social or whatever) ontological problem amounts to theorising about what exists and how it exists. To do it so, and from a methodological standpoint, first one has to establish how ontology can be pursued. This involves a theory about the legitimate entry points as well as tools that enable one to retrieve meaningful ontological propositions. The next section addresses the former, while the latter is the object of section 2.3.

2.2 Ontology and Philosophy

Earlier we saw that Aristotle put forward a definition of metaphysics. Metaphysics is thus anything but new. And neither is ontology. Yet, perhaps surprisingly, the path of both

¹⁴ One could argue that it follows from Hayek’s quotation above that social ontology ought to be concerned solely with the conceptions agents possibly hold about the relevant explanatory categories. As the formulation of the social ontological problem hints, that is not a vision endorsed here.

philosophy and science throughout the centuries led to a decline in the interest in, and even a discredit of, ontology. It was not until Willard Orman von Quine (1908-2000) that ontology came to be seen as a legitimate and fruitful form of inquiry. So much so, that some suggest that Quine not only revived the term ‘ontology’ (Hylton, 2004, p. 115) but ignited an Ontological turn in his discipline, analytic philosophy¹⁵ (Latsis, 2007). This turn was stimulated by the necessity and usefulness of addressing ontological matters as well as of providing a methodological structure, or framework, for such form of study. More than anything else, this framework was, in my estimation, crucial for the success of Quine’s intellectual project. For this reason, the turn to ontology in 20th-century analytic philosophy through Quine’s contributions can teach us about the ways in which ontological inquiries are to be pursued.

Quine was heavily influenced by the logical positivist tradition¹⁶ (Isaacson, 2004, p. 214), dominant in the first half of the 20th century. Within logical positivist (or empiricist) circles, philosophy (and thus metaphysics and ontology), as practised until then, was regarded as a largely sterile form of inquiry to understand the actual world. While natural science was concerned with experimental activities, one of the sources of knowledge to the empiricist, philosophy, the empiricist would tell us, was concerned with the study of the meanings of concepts. Accordingly, Rudolf Carnap – one of the most relevant contributors to the philosophy of logical positivism – argues that results of philosophy (and thus of metaphysics and of ontology), contrary to those of science, are extraneous and lack significance (Carnap, 1996).

Implicit in Carnap’s point is the idea – crucial for some positivists – that it is possible to straightforwardly distinguish between two kinds of propositions, namely *analytic* and *synthetic* propositions. Analytic propositions are the ones whose truth value is obtained by simply uncovering its meaning. As Quine notes, Leibniz referred to analytic truths as ‘truths of reason’, as opposed to ‘truths of fact’ (1951, p. 20). ‘Truths of fact’, in turn, are the ones whose truth value is not derivable from the meanings of words alone. These are synthetic truths. To illustrate, the proposition ‘All bachelors are

¹⁵ Friedrich Gottlob Frege, together with Bertrand Russell and George Edward Moore are the founders of this philosophical tradition. All of them were, at some point of their work, concerned with ontology. Quine, however, is usually credited with the revival of ontology as a necessary and fruitful endeavour within analytical philosophy Soames (2014).

¹⁶ For an overview and analysis of this doctrine, see Richardson and Uebel (2007).

unmarried' is true and I know it because I know the meanings of words: I do not need to go out to the street to inquire bachelors whether they are married or not. As for the proposition 'Daniel is single' there is nothing in it entailing its truth or falsehood. The truth value can only be discovered if, for instance, one asks Daniel about it. Given the conception of science and philosophy held by the positivists, the distinction between analytical and synthetic statements is important as it allows to straightforwardly distinguish science from philosophy¹⁷. Being concerned with the meanings of concepts, the results of philosophy consisted of analytic statements. On the contrary, those of (empirical) science, were synthetic, implying that science, and science only, truly adds to knowledge.

In one of his most influential articles, Quine (1976) points out that it is impossible to get a non-circular account of *analyticity* and thus it is impossible to set a definite boundary between analytic and synthetic statements. Let me briefly explain. Clearly, I cannot learn the truth value of the proposition 'Daniel is single' by understanding what it means, that is, there is nothing in the meaning of 'Daniel', 'is', and 'single' that implies a specific truth value. But, what about the first proposition: 'All bachelors are unmarried'? Clearly, 'bachelor' and 'unmarried' are synonyms, and this fact suffices for my judgement on the truth value of the sentence. From this it seems to follow that in order to define analyticity, one must come to terms with the notion of 'synonym'. The most obvious way to describe that category is to say that 'synonym' consists in two terms sharing the same meaning. But this simply carries the question over to the notion of 'meaning'. So, what is 'meaning'? Quine believes that the category of 'meaning' is not as clear as it would be necessary for the purpose at hand. One possible notion of 'meaning' could be as follows. As Quine notes, it is through use and experience, i.e. the usage of language in general, that the meanings of words are apprehended. As such, I know the sentence to be true because I know its meaning, and I know its meaning because I learned it through experience. The meanings of words are thus an empirical fact of language. However, if this is so, then, ultimately, the proposition 'All bachelors are unmarried' is not, strictly speaking, analytic, as experience plays a role in the determination of its truth value. The only way to overcome this conundrum would be to put matters of fact

¹⁷ Indeed, different authors have conceived analytical and synthetic statements in many different ways. The one exposed in this text is, broadly speaking, the one Quine was criticising, so it is the relevant one here.

completely aside from questions of meaning, in which case analyticity would have to be invoked to define ‘meaning’, and we would end up with a circular reasoning. In other words, defining meaning apart from fact would require analyticity, which is the very notion we are trying to define in the first place. Other alternatives, such as making the notion of meaning dependent on the notion of ‘definition’ or ‘necessity’ suffer from the same predicament. One can, therefore, conclude that the distinction between analytic statements and synthetic statements is not so clear as positivists thought.

By breaking down this distinction, Quine questions the existence of a meaningful distinction between science and philosophy. Philosophy and science are not sharply separated, but instead stand in continuity, intertwined, and thus they can and should influence each other¹⁸. In addition, this legitimizes, among other things, the use of empirical claims in philosophical reasoning. In the case of ontology, this means that the ontologist can use science, i.e. its theories and results, in the study of ontological issues. Science, or more precisely, scientific theories are legitimate entry points for ontological investigation.

It follows, then, that one possible way to solve the ontological problem is to pick science and its theories and investigate what their existential stance on the world is. And this was precisely what Quine did in his substantive work on ontology. How precisely this can be done is discussed later in this chapter.

2.3 Ontology and Economics

Although timidly, the ontology of economics emerged as a systematic form of study in the latter years of the last century through, essentially, the contributions of Mäki and Lawson. These contributions influenced and inspired others leading to an increasing interest in the ontology within the philosophy of economics. In the face of this, some have suggested that there is an ‘ontological turn in economics’. What is the nature of this turn? What does it consist in? What was the state of the things before the turn? It is to answer these questions that I now turn. Answering these questions, I suggest, sheds substantial

¹⁸ It follows, therefore, that philosophy, and thus ontology, may have practical significance, if only through their influence on science.

light not only on the role which ontological research may play in economics but also on how ontology can be pursued within economics.

The Philosophy of Economics Before the Turn

The philosophy of economics is a branch of the philosophy of science. Without being exhaustive, the philosophy of economics deals with the nature, status, and scope of economics; it also deals with the contours, if any, of its objects of study, its relationship with other fields, like ethics or psychology or neuroscience, the role and distinction, if any, between a positive and a normative dimension of economics, or the interpretation of central concepts for economics such as agency or rationality. An important sub-field in the philosophy of economics, one that is, though in different degrees, present in any discussion within the field, is the Methodology of economics.

The Methodology of economics comprises, essentially, three layers of analysis. First, the methodological layer (note the lower-case *m*, marking the difference to the Methodology of economics as a whole) deals with the specific methods, tools, or strategies that are used, or that should be used, in creating new knowledge. For example, studying a given research tool, say a given econometric tool or a certain kind of qualitative method, in order to define its scope and rules of application, is a concern of methodology. The second layer is epistemology. Epistemology refers to the study of the nature, origins, and conditions of validity of scientific knowledge. Here the level of abstraction is higher when compared with methodology, as what is at stake are the conditions and rules that render knowledge, in general, reliable or acceptable. In other words, an epistemic theory may state that knowledge is valid if it can be stated in falsifiable propositions. Of course, this says very little about the specific tools and strategies the scientist shall employ in order to derive those propositions. The first layer, that of methodology, is where one can learn about those tools and strategies. In the third layer, finally, we have ontology, whose definition has already been put forward. In a word, it refers to the study of the nature of the objects of science. In sum, ontology tells us what the nature of our object of study is; epistemology tells us what it takes for us to properly know it; methodology tells us what we should do to know it.

It follows that the possible interventions of ontology in economics are to happen, first and foremost, at the *Methodological* level. The ontological turn, although it certainly

refers to a turn in economics in general and in the philosophy of economics in particular, has its epicentre in the Methodology of economics.

Now, if one is justified in speaking of a *turn* to ontology, it follows that before the *turn* had actually happened, ontology was not part of the main menu of the Methodology of economics. The hot topics in economic methodology in the 1990s, when the ontological turn started to take shape, are well documented in a set of articles compiled together by Roger Backhouse in the book *New Directions in Economic Methodology* (1994). As Fullbrook notes, the book “lists forty-seven pages that refer to Thomas Kuhn, sixty-nine to Karl Popper and seventy-three to Imre Lakatos” (2009, p. 1). Thus, this book provides evidence that Popper, Lakatos (and also Kuhn), though they were not specifically concerned with economics, have exerted great influence in the discipline’s methodological thinking.

In Popper’s philosophy of science, as articulated in Popper (2010), the ‘demarcation problem’ is central. He wants to distinguish between science and other forms of knowledge and thus establishes the necessary conditions for a theory to be scientific. In turn, Lakatos’ contributions to the philosophy of science (especially, Lakatos (1976)) can be interpreted as a continuation of Popper’s ideas (Caldwell, 1981, p. 85), largely intended to provide a framework for assessing competing theories or research programs. This latter concept of *research programs* had been at least foreshadowed by Kuhn (1932). Popper’s, Kuhn’s, and Lakatos’ concerns are, therefore, ultimately *epistemic*.

In general, the methodology of economics that builds upon the contributions of Popper and Lakatos (and also Kuhn) seeks to assess the performance of the economics discipline. The underlying assumption of this approach (indeed a potentially problematic one) is that philosophical theories construed with an eye on the natural sciences are relevant to the social sciences in general, and to economics in particular (Lawson, 2003, p. 31).

There is, however, another strand in the methodology of economics, a strand that is of a much more descriptive, rather than inspecting, nature¹⁹. The most influential and, at the same time, controversial, piece of methodological writing in economics to date

¹⁹ For a comprehensive overview of this literature see Blaug (1992).

perfectly speaks for this strand. That piece is Friedman's article "The methodology of positive economics" (1953). Its arguments constitute the climax of several positivist ideas in economic methodology. Paradoxically, it also elaborates on some anti-Positivist ideas (Caldwell, 1981).

Let us have a closer look at this text. Friedman begins by establishing the differences between positive and normative economics and goes on to define what for him is a proper positive economics. Proper positive economics must be "objective" science, in precisely the same sense as any of the physical sciences" (1953, p. 4). Friedman further suggests that the goal of science is to derive 'meaningful' predictions, implying that scientific theories are nothing but instruments capable of generating (correct) predictions. Thus, assessing a theory involves merely a diagnosis of its capacity of generating accurate predictions, and not a judgment about its 'realism' (1953, p. 4). Since, however, in general, many different theories can generate equally precise predictions, a second set of criteria, such as simplicity and fruitfulness, have to be used.

Despite the widely noted ambiguities and opacity in Friedman's text²⁰, it is almost irresistible to see his essay as a defence (even if largely unintended) of a certain way of doing economics. His emphasis on prediction and his claim that the realism of assumptions is irrelevant seem to, *prima facie*, legitimize standard approaches to model building in economics.

As suggested above, Friedman's essay perfectly pictures a specific approach to the methodology of economics. It is first (implicitly) assumed that economics, as practised, is rational. Then comes an *ex-post* justification or rationalization of the methods, strategies, and interpretations actually employed by economists. Of course, this kind of methodological exercise is basically descriptive and so can hardly have critical implications for practice (Lawson, 2003, pp. 29–32; R. E. Weintraub, 1989, p. 487).

In short, the methodology of economics was essentially concerned with epistemic matters. The ontological turn consists in the actual broadening of focus in economic methodology so as to include ontology. This turn to ontology in economics is embodied, first and foremost, in Mäki's and Lawson's contributions, Lawson being the one who

²⁰ This text generated a long series of articles, some attacking Friedman's position, others contextualizing it, but all interpreting it. For overviews of that literature, see Blaug (1992), Caldwell (2015), Mäki (2009).

most vocally called for an ontological turn²¹. An analysis of these authors provides, I believe, a more or less comprehensive picture of the field²².

Uskali Mäki

*“I have been active in a few areas that are interrelated in many ways”*²³. The interrelation Mäki mentions, while briefly describing his research, refers to the fact that, generally, all his research in economics involves its philosophical/methodological foundations. Indeed, he has written about interdisciplinarity (Grüne-Yanoff & Mäki, 2014; U Mäki, 2016), scientific and economic imperialism (U Mäki, 2009a, 2013), realism in economics (U Mäki, 1998a), the nature of economic theory (U Mäki, 2005, 2009b), and, the topic of this section, the ontology of economics. Mäki has written extensively on this topic, furnishing plenty of material for an analysis of the key features of his approach.

Mäki identifies two ways in which ontology can be viewed:

“Economic ontology may be practised as an exercise in direct description of what is believed to be the fundamental nature of the economy, or it may be viewed as an exercise in describing or prescribing the ontological presuppositions underlying theoretical accounts of economic phenomena or the ontological convictions held by economists. In the first guise, it is an attempt to directly represent economic realities, while in the second mode, it is directly about economics and only indirectly about the economy.”

(U Mäki, 2001a, p. 11)

Hence, ontology is, ultimately, about the economy, even when what is at stake are, primarily, the ontological presuppositions of theories and economists. When stating the questions ontology should answer, Mäki takes those to be: “What are the underlying presuppositions?” and, ‘How do the presuppositions constrain and determine belief?’ as

²¹ Indeed, the first time I came across the expression “ontological turn” was in this second book, *Reorienting Economics* (2003), as the title of the 2nd chapter.

²² Hodge (2007) offers a more comprehensive comparison between Mäki and Lawson. Here, more than the specific results Mäki and Lawson achieved, I am interested in their view of, including their approach to, economic ontology.

²³ Quotation taken from his personal website on the 7th of May 2017, <http://www.helsinki.fi/tint/maki/research.html>

well as, ‘How does and can one justify or criticize the presuppositions?’” (Ibid, p. 8)²⁴. Ontology is therefore relevant to the extent that it furnishes answers to those questions. As such, one could infer, ontology is relevant if it is about the conceptual frameworks underlying theories and/or assumed by economists, and not about what is beyond those frameworks, as suggested in the former quotation. Yet that is not the case. The point Mäki is implicitly putting forward is that only through the study of those frameworks can we get to know the nature and content of the world beyond those frameworks.

This view emerges most clearly in a piece that can be interpreted as Mäki’s manifesto on his general approach to the methodology of economics. There he observes that,

“Much of the recent philosophy and methodology of economics has followed a ‘top-down’ approach in the study of its subject matter. On this approach, one adopts a favoured philosophical account of science, imposes its descriptive categories and prescriptive rules upon this or that bit of economics or economics as a whole. Inquiry in this mode is a matter of applying a ready-made philosophical theory to the special case of economics. More often than not, the outcome of the exercise is the conclusion that economics appears to be in more or less bad shape as it does not meet the presumed criteria of good science.”

(U Mäki, 2002, p. 91)

Later in the same text, Mäki adds

“I have therefore become sensitive to the peculiar characteristics of economics and selective about my philosophical sources. In regard to the philosophical resources, I have learned to appreciate a critical and creative attitude: I believe those resources have to be adjusted and tailored so as to do justice to the specificities of the target of study, economics. The other side of the strategy is an empirical attitude: our philosophical account of economics should be informed about the actual theories, methods, and meta theories that practicing economists hold.”

(Ibid)

²⁴ Note that, ‘presuppositions’ here should be understood as ‘ontological presuppositions’.

Thus, for Mäki, economics as practised must play a role in the study of the philosophy/methodology of economics, even when contributions have a less descriptive, and a more normative, bent.

This kind of approach is exemplified in Mäki's discussion about the compatibility of economics with the philosophy of scientific realism (U Mäki, 1996, 2011). Scientific realism is understood, Mäki holds, in many different, and not equally plausible, ways. In the philosophical mainstream, scientific realism consists in the belief that current scientific theories are justifiably believed to be true of a set of entities they are ultimately about. These entities are independent of the human mind and lie beyond ordinary experience (U Mäki, 1996, p. 427). Instead of taking this version of scientific realism to be (either descriptively or normatively) relevant to economics – which would be a 'top-down' approach – Mäki suggests that the compatibility of scientific realism and economics should be analysed considering the specificities of economics.

Moreover, Mäki justifies his tactic by noting that the current mainstream version of scientific realism is itself a particular theory (not a general theory as it is usually taken to be) as it was developed with an eye on, and thus shaped by, physics (Grüne-Yanoff & Mäki, 2014, p. 4). Thus, scientific realism fails to accommodate some sciences not primarily because they are non-realist in the relevant sense, but because not all of them share relevant similarities with physics. Thus, the direct application of scientific realism to economics leads inevitably to a critical attitude towards economics, an attitude which is ultimately unfair as it is the product of overlooking the particularities of economics. This indeed mirrors Mäki's idea that, in general, most criticisms of economics are not a sign of its failure, but of its (neglected) specificity (U Mäki, 2011).

It follows that in order to study the compatibility of economics and scientific realism, the latter has to be adjusted²⁵. Yet the adjustment shall not be too radical if the label 'realism' is to remain in use. In other words, Mäki needs to establish what the essence of a realist science – natural and social alike – is. His suggestion is that scientific realism is simply the idea that there is a world out there that science, a fallible enterprise, tries to illuminate. At the ontological level, scientific realism holds that the objects of

²⁵ As my interest here is the study of Mäki's approach to ontology and not his results, the particular adjustments are not relevant for the current discussion. The same goes for the merits of the arguments being presented.

(true/current/best) scientific theories *exist* beyond them. Anything beyond this version of scientific realism is simply the description of discipline-specific features that never directly imply a non-realist interpretation of a given science which happens to lack them. Under this definition, economics (both mainstream and non-mainstream) is shown to fit with scientific realism^{26;27}. It follows, then, that we *can* learn about the nature of the economic world through the study of economic theories for they are about a world beyond them and thus include a set of ontological presuppositions.

This way, the goal of representing ‘economic realities’, to use his expression, can be successfully achieved through the study of the ontological presuppositions of theories and economists²⁸. By studying the “ontological presuppositions underlying theoretical accounts of economic phenomena or the ontological convictions held by economists” (U Mäki, 2011, p. 11) we learn about those directly; indirectly, that knowledge gives us material that helps to illuminate the actual nature of the economic world. Note that, this fulfils Mäki’s view that economics as practised *must* play a role in the study of the philosophy/methodology of economics. The study of economic ontology without considering economic theories or methods deployed by economists is uneatable.

Situations may occur, however, in which “the ultimate ontological convictions of an economist and the ontological presuppositions of the theories she holds are not perfectly tuned with one another” (U Mäki, 2001a, p. 10). Among other reasons, this may be due to the ignorance about the ultimate ontological implications of theories. This, under Mäki’s conception of ontology, creates both a challenge and an opportunity. A challenge because if the ultimate aim of economic ontology is to study the nature and contents of the economic realm, the fact that economists may hold ontological beliefs that are at odds with those their theories imply means that we end up in a situation where we have to judge which one should be taken into account. And it is not obvious by which criteria that judgment can be made. On the other hand, it is an opportunity since it creates

²⁶ Mäki (2002) also considers some common arguments against realism in economics, inevitably concluding that they do not provide enough material to reject realism.

²⁷ The tacit opponent here includes the instrumentalist or the fictionalist about scientific theories. Interestingly, Mäki (2009c) even proposes a realist interpretation of Friedman (1953).

²⁸ “[t]he contents and confines of the economic realm are suggested by the ontological commitments that economists hold” (U Mäki, 2001a, p. 4).

an occasion to revise the ontological beliefs of the economist or to adjust the theory in order to make it ontologically sound.

Also, awareness of that mismatch of ontological frameworks may spur reactions in terms of the choice and development of theoretical frameworks. As Mäki notes, “economists invoke ontological constraints in their arguments for or against particular theories or lines of theorizing” (2001b, pp. 386–385) and they do so, most of the time, without explicitly stating, and, I suggest, understanding, the nature of their arguments. So long as this type of constraints may have consequences in terms of which theories are generally accepted or dismissed, a complete history of economic theory should take ontology as well as the ontological beliefs of economists into account.

In sum, for Mäki, ontology is the study of the economic realm, a realm that is beyond theories and conceptual frameworks. As a branch of the Methodology of economics, ontology shall be pursued with an eye on actual economic theory and methods, *something that is legitimized once economics is shown to fit with (a minimal version of) scientific realism*. This definition of the domain of starting points for economic ontology is, actually, one of the sharpest distinctions between Mäki and Lawson, to whom I now turn.

Tony Lawson: a project to fix economics

Contrary to Mäki, Lawson found its way to ontology spurred by the belief that economics was in bad shape and needed reform. The remark that modern economics hardly explains, or addresses, real-world matters and that it fails at the level of policy (Lawson, 1997, p. 3) is the content of that belief²⁹. Lawson explains the source and nature of those problems while outlining a critique of modern mainstream economics, through three related, yet irreducible, vectors³⁰. Firstly, the vector more explicitly featuring a philosophical orientation focuses on the elaboration of a *philosophical ontology* for the social world, or social ontology, initially referred to as Critical Realism (after Bhaskar (1975), now more frequently characterized as Cambridge Social Ontology³¹. Building on

²⁹ For a more recent and comprehensive overview on Lawson’s position on this issue, see Lawson (2015).

³⁰ There is no suggestion here that Lawson explicitly frames his research in this way, or even that this depicts his strategy. This identification simply facilitates my exposition. Moreover, no ranking of the relative importance or temporal succession of each vector is implied.

³¹ The label “Cambridge Social Ontology” highlights the fact that most of the contributors to this ontology have or had some connection to the University of Cambridge, where the Cambridge Social Ontology Group

that ontology, plus several other considerations, Lawson develops the second vector as an ontologically grounded critique of the approach and substance of mainstream economics. Finally, the philosophical ontology of the first vector is supplemented by the elaboration, using a variety of approaches, of ontological accounts for some (central) categories of economics, such as institutions, corporations, and money. *Scientific ontology* emerges predominantly in this last vector.

If one were to follow Mäki's suggestion for the elaboration of an ontology of economics – the first vector in Lawson's project – one would necessarily have to consider current economic theories as starting points for such enterprise. This is not the strategy Lawson follows. For if, as Lawson holds, mainstream economic theory does not address real-world matters, in other words, if it is not relevant to understand the economic world, what could one learn about the actual nature of the economic realm by studying the ontic presuppositions of those theories? Someone who doubts its soundness, cannot use mainstream economic theory as an entry point to formulate an ontology; the way the world is actually like is, by and large, taken as inaccessible through economic theory. Of course, there is something to learn through the analyses of the ontological presuppositions of (believed to be) flawed theories. To cite but one example, one can articulate how the world would have to be like so as to 'make' a flawed economic theory reliable, which may, in turn, furnish elements to account for its failures. To avoid terminological confusion, this type of study – concerned essentially with uncovering the ontological presuppositions of theories – may be labelled, following Lawson (2014 p. 23), *internal metaphysics*³².

If, instead, one is interested in studying the existential dimension of the social world, as it is and not merely as it is represented in (possibly flawed) theories, is there any reliable entry point? There is no question that there exists a part of reality eminently social in nature, i.e. a part of reality that necessarily depends on human action. Now, human action, i.e. human practices, have explanations and conditions that render them possible. This is Lawson's *intelligibility principle* (2003, p. 33). As long as those explanations and conditions of possibility refer to the nature and structure of reality, they are *ontological*. As a result, Lawson, following Bhaskar (1975), and transcending Mäki,

– a research group on ontology – is based.

³² The label *internal metaphysics* also accurately describes Mäki's project, as Pratten (2007) suggests.

pursues a strategy in which the premises for ontological inference, i.e. the starting points for ontological research, are ‘generalized phenomena of experience’, including human practices. These include human intentionality, or the relative success of experimental activities within natural science, just to cite two. From those observations, the researcher poses, and hopefully answers, the question “what claims about the nature of reality are viable in the sense that they render intelligible” those observations? (Lawson, 1994, p. 271). This way of deriving ontological propositions is referred to as a *transcendental inference*.

Since the late 1980s, Lawson and other Cambridge scholars have been engaged in the formulation of a theory of social ontology along these lines. Their programme is to elaborate on “the specific nature of the fabric of society and economy” (Lawson, 1994, p. 273). As such, their attempt has been to elaborate an account of the most general features of being in the social realm. As Lawson notes, “in the Cambridge project it is philosophical ontology, and in particular social philosophical ontology, that so far has figured most prominently and extensively” Lawson (2014, p. 23). The main contributions to this project are gathered together, especially, in *Economics and Reality* (1997), and in *Reorienting Economics* (2003). This approach and vision of ontology – shared by Lawson and other Cambridge scholars – appears to be more ambitious than Mäki’s, in the sense that “it is conceived of as being about the world, not just about conceptual schemes or linguistic frameworks or the world-as-it-appears-to-us” (Pratten, 2014, p. 89).

The theory of philosophical ontology thus derived feeds Lawson’s second vector, the ontologically grounded critique of the mainstream. Briefly, this critique emerges from the comparison of the ontology derived from those noted ‘generalized phenomena of experience’ with the one underlying mainstream economics. The articulation of the ontological commitments of mainstream economics is, therefore, a crucial step of this second vector. For Lawson, the mainstream of economics is defined by its insistence on the use of formal mathematical methods, which in turn are anchored in a deductivist type of reasoning (Lawson, 2006, pp. 488–489) If the mainstream of economics is defined by its methodology, and not by substantial theoretical claims or any other kind of orientation, then the ontology of the mainstream is the ontology presupposed by those methods. Scientific methods are nothing but examples of human practices, and thus, according to the intelligibility principle, have explanations, i.e. conditions that render them intelligible.

There is yet another angle from which we can reach the idea that methods of science have ontological presuppositions. To know is to know about something, an object of knowledge. Our knowledge of it, however, can only be obtained in very specific conditions and supported by specific instruments or methods. And this follows from the fact that the ways of getting to know something are not independent of the nature of that something. In general, this means that methodology – the study of the techniques and instruments for research – epistemology – theory of knowledge, i.e. the study of how to obtain knowledge, and what circumstances render it valid – and ontology – the theory of being – are irreducible, yet related, to each other. The fact that a microscope is not a useful instrument for the study of the outer-space simply results from the fact that the nature of outer-space and of its constituents do not match the features of a microscope. Cosmologists are certainly better served with the Hubble telescope. Thus, generally, all theories of knowledge, which prescribe a set of useful and acceptable approaches, methods, and instruments, carry with them, albeit implicitly, some conception about the nature of the world.

Now, more to the point under consideration, Lawson shows that the ontology underlying the mainstream of economics is at odds with the one he infers from the ‘generalized phenomena of experience’. This conflict is the basis of his ontologically grounded critique of the mainstream of economics. Thus, while ‘successful human practices’, can teach us about how the world actually is, ‘unsuccessful human practices’ – as mainstream economics, Lawson would tell us – suggest us that the world may not be as those practices suggest.

Finally, in his third vector, Lawson focuses on the study of the ontology of some categories usually invoked by economists, orthodox and heterodox alike, in their theorising. As elucidated above, the study of the ontology of those categories involves, first and foremost, the study of their being, of the way they exist.

Taken together, these three vectors embody both the diagnosis and the prescription to cure the malaise of economics. The diagnosis, so Lawson argues, is that *economics is in bad shape as a result of ignoring ontology*. Accordingly, the models it produces can hardly be relevant, for only by chance can they embody, or relate to, the actual nature of the social world. Obviously, the way out of this predicament involves

explicit ontological reflection by academic (and practising) economists. In the next section, I articulate the specific ways in which this prescription can be accomplished.

Mäki and Lawson: what follows?³³

One of the objects of this study of Mäki and Lawson concerns the *feasibility* of ontology within economics discipline, i.e., proposition 2.1. Each author derives that feasibility in different ways. On the one hand, Mäki simply shows that, since economic theories are ‘realist’ – in Mäki’s own minimal sense – it contains ontological commitments and those can be scrutinised. On the other, Lawson prefers to show that the meta-theory of (mainstream, and non-mainstream) economics is a locus of ontological commitment, and that is sufficient to legitimize ontological inquiries in economics. Furthermore, his intelligibility principle authorizes inquiries into the nature of reality, taking as starting points general features of experience. In any case, these arguments constitute a conceptual (and *ex-ante*) case for the feasibility of ontology, thus supporting proposition 2.1: ‘Ontology is a feasible form of study within the economics discipline’.

Regarding the second proposition, i.e., P2.2: ‘Ontology is fruitful in the sense that economics can benefit from its systematic study’, the above hints to some ways in which ontology can be useful. Very briefly, Mäki suggests that ontology can help in accounting for the evolution of the discipline as ontological arguments are sometimes used in theory justification. Lawson, in turn, believes that the study of the ontology of economics furnishes crucial material to enable informed choices of methods, but also to indicate consistent ways of conceiving economic categories.

2.4 Practising Ontology: Towards a ‘How-to’ Guide

So far, I have analysed three approaches to ontology; not incidentally, those approaches are responsible for a turn to ontology in their respective fields.

³³ There are, of course, multiple implications of the projects Mäki and Lawson follow, but this section merely indicates those that are relevant for the propositions under study. One of the implications not touched here, but with a special relevance for the idea of economics as a science, is that since Lawson starts his ontological project from generalized human practices, including the practices of economists (i.e., their methods), rather than specific economic theories as Mäki does, Lawson is developing an ontology for a unified social science, where economics is a field within social science rather than a separate science.

From Quine we learned that (natural) science – its theories, especially those of physics – are legitimate entry points for ontological research. On the one hand, there are no fundamental or conceptual problems in using scientific (or empirical) claims in philosophy, and therefore, ontology – the study of being – should benefit from the relative successes of science. On the other hand, this is facilitated by the fact that theories do contain ontological presuppositions, a condition of possibility for ontological study as Quine envisions it.

In turn, Mäki and Lawson present us with, so to speak, a practical example of ontologically oriented research within economics. Mäki regards the study of the ontological presuppositions of theories as the main task before the Ontologist. Lawson, in consequence of his belief that (most of) economics is less than successful (in its own terms, he would argue), takes a different route. He focuses on the study of the conditions of possibility of human practices, whether successful or not, including approaches to science and scientific methods, but also general phenomena of experience. In this way, Lawson grasps both the actual nature of the world and the ontological theses underlying approaches and methods of the social sciences, economics in particular.

Two important elements – elements central to the main argument of this thesis – remain, however, still underdeveloped. First, how can one pursue economic research from an ontological viewpoint? That is, how to do economic ontology? From the above we already know that we can use (economic, or social) science or (generalized/successful) human practices as starting points. But, obviously, this tells very little. It is as if one teaches a child the alphabet without teaching her how to read properly. What is needed is a set of rules or procedures that any economist can use to derive ontological propositions – whether ontological presuppositions of theories, ontological beliefs of economists, or an ontological account of the object of economics – from a given legitimate starting point. The methods this section outlines – partially inspired by the authors studied in the present chapter, but also informed by other considerations in the relevant (philosophical) literature – are not to be taken as exhaustive, infallible, or strict. They are simply the ones I found myself using while reflecting on the issues this thesis touches upon, and in this sense, they seem to have worked for me. As it happens, by setting out how to do ontology, one is at the same time putting forward a methodological case for the feasibility of ontology, thus further substantiating proposition 2.1.

The second element – the rationale for bringing ontology into economics – is postponed to the next section.

Forms of ontological research and the scope of methods

Earlier I distinguished three forms of ontological research: philosophical ontology, scientific ontology, and internal metaphysics. The difference between philosophical and scientific ontology concerns essentially the level of generality considered. While the object in scientific ontology consists of specific categories deployed by scientists in their theorising, in philosophical ontology the interest is more about the general and ultimate structure of the world. In any case, the line between scientific and philosophical ontology is not easy to draw *a priori*.

Internal metaphysics is rather easy to distinguish from the other two forms of ontological research. It refers to the study of the ontological presuppositions of theories or methods, or the ontological beliefs of scientists. As such, the primary interest is not the formulation of a theory about how the world is, but a theory about how the world is taken to be in those theories, by those methods, or scientists. Accordingly, its results are not, surely not right away, to be interpreted in terms of a good or valid account of the nature of the world.

Quine and Mäki pursue ontology essentially by looking to the ontological presuppositions of scientific theories. They do so, recall, because they believe that this is the proper, or indeed the only, way to get to know the nature of the world. Internal metaphysics is, therefore, a mere necessary initial step to elaborate a theory of the actual nature of the world. Moreover, studying the ontological presuppositions of theories or methods is still of value when one is interested in studying the ontological beliefs of scientists. To that end, one simply has to pick the theories they propose or endorse or the methods they employ or approve.

Lawson, in turn, focuses on the study of the conditions of intelligibility, or possibility, of human practices. If these are successful, then the conclusions of ontological inquiry correspond to an (of course, fallible) account of the nature of the world. If, on the contrary, the practice is unsuccessful one gets an account about how the world would

have to be like so that for the practice to be successful or an account about the ontological beliefs of whoever is pursuing that practice.

The methods to be elaborated in this section are, generally, applicable to any of these forms of ontological research, provided the necessary specificities are taken into account, and the necessary adjustments are made. After all, to cite just one example, deriving ontological presuppositions from a human practice must be, at some level, distinct from deriving ontological presuppositions from scientific theories.

The transcendental inference

In order to retrieve ontological propositions, Quine asks what entities are necessary to make the theory true. Mäki, in turn, asks how the world must be like so that the theory under scrutiny is true. Finally, Lawson asks how the world must be like so that the observation making up the starting point is intelligible.

The apparent resemblance of the questions posed by each one of these contributors is by no means insignificant. For it suggests that they are all using a similar, if not the same, form of reasoning, or inferential strategy, to derive ontological propositions. Out of the three, Lawson is the one devoting more lines to explicitly discussing the nature of his way of reasoning, which he labels, following Kant and Bhaskar (1975), Transcendental Inference or Reasoning.

The transcendental reasoning is a special form of the retroductive argument, or abduction (see, for instance, Lawson, 2003). The term ‘abduction’ was first coined by Charles Sanders Peirce to name a form of reasoning that is neither a deduction nor an induction (see Peirce, 1955, pp. 150–156). Contrary to deduction and induction, Peirce claims that abduction is capable of generating new knowledge. What does Peirce mean by ‘generating new knowledge’? Peirce claims that all knowledge has to rest on observed facts. Our minds might furnish conceptual frameworks and tools (senses and ability to reason, for instance) for interpreting and making sense of observed facts. Yet our knowledge is not about those frameworks or tools; for if so, that knowledge would ultimately be about our minds; but it cannot be about observed facts either. After all,

“Observed facts relate exclusively to the particular circumstances that happened to exist when they were observed. They do not relate to any future occasions upon which we may be in doubt how we ought to act”

(Peirce, 1955, p. 150).

Then, Peirce goes on, “such knowledge must involve additions to the facts observed” (*Ibid*). These additions to “observed facts” take the form of propositions and are formally designated as hypotheses. Hence, “generating new knowledge” means adding new hypotheses to facts.

In induction, a given hypothesis is successively tested and, if those tests fail to demonstrate its falsehood, then the level of confidence in that hypothesis is strengthened. Or, putting it differently, one infers the generality of a hypothesis from a set of particular cases in which it has been found to hold. In turn, deduction establishes a given hypothesis by demonstrating that it logically follows from both a set of initial conditions and at least one general law. Finally, abduction is the creation of a hypothesis out of observed facts, or the “operation of adopting an explanatory hypothesis” (Peirce, 1955, p. 151). To illustrate this newly defined way of reasoning, Peirce writes:

- (1) “The surprising fact, *C*, is observed;
- (2) But if *A* were true, *C* would be a matter of course.
- (3) Hence, there is reason to suspect that *A* is true.”

(*Ibid*)

In this example, *A* is the created hypothesis. In philosophy, especially in metaphysics or ontology, the proposition that articulates the created hypothesis, in the context of transcendental reasonings, is labelled as the ‘*synthetic a priori*’. This notion was first introduced by (see Tyfield, 2007). It designates a truth that is neither logical nor analytic (i.e., not a truth of reason). It is, however, an *a priori* truth as it is a reasoned *necessity* and it is *synthetic* for the mind plays an active role in the conceptualization of the premise (1) (Tyfield, 2007, pp. 154–156). Let us explore how this works in practice.

How is the abductive mode of reasoning used for ontological theorising? In Lawson’s language, *C* would be an entry point for ontological inquiry. In logical terms, *C* corresponds to the premise of the transcendental argument. If our starting point is a theory, then premise *C* would be a sentence of the theory. Indeed, for Quine and for Mäki,

the premises of their transcendental inferences are the sentences of the theories they picked as starting points. But what if the starting point is instead a generalized human practice or a generalized feature of experience?

Those ‘human practices or generalized features of experience’, do not constitute, strictly speaking, the starting point for transcendental analysis. As with any other argument, transcendental reasonings deal with propositions, rather than what propositions possibly refer to (such as those generalized features). Thus, the starting point of transcendental analysis, as hinted above, consists of descriptions or conceptualizations of those human practices or generalized features of experience. Prior to the transcendental reasoning, some sort of conceptualization of the entry point for ontological theorising must be done. As Lawson puts it,

“The premises of transcendental arguments will be *descriptions* of practices given prominence by influential, or potentially influential, science-oriented philosophies”

(Lawson, 1997, p. 50)

“The features of experience to be accounted for and *the way they are philosophically conceptualised* (i.e. the premises for the transcendental inference) may each be corrigible...”

(Lawson, 1997, p. 49 emphasis added)

Once the entry point *C* is given, the operation through which the conclusion of the transcendental argument is derived involves posing a *transcendental* question – a question that inquires about something not directly accessible, thus the label *transcendental*. Using Lawson’s words, the standard form for that question is:

“What claims about the nature of reality are viable in the sense that they render intelligible the generalized phenomena of experience taken as premises?”

(1994, p. 271)

Rephrasing it, how must the world be like so that *C*, a ‘surprising fact’ is observed? The answer involves asserting premise (2), which corresponds to the inferential step of the argument.

In sum, the transcendental reasoning is a type of argument, a tool that may be used to obtain ontological propositions from other propositions. These other propositions may be part of scientific theories, or propositions describing some phenomena of interest.

The notion of ontological commitment

In the above we discussed a form of reasoning that seems to be the most commonly used in the ontological research this chapter revised. As it is highlighted, it is a reasoning that, when used in the context of an ontological inquiry, allows us to retrieve an ontological proposition from other, non-ontological, propositions.

Implicit in the appropriateness of this form of ontological research is the notion that there exists some sort of relation, a link connecting the entry points – premises of the transcendental argument – to the conclusion, the ontological proposition. The existence of that link is hinted by premise (2) in the transcendental argument scheme: ‘But if *A* were true, *C* would be a matter of course’. Thus, uttering that *C* is true *suggests* the truth of *A*. Accordingly, we could call that link as a presupposition. Yet, it turns out that the interest here is in a very specific type of presupposition, an ontological presupposition in the sense that it refers to the nature of the world. As such, we can label the link between the premise of the transcendental argument and its conclusion as an *ontological commitment*.

The notion of ontological commitment is conceived in different ways by different authors, but a general overview of what is at stake is still possible. It follows from the above that studying the concept of ontological commitment is studying the nature of a very specific type of presupposition. Propositions, to the extent that they are about the world beyond them, have truth conditions. These correspond to the “demands that the sentence’s truth imposes on the world” (Rayo, 2007, p. 428) Ontological commitments are nothing but “those demands that concern ontology” (Ibid). For example, “for a sentence to carry commitment to *F*s is for the sentence’s truth to demand of the world that it contain *F*s” (Ibid). Different conceptions of the notion of ontological commitment stem from different interpretations as to the relevant truth conditions.

Let us first see how Quine tackles this issue. Quine's approach is predicated in a certain vision of what language is and how it works. For the purpose at hand, it is sufficient to assert that, for Quine, as far as ontology is concerned, meaning is of no interest, and reference takes a central role (Quine, 1948, pp. 5–7).

Scientific theories are sets of statements that account for past experience in order to predict the future (Quine, 1951, p. 44). Any theory must be about some objects, and those are the entities the theory *refers to*, which, in turn, are the entities the sentences of the theory refer to. If the theory is true, then the referents of the sentences of the theory, it follows, exist. Using Latsis's words: "the objects which any given theory is about must be those objects that are claimed to exist if the theory is to be true" (Latsis, 2007, p. 55). Putting it in a simpler way, a theory is committed to those objects whose existence is *necessary* for the theory to be true. This is the Quine's standard for ontological commitment.

The challenge here is that not all terms in a theory refer to something and therefore Quine has to come up with a way to adjudicate, in general, which terms really name something. To this end, Quine uses Russell's theory of descriptions. Briefly, Russell's theory guides us in paraphrasing terms by descriptions. To illustrate: "The sentence 'The round square cupola on Berkeley College is pink' is explained as 'Something is round and square and is a cupola on Berkeley College and is pink, and nothing else is round and square and a cupola on Berkeley College'" (Quine, 1948, p. 6). According to Quine, by paraphrasing terms by descriptions, "The burden of objective reference which had been put upon the descriptive phrase is now taken over by words of the kind that logicians call bound variables, variables of quantification, namely, words like 'something', 'nothing', 'everything'" (Ibid). Hence, reference is related to the concept of "bound variable". The use of bound variables, when coupled with the existential quantifier, commits the theory to the values those variables range over.

Formally, Quine's definition of ontological commitment is:

'X presupposes P if and only if P must be true if X is true'.

The nature of the necessity encrypted in the 'must' is, as Tyfield (2007, p. 147) points out, logical. This means that (the truth of) *X* logically entails the existence of *P*. This

means that among the consequences of X , there is the proposition ‘there exists P ’ (Michael, 2008, p. 47).

This account also points to the way one should proceed in order to retrieve the ontological commitments of theories. First, one must translate the theory into the language of first-order logic. Once that is done, the use of bound variables becomes apparent, and so do the ontological commitments of the theory.

Despite instructive, Quine’s approach can be very limiting. This is best explained through an example. In some theories of physics, a ‘frictionless plane’ is invoked. This entity is, of course, and is explicitly addressed as, ideal rather than real. The sheer use of an existential quantifier as the unique criterion of identification of ontological commitments seems thus to be misleading at times. Quine takes the ontological commitment criterion – the use of bound variables attached to the existential quantifier – as a sufficient principle for identification of what phrases are actually ontologically relevant. The frictionless plane of physics is just a counterexample. Contrary to what the blind application of Quine’s method would imply, the use of such entity in science appears to be of little *ontological significance* (as far as it concerns the existence of such entity). Notwithstanding, the fact that physicists *use* such an ideal entity and that such use is fruitful can tell us something about the nature of the world. Lawson and his approach are, in this respect, illuminating.

The way Quine pursues ontology is entrenched in a machinery of logical concepts about what language is, how it works, and how it should be harnessed in order to pursue ontology. So far as the apparatus is concerned, Lawson’s approach is, so to speak, in the other extreme.

Recall, Lawson takes as a starting point propositions consisting of conceptual representations of ‘features of experience’. Through transcendental reasoning, i.e., by asking and answering a question of the form ‘how the world must be like so that these features/practices are *intelligible*’, one obtains propositions constituting the conclusion of the transcendental inference which, furthermore, are supposed to be reliable accounts about the nature of the world.

This has an important bearing upon the conception of meaning and reference that this type of ontological research implicitly assumes; a conception that, it seems to me,

challenges Quine's understanding of the subject. There are many examples of meaningful propositions that do not refer whatsoever. 'The King of France is wise' is perhaps the most explored example. This example shows that the meaning of a sentence or proposition cannot be its reference: the 'King of France' does not exist and so the sentence does not refer. But (and this is the crucial idea) though the 'King of France' does not exist, he could exist, were France a monarchy. Therefore, 'The King of France is wise' does not refer but it *could* refer and that is why, so the argument goes, the proposition is intelligible. Meaning is thus the possibility of reference. But reference, in turn, requires a meaningful proposition. To explain this very last point, any example of a meaningless proposition would do the job; Tyfield puts forward the following: 'The king of Pope is wise'. This sentence is not meaningful for it cannot possibly refer. Meaning and reference are thus mutual conditions of possibility (Tyfield, 2007, p. 154).

It, therefore, follows that the starting point for the transcendental inference, say the proposition *P* describing some phenomena of interest – a generalized feature of experience – is intelligible. Now, if reality can be described by intelligible propositions, then reality is itself intelligible (this is Lawson's intelligibility principle in yet another formulation). Thus, it is possible to scrutinize its conditions of intelligibility, as 'demanded' by the standard transcendental question, which are simultaneously the conditions of intelligibility of reality. It thus follows that the ontological commitment criterion is of the form:

'P presupposes X if and only if X must be true if P is intelligible.'

That is, the ontological commitments of *P* are those propositions *X* that are *ontologically meaningful* conditions of intelligibility of *P* and the feature of experience *P* describes. Tyfield (2007, p. 153) labels the underlying type of necessity as *intellective necessity*.

This ontological commitment criterion is the conceptual link between human practices and the nature of reality. If a given practice is, under some description successful, the results of ontological inquiry consist of an account of the actual nature of the world.

Ontologically significant propositions

A sentence, or proposition, can be ontologically significant in two different senses. First, it can be significant in the sense that the proposition is itself an ontological proposition that can and should be taken as such: it articulates something about the nature, structures, or existents of the world. Second, a proposition can be ontologically significant in the sense that its commitments, or at least a subset of those commitments, are significant ontological propositions in their own right.

But how to identify ontologically significant propositions? Peter van Inwagen (1998, pp. 11–14) reflects on this issue by systematizing a set of criteria, a rule book that the ontologist can rely on to identify an ontologically significant utterance.

Let me introduce his list by means of an example. Consider, for example, that while looking outside the transparent window in my office, I utter: ‘there is no water, and the sun is already trying to leave’. It would obviously be strange, not to say absurd, to read this proposition as actually claiming that the substance water does not exist, or that the sun has some sort of conscious behaviour. Of course, anyone, however ill-trained in social interaction, will immediately recognize that I mean to *metaphorically* describe a *particular* affair within *my office* and its surrounding environment. Thus, the sentence refers to a *particular, localized* state of things in a *non-literal* way, and so, it is not ontologically significant.

Let us first focus on the first phrase of the sentence, i.e. ‘there is no water’. The sentence refers to a localized state of things and that can be understood from the context in which it is used. Indeed, the context places a ‘*restriction of intended reference*’ (Ibid, pp. 12). The restriction of intended reference entails that I am merely describing the non-existence of a substance – water, H₂O – within the confines of my office – and thus the sentence is not stating that water, the entity, does not exist at all, or that it is nothing but an illusion. This means that this proposition is not ontologically significant as I am concerned only with a small, very small, portion of the cosmos. In general, the context in which a proposition appears provides enough material to adjudicate whether there is any restriction of intended reference. So, the first condition is that the statement or proposition is free of any restriction of intended reference.

Now the second phrase, i.e. ‘the sun is already trying to leave’. Clearly, the meaning of this sentence is that the sunset is happening. Saying that ‘the sun is already

trying to leave' is simply a metaphorical way of stating that. This highlights an important aspect of language, which inevitably makes its way to scientific theories and scientific discourse. It is not enough to invoke a certain name in some theory to then conclude that it corresponds to a category in the world – recall the example of the 'frictionless plane' in physics. Very often, scientific theories make use of hypothetical entities; entities that are never meant or taken to correspond to real beings³⁴. This means that, whenever a proposition names a non-real, the literal truth of that statement is nothing the speaker would stand for. Thus, not everything that we state carries the same ontological significance. Not all our thoughts, utterances, and theories are meant to state, or to describe, events, states of affairs, or objects of the world as conveyed by the strict, literal meaning of the words used. So, the second condition is that the sentence or proposition is meant to carry its strict and literal meaning.

This criterion seems to pose a challenge, for instance, in the study of the ontological presuppositions of the mainstream of economics. For, regardless of whether the instrumentalist conception of scientific theories is explicitly endorsed or not, it is common to identify false propositions in those theories. These false propositions, if not purposefully included as such, are nevertheless recognized to be false. This means that those theories shall not be given a literal meaning, and for this reason, they may not be ontologically meaningful or significant.

Lawson, who, as we saw, studied the ontological presuppositions of the mainstream, did indeed overcome this. Recall that he located the ontological presuppositions of the mainstream by looking at the methods the mainstream economists deploy. This means that, in the context of our language, while the propositions of the mainstream theories may not be ontologically meaningful, for they are not meant to be literal, their meta-theory and its propositions are indeed ontologically meaningful, and therefore, suitable and justified starting points for ontological inquiry. This is nothing but an example of the application of the criteria this section set out.

Finally, metaphysical statements have to be of a high level of generality. How high is not easy to establish as measures of generality are only relative. If I utter the

³⁴ The use of hypothetical entities shall not be confused, however, with hypothesising or postulating entities whose existence has not yet been confirmed.

sentence ‘all trees are mortal’, and if there is no restriction of intended reference – so that the sentence applies to all trees, not just, say, to the sycamores outside my office, and if my intention was to state the strict and literal truth (there is no figurative language), is this a general enough proposition? Probably not. It tells very little about reality. After all, the category ‘tree’ concerns only a very narrow category of existing things. A different case would be if the sentence was ‘all living beings, by their very nature, will die’. In this case, the statement is general and describes the nature of the category ‘living beings’. Such judgment is, in any case, difficult to make. Peter van Inwagen (1998, pp. 11–14) suggests that, heuristically, there are some categories that given their generality we can use as marks of generality in the statements in which they are used. Those are, for example, causality (or cause), event, property, etc. An exhaustive list of those sufficiently general categories is, however, uneatable.

All this discussion applies to the entry points of ontological research and helps in judging whether the results of the transcendental reference are ontologically meaningful propositions.

What Follows? A brief ‘how-to’ guide on how to do ontology

To close this section, the following paragraphs systematize the main insights on how to do ontology, taking the opportunity to say something about the applicability of each criterion of ontological commitment.

The first task before the ontologist is to find an appropriate starting point for his analysis. Strictly speaking, that must be a proposition. So, either the starting point is itself already a proposition, or it is not – if it is, for instance, a human practice – a proposition describing the entry point needs to be articulated.

Now, if the starting point is a theory, and one is interested in uncovering the ontological commitments of that theory then one should look at the propositions of the theory. The relevant criterion of ontological commitment is Quine’s, recall:

‘X presupposes P if and only if P must be true if X is true’.

In fact, the ontological commitment criterion associated to Lawson seems to be inappropriate here. For a theory is intelligible if its propositions are intelligible, but that is simply to say that they are possibly true or false. However, neither is one interested in

the conditions of intelligibility of the theory, nor in the conditions of intelligibility of the belief that the theory is true, but in the ontological presuppositions of the sentences of a theory and to uncover them one has to take as axiomatic – as a working hypothesis of sorts – that the theories one is working with are true. As such, Quine’s criterion for ontological commitment seems to be more relevant. Importantly, however, we have to distance ourselves from Quine’s vision of meaning and reference, for it implies that commitments are a feature of actual reference and we may be dealing with a (believed to be) false theory. In any case, the relevant transcendental question is of the type:

How must the world be like so that the theory is true?

Then, one has simply to answer the question with an ontological meaningful proposition. It may, however, be the case that multiple and contradictory ontological meaningful propositions ‘make’ the theory true. In such cases, one should prefer the simpler answers, in accordance with Ockham’s razor.

If the starting point is a human practice, then, as already hinted, the appropriate criterion is Lawson’s,

‘P presupposes X if and only if X must be true if P is intelligible.’

In general, the transcendental question to be posed is of the form,

How must the world be like so that this practice is intelligible?

Again, the answer must be an ontologically meaningful proposition, and the Ockham’s razor should be always kept at hand.

2.5 Ontology in Practice

The last section elaborated upon the way ontology can be pursued. The elephant in the room, however, is related to a different question. And that is, how, if at all, can ontology be relevant to economics? Can academic and non-academic economics benefit from an explicit and systematic concern with ontology? If so, how? In attempting to answer these questions, I will touch upon some matters hitherto purposefully undeveloped. So far, the approach to, and the possibility of, ontology in the social domain, were discussed through the lens of Mäki and Lawson. The goal was to elaborate on the scope of, and approach

to, ontology. To a certain extent, it was a discussion of the *form*, i.e. of the theory of how to do ontology.

In this section, I will not focus on how to do ontology in economics, but rather on what ontology does for economics. Thus, I am going to analyse contributions to ontology of a more *substantive*, rather than *formal*, character. Taking advantage of the wider range of contributions of this kind (when compared with those of a more *formal* character), I will pick contributions by Mäki and by Lawson, but also by other authors³⁵. Rather than focusing on the specific ontologies or conceptions that those texts articulate; my focus is on how economics benefits from them³⁶.

Ontology, Heterodox Economics, Theory Choice, and History of Thought

Let me start out by the study of the discipline of economics as such. An interesting feature of economics (and, for that matter, of any other science) is the co-existence of different traditions of thought. These different traditions of thought tend to materialize essentially, but not exhaustively, in different interests, different approaches, and most of the times, in different results (including policy implications). As a result, many commentators are tempted to rely on those different interests, approaches, and results when providing an account of heterodox economics (see Roger Backhouse, 2000; Lee, 2009).

The problem, however, is that those types of demarcation do not seem to be universally valid; they pinpoint and articulate existent differences, but they are easily shown to break down in the face of the particular case of some economist or contribution (Lawson, 2006). Also, the fact that mainstream economics does seem to change over time (Colander, Holt, & Rosser, 2004) adds up to the challenge of establishing what exactly heterodox economics opposes to. It is therefore not surprising that commentators often note that the specific nature of heterodox economics as a whole, and the nature of each school of thought, in particular, is underarticulated, or that disagreement on these matters prevails.

³⁵ These authors are nevertheless more or less inspired by Mäki and Lawson.

³⁶ This section is not intended to be representative of this type of work. Rather, the selection was made bearing in mind the message I want to convey.

In the face of this, Lawson (2006) adopts an innovative strategy to study heterodox economics and provides an analysis of the existence of different heterodox traditions. His main point is that the opposition of heterodox economics to the mainstream is, ultimately, ontological in its nature. Economists commonly identified as heterodox, even if they are not aware, are so because they oppose the ontology underlying mainstream economics, even if they do not fully internalize the consequences of their beliefs (*Ibid*, p. 493).

This way heterodox economics is characterized by a different ontological vision from the one underlying the mainstream. Lawson provides evidence on that, but it may be interesting to look at what, independently, others have said on this matter. Mäki, for instance, backs, indirectly and implicitly, Lawson's argument by providing an analysis of the ontological commitments of Austrian economics (U. Mäki & Caldwell, 1992; U Mäki, 1990a, 1990b). Implicitly, those ontological beliefs are in opposition to the mainstream ones. For instance, he discusses the nature of money behind Austrian theorising, proposing that it is conceived as a collection of causal powers. Entrepreneurship – a central category of Austrian thinking – is, in Mäki's reading, also conceived as a particular causal power. Moreover, Austrian economists are committed to the view that the fundamental objects of economic theory are subjective: beliefs, preferences, expectations, and so forth (U Mäki, 1990a).

In the same vein, Jack Vromen (2001) studies the ontological commitments of evolutionary economics. His main findings are related to conceptions of human behaviour. The behaviour of economic agents is conceived as resembling the operation of a computational algorithm. Moreover, agents are viewed as bounded in their rationality and as the product of their own past. Questions of identity are also addressed. As agents are distinguished by their routines, and those, in turn, signal the agent's properties, which possibly evolve over time – by pre-specified paths at the theory level – today's agent is a function of her/his past.

And this list could go on, but the point is now clear. Heterodox economics opposes the mainstream essentially on ontological grounds, but each school of thought frames that opposition against one specific aspect of the mainstream ontology (Lawson, 2006, pp. 498–501).

Given the above, it is natural to assume that economists develop theories or choose to endorse particular theories, taking into account, albeit implicitly, ontological matters. And in fact, there is evidence on this. Mäki (2001b) provides a historical analysis of the criticisms, and refinements, of the perfect competition model. This case study furnishes evidence that sometimes some economists, though unaware of this, employ ontological arguments in order to endorse, reject, or call for adjustments in, theories. As such, ontology plays a role in determining which theories get acceptance within the scientific community, but also what kind of theories are developed or in what ways existent theories are changed.

The topics of the last paragraphs can be viewed under their historical significance. In so far as the analysis of different schools of thought and the study of the determinants of theory choice touch upon ontological issues and this helps to explain the evolution of the discipline, we are in the field of the history of economics. But there is yet another way in which ontology can contribute to the understanding of our discipline from a marked historical viewpoint. That is the interpretation of economic texts.

Let me cite some acclaimed examples where ontology played an important role in the interpretation of economic texts (see Lawson, 2015a)³⁷. Mário Graça Moura (2002, 2003, 2015, 2017) tries to account for the inconsistencies present in Joseph Schumpeter's writings. He argues that they are attributable to Schumpeter's commitment to a mode of explanation the implicit ontology of which is at odds with other, sometimes explicit ontological commitments of his. In a different context, Leonidas Montes (2003) shows that, although general equilibrium theorists seem to imply otherwise, the ontological conceptions underlying general equilibrium theorising are quite different from those of Adam Smith's famous invisible hand mechanism. Steve (Fleetwood, 2001a, 2001b) argues that Karl Marx's account of capitalist laws is not a deterministic account of the future paths of capitalist economies, as is often implied.

These examples show how ontological intervention can contribute to an enhanced understanding of economic texts. This may be particularly relevant when economic texts feature tensions and imprecisions. Equally important is that ontological insight may reveal previously unknown inconsistencies in substantive theories or even furnish the

³⁷ The referenced examples are sort of landmarks in (this kind of) ontological research.

explanation of the source of previously noted inconsistencies. This is one example of how ontological inquiries can be useful in economics. Specifically,

P 2.2.1: Ontology can help in accounting for the historical evolution of economics, while diagnosing and explaining tensions or conundrums in the discipline, in specific schools, or in specific theories.

In this proposition, ontology is conceived as an instrument of analysis. The history of economics deals with the succession or preservation of economic ideas over time. P2.2.1 thus suggests that the study of the ontology of economics, i.e., of the ontological presuppositions of theories, of the ontological beliefs and commitments of economists, and so forth, can contribute to an enhanced understanding of the reasons behind a pattern of succession or of preservation, including the emergence and dominance of economic ideas. The expression “tensions or conundrums” should be understood as explanatory failures, contradictions, paradoxes, or puzzles within scientific paradigms or practices and theories. Of course, if ontology is to play any role here, those “tensions or conundrums” must be of an ontological nature. These will often result from a lack of explicit and systematic ontological theorising or, at least, from faulty ontological theorising. This may result, for instance, in the use of different and incompatible explanatory categories simultaneously in the same text or theory. This represents just a possible pattern of inconsistency in economic texts, and more narrowly, a particular pattern of ontological inconsistency

Ontology, substantive theorising, and methods

Ontology can also be of relevance for substantive theorising. In this respect, and perhaps because the literature on economic ontology is still scarce, most contributions have taken the form of an ‘underlabouring’³⁸ for theorising. This underlabouring consists in clarifying concepts and the nature of social objects often employed by economists in their theories. Let me provide some examples³⁹. Philip Faulkner, Clive Lawson and Jochen Runde (2010) and Faulkner and Runde (2013) discuss the status of technological artefacts. They argue that technological objects are to be conceived as material artefacts

³⁸ I borrow this expression from Lawson (2015a).

³⁹ These examples are often cited in contexts similar to the present one. They are, again, kind of landmarks in ontological research.

created with the aim of extending human capabilities. Their conclusions can be fruitfully used in settling disputes and definitional issues in research on technological development. With a similar approach, Lawson provides an account of the nature of gender (2015c); of corporations (2015d); of money (2016, 2018); or of institutions (2015e). These kinds of contributions provide raw material for further theorising. Thus, for example, one can use the ontology of money as expounded in Lawson's (2016, 2018) writings as a building block in some pure, or applied, (substantive) theory.

The ontological elaboration of these key categories of economics is something not yet fully explored, which leads Lawson to note that scientific social ontology focusing on the study of the nature of specific economic categories is something that has not yet been systematically addressed (see Pratten, 2007, p. 62).

Turning to a more applied field within economics, Nancy Cartwright (1988) compares two traditions in econometrics: the one propelled by the Cowles Commission, and the one associated with David Hendry and his followers, often referred to as the 'British Tradition', or the 'LSE approach'. Of course, both approaches agree on many matters and disagree on others. Cartwright focuses on ontology, in particular, on the notion of causality underlying each approach. She shows that, while the Cowles Commission endorses a procedure supposing a world of capacities, Hendry and his followers are not committed to a notion of causality along the same lines. For this reason, it would be unjustified to look at the results obtained by Hendry and his followers and read any statement about the role that separable and individual causes (may) have in the phenomenon addressed (Ibid, pp. 196–197).

Cartwright's contribution is an example of how ontology and method are connected. Of course, whether capacities exist or not – and so, perhaps, which approach to prefer – is something to be addressed at the level of philosophical ontology. Cartwright's purpose is not really to discuss that – although her position is easily retrieved from the text – but instead to show the different ontological commitments of two approaches to econometrics and their respective methods.

Here, ontological analysis is being used in connection with the study of methods. The point is, as Lawson suggests, ontological insight provides material to an informed

selection of methods of research, a point he has been making in almost all his contributions.

Perhaps the main insight of these previous paragraphs is that ontology is of relevance in setting out a path for future research endeavours. That is,

P 2.2.2: Ontology can provide a way forward for substantive theorising; To provide a way forward for “substantive theorising” means that ontology furnishes a meta-theory, a roadmap, or a mould that, once filled by “substantive theory”, constitutes the scientific output. While the ontological study of methods allows the description of the situations in which their application is appropriate, the ontological elaboration of categories of economics furnishes material for the creation of theories hinging upon issues the explanation of which may be framed in terms of those categories.

What follows?

The literature review on ontology just set out furnishes evidence on the validity of the two propositions this chapter deals with. Recall,

P2.1: Ontology is a feasible form of study within the economics discipline;

P2.2: Ontology is fruitful in the sense that economics can benefit from its systematic study;

On the one hand, there is no better proof that ontology is indeed a *feasible* form of inquire than to document actual ontological contributions to economics. On the other, by looking at what those studies have achieved, even if only briefly, it is possible to systematize what benefits can possibly accrue from ontological inquires to economics. As it was seen, those can be summarized in the following propositions,

P 2.2.1: Ontology can help in accounting for the historical evolution of economics while diagnosing and explaining tensions or conundrums in the discipline, in specific schools or in specific theories;

P 2.2.2: Ontology can provide a way forward for substantive theorising;

To summarize, the analysis of the practice of ontology illustrates that this form of study can help us to understand the landscape of our discipline and its evolution. It can provide accounts of basic economic categories which are central for economics theorising, and finally, it can provide insights into the nature of the methods we employ.

2.6 Fitting Ontology in Economics: A Summary

This chapter intended to fit ontology in economics by studying the feasibility and fruitfulness of ontology in economics, while, at the same time, developing an interpretative framework - tools for ontological analysis in economics.

The ontological problem – the problem that ontology addresses, and therefore a crucial element to understand whether ontology is feasible (the problem can be solved) and fruitful (solving the problem brings benefits beyond the process of solving that problem) – concerns the inquiry over what exists. There is, however, a crucial difference when we speak of the ontological problem for the object of social sciences. In the social world, questions of existence lack the relevance that they have in the natural world. Instead, for the very nature of the type of inquiry social scientists pursue, the ontological problem is more about understanding how the categories that are known to exist should be conceived and used in substantive theory.

Having thus set the problem that ontology seeks to illuminate, this chapter moved on to the study of Willard Quine. Quine believed that ontology was a legitimate and fruitful form of inquiry. Legitimate because science and its theories can be used in philosophical inquiries in general, and in ontology in particular. Those theories have ontological commitments that can be easily retrieved once the theory is translated into a first-order logical language. Fruitful because, so envisioned, the study of what exists really tells us something about the world.

As it happens, Quine's revival of ontology was important as it indirectly paved the way for the emergence of ontology in other fields. In economics, Mäki and Lawson are the most influential contributors to the discipline of economic ontology. Mäki places a heavy emphasis on the study of the ontological presuppositions of economic theories and the ontological beliefs of economics. Lawson, instead, approaches ontology by studying generalized human practices, including scientific methods and approaches to science.

Among other things, the exercise of reviewing these authors' approaches enables me to compile a number of concepts that seem central in doing economic ontology. Those concepts are the transcendental inference, or transcendental reasoning – a form of reasoning that can be used to derive ontological propositions – the associated notion of

ontological commitment, and a three-criteria list of the typical features that both starting points for ontological enquiry and Ontological propositions feature.

Finally, and to complement the discussion, this chapter also revised some works in economic ontology of a more substantive, rather than programmatic or methodological nature.

Overall, this chapter established, recall, these propositions, P2.1: ‘Ontology is a feasible form of study within the economics discipline’; and P2.2: ‘Ontology is fruitful in the sense that economics can benefit from its systematic study’.

The most basic model of economic dynamics is the neoclassical model of capital accumulation. [...] As such it is natural to consider it as the benchmark model for our understanding of economic fluctuations as well as growth.

(Plosser, 1989, p. 54)

3 Real Business Cycles and the Neoclassical Theory of Capital

“Real business cycles” is the label adopted to refer to a wide range of dynamic theories that account for short-run macroeconomic fluctuations by relying on some mechanism other than a monetary one: the role of technology, of consumer tastes or preferences, or even fiscal policy. This chapter presents the basic structure and features of this type of theories, with a special focus on the underlying theory of capital.

Indeed, the main point of the chapter is to show that real business cycles theories rely on the concepts and results of the traditional neoclassical theory of capital. Those results can be obtained in the typical approach through which capital is brought into the scene in mainstream macro models, i.e., through an aggregated production function with homogeneous capital.

Section 3.1 articulates the scientific context of the emergence, in the 1980s, of this type of theories. Providing this context is relevant, as it helps to understand some options made by economists contributing to this line of theorising. This understanding, in turn, provides a more complete picture of the structure of these theories. Moreover, by doing so, it becomes easier to understand why this kind of theories gained prominence in the field. Section 3.2 presents a verbal introduction to the model, complemented, in section 3.3, by a canonical, textbook version of real business cycles theory. Despite the existence of numerous variants, each one with its own peculiarity, the main mechanisms and features can be easily captured in a relatively simple setting. In addition, the option to rely on what I call a canonical, textbook version also saves the text from unnecessary complications and complexities, which ultimately are not important for the main argument of this thesis. Of course, when relevant, references to more complex versions of the model are not avoided. Section 3.4 studies the capital theory foundations of real

business cycles theories. It does so by expounding the traditional neoclassical theory of capital, which is then shown to be the way capital is incorporated in real business cycles theories. Thus, section 3.3 and 3.4, taken together, collect evidence for the following proposition: ‘real business cycles theory crucially relies on a specific, traditional version of the neoclassical theory of capital.’ Section 3.5, in turn, points out that the traditional neoclassical theory of capital is not the only way to deal with capital within a neoclassical framework. This is done by simply describing an alternative theory of capital which is built on a different conception of capital. Thus, this section establishes the proposition that ‘within the neoclassical thought there is an alternative theory, or way of dealing with, capital, viz. ‘Walrasian capital’’. Section 3.5 thus raises the question as to why the traditional version of the theory of capital was the selected framework to integrate capital into real business cycles theory. Hence, section 3.6 systematizes possible explanations for that. Part of those explanations can be located in the intellectual context out which those theories emerged, and thus this section picks some threads of the arguments in section 3.1 to account for the option to rely on the traditional neoclassical theory of capital in real business cycles theorising. Finally, Section 3.7 concludes.

In sum, this chapter studies the theory of capital underlying the real business cycles paradigm and contrasts it to the other neoclassical alternative framework for capital. The two main propositions addressed are:

P3.1 Real business cycles theory crucially relies on a specific, traditional version of the neoclassical theory of capital.

P3.2 Within neoclassical thought there is an alternative theory, or way of dealing with, capital, viz. ‘Walrasian capital’.

3.1 The Monetary Theories of the ‘70s and the Real Theories of the ‘80s

The dismissal of Keynesian economics

Our story begins in the 1970s. It was about this time that the accumulation of critiques of the so-called Keynesian models, dominant in mainstream circles in the 1960s,

together with a renovated interest in the study of economic fluctuations, conjured to spur a new chapter in mainstream macroeconomic research. As Plosser notes,

“The return of the business cycle in the 1970s after almost a decade of economic expansion, and the accompanying high rates of inflation, came as a rude awakening for many economists. It became increasingly apparent that the basic Keynesian framework was not the appropriate vehicle for understanding what happens during a business cycle nor did it seem capable of providing the empirically correct answers to questions involving changes in the economic environment or changes in monetary or fiscal policy. The view that Keynesian economics was an empirical success even if it lacked sound theoretical foundations could no longer be taken seriously.”

(Plosser, 1989, pp. 51–52)

The failure of Keynesian economics that this quotation notes was denounced essentially in two works, one focusing on theoretical issues and the other on the methodological dimension of the Keynesian paradigm. At the same time, Keynesian economics was also being criticised for its (poor) empirical performance (see, for instance, Robert Lucas and Thomas Sargent (1979))⁴⁰. I shall focus on the methodological and theoretical problems of Keynesian economics as understood by the economists responsible for the emergence of the new paradigm that would eventually lead to real business cycles theories.

Firstly, in his American Economic Association presidential address, Milton Friedman (1968)⁴¹ articulated an idea which undermined an important result of Keynesian economics. Traditional Keynesian analysis indicated that higher levels of (aggregate) demand would correspond to higher levels of economic activity, higher levels of output, and thus a lower level of unemployment⁴². Friedman demonstrated that, when the long run is considered, this proposition is incompatible with the idea that agents' behaviour is

40 Lucas and Sargent (1979, p. 6) note that Keynesian models predicted that high levels of inflation would be associated with low levels of unemployment. The economic events of the 1970s simply refuted that.

41 The presidential address was delivered in 1967, and it was published the following year in the *American Economic Review*. For an account on the impact that Friedman's address might have had in macroeconomics see Mankiw and Reis (2018).

42 Lucas and Sargent (1979) show that such relation lacks empirical support. Friedman (1968) theorises a mechanism implying that such relation is not universally valid.

the outcome of some optimization. It could be the case that an expansionary monetary policy, to use his example, would, in the long run, lead to a situation of higher inflation, for the same level of unemployment (a vertical long-run Phillips Curve). This follows from the interplay between expectations of inflation, wage rate adjustments and actual inflation rates (Friedman, 1968, pp. 7–11)⁴³. In a word, the trade-off between inflation and unemployment breaks down in the long-run.

Lucas' (1976) argument is of a more methodological nature. Indeed, the way Lucas' insights were interpreted by the group of economists working on the theories this chapter focuses on confirms precisely the methodological nature of his contribution (see Duarte (2012))⁴⁴. Lucas reasoning runs as follows. Keynesian theory and the economic policy theory it feeds try to exploit a number of aggregate relations which are, Lucas reasons, ultimately non-stable. That is, the aggregate relations among aggregate variables, produced by the theory and empirically estimated, may change once a change in policy is taken into effect. That is so because agents may change their decision rules (i.e. rules that optimize some outcome) once a given change in policy is envisioned. The change in agents' decision rules may, in turn, change the parameters characterising relations among aggregates: aggregate relations before the policy and after the policy may be different. Thus, evaluating the effects of policy using previously estimated aggregate relations is a flawed procedure (Lucas, 1976). This is the famous Lucas critique.

It is interesting to note that both critiques, Friedman's and Lucas', are informed by conceptions about how individual economic agents make choices or formulate expectations. In other words, some premises of those arguments about the macroeconomy are based on the principles of (neoclassical) microeconomics. This, in a sense, constitutes an anticipation of how macroeconomics would evolve afterwards. Indeed, in the 1970s, the modelling of aggregates started to be explicitly founded in the behaviour of

43 Mathematically, this result follows if demand functions are homogeneous of degree zero (Lucas, 1976). In that case, if the income of the consumer and the prices of goods and services are both multiplied by a constant t , and everything else is kept constant, then the demand for goods and services remains at the same level. For this reason, to the extent that increases in the monetary growth rate are translated into proportional increases in (money) wages and in prices – due to the mutual interaction of these two variables – aggregate demand will end up at the (real) level of the initial situation. This mathematical property is, by the way, crucial in deriving the Monetarist proposition that, in the long run, real variables are independent of the monetary side of the economy.

44 “New classical, RBC and new Keynesian economists all worked in a similar fashion to address the Lucas critique by providing the kind of micro-foundations that nowadays characterizes not only their research programs but also the models of the new consensus macroeconomics” (Duarte, 2012, p. 9).

individuals. This involves the explicit consideration of the optimization problem agents are taken to solve, and the behaviour of aggregates then becomes the aggregated implications of those optimal decisions. This change in the way of doing macroeconomics was profound. Michel De Vroey (2012, p. 168), for instance, even argues that this change in the way macroeconomics was practised marked a “revolution”.

The monetary models of the '70s and the emergence of the real business cycles paradigm

The landscape of mainstream macroeconomic thought in the seventies included a group of economists working in the tradition of those who showed the flaws of traditional Keynesian analysis, commonly referred to as new-classical.

These economists both attacked traditional Keynesian economics and formulated an alternative. On the methodological side, they strove to fix the difficulties highlighted by the Lucas critique. This was done essentially by the formulation of models in which the choice problem that economic agents face is explicitly considered: models based on the neoclassical theory of choice augmented by the introduction of imperfect information and rational expectations.

On the theoretical side, these economists privileged the role of information in shaping the way policy affects the economy. Thus, Lucas (1972) emphasises the role of imperfect information in accounting for the impact of money on the level of economic activity in the short-run. In the long run, however, the effects of money would vanish as all the relevant information is progressively apprehended by economic agents and they get enough time to optimally adjust their decisions. In this view, monetary policy is effective only in the short run and provided that some sort of incomplete information exists. These contributions thus furnished theoretical support for the idea that, in the long run, there is no trade-off between inflation and output, which is equivalent to saying that there is a natural rate of unemployment⁴⁵. This idea was a central tenet of the macroeconomics of this period.

There was, however, an alternative in the making. Indeed, there was another group of economists who can be seen as the direct heirs of the traditional Keynesian theories of

⁴⁵ The main contributions are Lucas (1972), Sargent (1976), Barro (1976) and Sargent and Sims (1977).

the 60s. Parallel to the new-classical developments, these economists were working out ways of solving the problems Lucas (1976) pointed out (Duarte, 2011, p. 9) while preserving some Keynesian assumptions (as they were understood by mainstream economists). These economists kept emphasising the rigidity of prices, notably of wages, which meant that markets may not always clear, and also that monetary and fiscal policies are effective. The challenge these economists took up was to make the rational behaviour premises and results and the relational expectations approach compatible with the traditional Keynesian view of the economy. Later, this group of economists came to be known as new-Keynesian (Gordon, 1990)⁴⁶.

Robert Hall (1976) interestingly observed that most economists working in this latter tradition were based in (American) universities close to the coast, and labelled them the ‘saltwater’ economists, as opposed to, the ‘freshwater’ economists, who were based in universities close rivers or lakes and were working in the new-classical tradition.

Contrary to what this brief description of the two camps in mainstream macroeconomics might suggest, the progressive dilution of the influence of the monetary models of the 1970s was not due to the success of the saltwater approach. Instead, the monetary models of the seventies were crowded out by a different, emergent approach: the real business cycles paradigm.

In many analyses, new classical economists and real business cycles theorists are actually or implicitly grouped together (Blanchard, 2009; De Vroey and Malgrange, 2011; De Vroey, 2012). Prescott, Plosser and King were as “freshwater” as Lucas, Sargent and Barro were. In fact, Finn Kydland, who co-authored the seminal contribution in real business cycles with Prescott (Kydland & Prescott, 1982), often pays tribute to Lucas (Kydland, 2005). Also, Lucas and Prescott worked together in a couple of papers. (Lucas & Prescott, 1971, 1974). They were all, therefore, working in the same tradition. If anything, then, the substitution of real business cycles for the monetary models constituted an evolution rather than a revolution. This is indeed assumed by Prescott when, in a private letter to Lucas, he states that,

⁴⁶ The most influential contributions are Fischer (1977) and Phelps and Taylor (1977).

“much of what we are doing [at Minnesota] is working out the research program that you defined. I wish there were a noun for calibrate or a noun that captured the idea of being rigorous”

(quoted in Duarte, 2012, p. 225)

The question that these few paragraphs point to is why there was such an evolution of the new classical paradigm that consisted in the emergence and dominance of real business cycles. Interestingly, there is not yet a definitive history as to why the monetary models of the 1970s failed to play a long-lasting and central role in macroeconomic research. Indeed, there seems to be a lack of understanding regarding why real business cycles models effectively displaced the monetary models of the 1970s.

For this reason, I would like to sketch a non-exhaustive list of the factors that might account for the failure of the monetary models once real business cycles emerged. First, monetary models tended not to have good empirical performances. Moreover, with the emergence of the real business cycles paradigm, a new way of assessing the empirical performance of models began to get attention, and the structure of real business cycles models made them especially fit for that new method. Finally, real business cycles constituted a successful attempt to materialize a particular vision of economic modelling, a vision that was influential within mainstream/neoclassical circles. Let me briefly elaborate on these factors.

In the late 1970s, but especially in the early 1980s a number of studies started to question the empirical adequacy of new classical monetary models (see RE Backhouse, 2012, pp. 175–176). It was in this background that, since their inception, real business cycles models materialized a new vision which was to mould the way theoretical and, more to the point now, empirical macroeconomics was done. To the eyes of real business cycles economists, real business cycles modelling strategy rendered the traditional ways of doing empirical macroeconomics obsolete. As De Vroey notes,

“Kydland and Prescott (1991) transformed Lucas’s qualitative modeling [sic] into a quantitative research program – as (Greenwood, 1994, p. 1) put it, they took macroeconomics to the computer.”

(2012, p. 169)

This “quantitative research program”, in its empirical dimension, is related to the ways macro models were to be empirically assessed. Typically, the empirical validity of models of the macroeconomy involved the estimation of the parameters of the relevant equations. This was the case for Keynesian models, and also for the monetary models of the 1970s (see Lucas & Sargent, 1981). Real business cycles theorists, on the contrary, seemed to prefer a different, innovative way to validate their models; a way they called calibration. This methodology consists in, given a model, specifying values of fundamental parameters of the economy, such as those describing preferences or technology,

“the specifications of preferences and technology are close to those used in many applied studies. This facilitates checks of reasonableness of many parameter values. Second, our approach facilitates the selection of parameter values for which the model steady-state values are near average values for the American economy during the period being explained. These two considerations reduce dramatically the number of free parameters that will be varied when searching for a set that results in cyclical covariances near those observed”

(Kydland & Prescott, 1982, pp. 1360–1361)

In other words, the fundamental parameters of the economy are set at the values that applied studies suggest, given assumptions regarding the shape of the relevant functional relations. In addition, the real business cycles model is set in order to be consistent with the regularities of long-run economic growth, that is the specific values of the fundamental parameters should be such that the empirical regularities of long-run growth are reproduced by the model. Once this is done, a computational experiment (Kydland & Prescott, 1991) is run and its output, a set of model-generated time series is compared to actual macroeconomic time series, with a special focus on the short run. The better the model, the rule goes, the closer the population moments generated by the model will be to those of the actual series.

This method differs from estimation for, briefly, estimation deals with data and statistical theory which, together with an estimator and a probability model, measure the ‘size’ of the relevant parameters⁴⁷. In the view of real business cycles theorists, calibration

⁴⁷ Calibration and estimation are indeed difficult if not impossible to distinguish see (see Hansen & Heckman, 1996; Kim & Pagan, 1995; Quah, 1995); and the ensuing controversy. Despite this conundrum, a lot of contributions have been made in the direction of interpreting what the nature of calibration really is

was superior to estimation, i.e., to ‘traditional’ econometric analysis, such as regression analysis⁴⁸.

Finally, the fact that imperfect information drives the cycle seems to be problematic for some economists (Hellwig, 2017). For, in that case, there is a gain in making all relevant information public, and therefore a market for information would emerge. The lack of an obvious theory on why such a market fails to exist undermines models relying on imperfect information mechanisms. Also, it is pretty implausible to assume that economic agents lack information about key macroeconomic variables, namely price levels or inflation.

Perfect markets, rational expectations, alongside perfect information seem to be the natural way of new-classical economists to think about the economy. This ideal seems to be implicit in the words of Lucas when he reports his impressions on the Kydland and Prescott (1982) model when it was (apparently) first presented in a conference, sponsored by the Federal Reserve Bank of Boston in 1978,

“Though I did not see it at the time, the Bad Peak also marked the beginning of the end for my attempts to account for the business cycle in terms of monetary shocks. At that conference, Ed Prescott presented a model of his and Finn Kydland’s that was a kind mixture of Brock and Mirman’s model of growth subject to stochastic technology shocks and my model of monetary shocks. When Ed presented his results, everyone could see they were important but the paper was so novel and complicated that no one could see exactly what they were. Later on, as they gained more experience through numerical simulations of their Bad Peak model, Kydland and Prescott found that the monetary shocks were just not pulling their weight: by removing all monetary aspects of the theory they obtained a far simpler and more comprehensible structure that fit postwar U.S. time-series data just as well as the original version.”

nature (Dawkins, Srinivasan, & Whalley, 2001; Gregory & Smith, 1990, 1991, 1993; Pagan, 1994).

⁴⁸ I learned in Duarte (2012, p. 225) that some of Prescott’s – one of the figureheads of the real business cycles movement – students reported a funny provocation that Prescott did to one of his econometrician colleagues. According to the story, Prescott posted a phrase on his office door, at the University of Minnesota, that went as follows: “don’t regress; progress.” This sentence sums up Prescott’s view that estimation should be replaced by calibration. As Duarte (2012, p. 199) warns, although calibration was the dominant approach, some economists working in real business cycles paradigm did in fact approach its empirics through estimation.

(Lucas, 2001, p. 28).

In a word, real business cycles provided a framework more appealing to the new classical ways of thinking and at the same time, made the reliance on imperfect information unnecessary. Research of business fluctuations thus moved away from considerations about the role of money or of monetary policy and dropped, for the most part, the assumptions of imperfect information:

“Under Prescott’s leadership, nominal rigidities, *imperfect information*, *money*, and *the Phillips curve*, all disappeared from the basic model, and researchers focused on the stochastic properties of the Ramsey model (equivalently, a representative agent Arrow Debreu economy), rebaptized as the Real Business Cycle (RBC) model.”

(Blanchard, 2009, p. 211, my emphasis)

3.2 Real Business Cycles: A Non-Technical Introduction

This section wraps up some ideas already introduced, though not elaborated upon so far. The purpose is to articulate the real business cycles approach in its main features. De Long and Charles Plosser (1983, pp. 39–40) conveniently summarize the object of real business cycles theory:

“The term "business cycles" refers to the joint time-series behaviour of a wide range of economic variables such as prices, outputs, employment, consumption, and investment. In actual economies, this behaviour seems to be characterized by at least two broad regularities: (1) Measured as deviations from trend, the ups and downs in individual series exhibit a considerable amount of persistence. Given that a variable is currently above (below) its time trend value, it tends to stay above (below) trend for some time. (This is a meaningful restriction only to the extent that deviations from trend form a stationary, zero-mean process.) (2) Most important, measures of various economic activities (e.g., outputs in different sectors) move together. At times when one measure is above (below) its trend, others tend also to be above (below) their trends.”

Putting it crudely, real business cycles theorists aim to reproduce, in an artificial, model economy, a certain pattern present in macroeconomic time-series. That pattern consists in the deviations of the actual realizations of macro time-series from an underlying trend. An economic cycle corresponds, in this view, precisely to that deviation from the trend; the concept of ‘cycle’, or of ‘economic fluctuation’, is therefore defined in terms of time-series.

Usually, trend movements are interpreted as long-run tendencies. From a neoclassical viewpoint, trend movements are under the scope of growth theory. As such, real business cycles theory puts forward the idea that questions of growth and short-run fluctuations can be addressed using the same type of framework. As Sérgio Rebelo (2005, p. 2), notes “it is possible to unify business cycle and growth theory by insisting that business cycle models must be consistent with the empirical regularities of long-run growth”.

This unification occurs at two levels. First, at the empirical level, the unification of growth and fluctuations is attained through the calibration of real business cycles models so as to generate results consistent with the facts of long-run growth (Kydland & Prescott, 1982, p. 1359).

Second, at the theory level, this unification is achieved by taking neoclassical growth theory as the starting point of a theory that concomitantly accounts for the trend behaviour of the economy and for the deviations from that trend. It follows from the neoclassical growth model that, in equilibrium, technological change is crucial to account for growth. real business cycles proponents suggest that it also drives short-run fluctuations:

“It is this emphasis on productivity changes as the predominant source of cyclical activity that distinguishes these models from their predecessors and rivals.”

(Stadler, 1994, p. 1752)

That is why the Solow (1956) model, augmented by a stochastic component affecting technology, and with explicit micro-foundations, anchors real business cycles models (Prescott, 1988, pp. 9–10).

This sets the agenda of the real business cycles approach. To repeat, it should account for the short-run fluctuations in the economy in such a way that those results are consistent with the regularities of long-run growth, including correlations (co-movements) in time series. The main analytical framework is Solow (1956) with explicit micro-foundations. This model is often referred to as the neoclassical optimal growth model, or the Ramsey-Cass-Koopmans model, as these authors made crucial contributions for its formulation (Cass, 1966; Koopmans, 1963; Ramsey, 1928).

But what are the main features of this analytical framework? Plosser (1989, p. 53) puts it very simply:

“Real business cycle models view aggregate economic variables as the outcomes of the decisions made by many individual agents acting to maximize their utility subject to production possibilities and resource constraints. As such, the models have an explicit and firm foundation in microeconomics. More explicitly, real business cycle models ask the question: How do rational maximizing individuals respond over time to changes in the economic environment and what implications do those responses have for the equilibrium outcomes of aggregate variables?”

The real business cycles economy is thus constituted by rational agents, usually referred to as households. Households have preferences, which are described by a utility function. In different settings, this function will include different arguments, but in general, those are consumption and leisure. Agents are rational, which means that they chose the levels of consumption and of leisure that maximize their utility.

Of course, the choice households have to make regarding the levels of consumption and leisure is subject to resource constraints. To afford consumption, the household must get an income, which may come from two different sources: participation in the labour market, or financial gains if the agent chooses to hold assets.

Firms are the agents of households in the production of goods, and they maximize profits. Firms have access to a production technology described by a production function relating the quantities of factors – labour and capital – to the level of output. That production function is subject to changes over time, which are interpreted as productivity changes.

The real business cycles framework is a dynamic setting. This means not only that households are taken to maximize their utility every period or over time, but also, and more importantly, that they have to take into account the possible implications of their present decisions in their future utility. As such, they form expectations about the future. How, then, do agents form expectations? This is easily solved in real business cycles models by assuming that agents are able to use all relevant information when estimating the future value of relevant variables. Moreover, it is assumed that agents are able to create expectations that will be correct on average and the average mistake of which will be zero, i.e., agents do not commit persistent errors. One way of rationalizing this is to assume that households know as much about the functioning of the economy as does the economist building that economy. That is to say, agents know the actual model of the economy and use it to form expectations. In equilibrium, these expectations will be confirmed, and for this reason, this kind of expectations are also called, self-fulfillment expectations. This hypothesis is, of course, known as rational expectations (Muth, 1961).

Finally, the economy of real business cycles models features perfect competition markets.

3.3 A Canonical Real Business Cycles Model

This section presents a stylized version of the real business cycles economy. By stylized I mean a framework including *only* the basic, fundamental elements of this approach. To be sure, there are hundreds of different models of this type, but what is relevant in this chapter (and in this thesis overall) is conveyed in the simpler models. Indeed, text-book versions of models have the virtue of making more explicit the key features of the theory/model in question while preserving, as much as possible, technical simplicity and analytical tractability. The model I present in this section closely follows the one in Wickens (2008).

The real business cycles model assumes a continuum of identical individuals of a unit mass. Each agent is endowed with one unit of time per period. The unit of time available for each agent may be divided between leisure and labour. This continuum can

be studied through a representative agent⁴⁹ who maximizes his own utility by choosing between leisure and consumption,⁵⁰.

Labour is supplied by households in the labour market, where a given wage rate is established in perfectly competitive conditions. There is also a market for capital where firms ‘meet’ households. The latter supply funds, the result of their savings, to firms so that they can acquire the capital required for production. This means that households’ savings are kept in the form of bonds – yielding a given return – which consist in debts firms owe to households. Since firms are owned by households – each household is endowed with the same amount of equity – they are also entitled to receive dividends from firms.

In sum, households are earning income from their labour, from the bonds they may hold and from firms’ distributed profits. Finally, technical possibilities can change over time. This will affect the productivity of factors of production and may have an impact on firms’ and households’ decisions. Markets are competitive and agents take prices as given. These paragraphs provide the gist of the model presented in this section. The study of the mathematical formulation of the model is the object of what follows.

3.3.1 The mathematical formulation of real business cycles theories

As stated above, households make consumption decisions, decide how much labour to supply, own firms and save in the form of financial assets. These decisions are modelled through a utility maximization problem.

The representative household is described by a utility function,

$$U_t = U(c_t; 1 - n_t), \quad (3.1)$$

⁴⁹ This follows from the second welfare theorem. If markets are competitive, there are no missing markets, no externalities and a finite number of agents, then the equilibrium of the model can be computed through the representative agent construct.

⁵⁰ By choosing the amount of leisure, the representative agent is also setting the amount of labour she will supply, given the endowed amount of time. By choosing her consumption, she is also setting her savings, for a given level of income.

Where c_t stands for the level of consumption at t and n_t is the number of hours of labour supplied at t ⁵¹. This utility function has the habitual properties: first and second derivatives exist and are such that

$$\frac{\partial U}{\partial c} > 0 \wedge \frac{\partial^2 U}{\partial c^2} < 0,$$

which says that the marginal utility of consumption is positive but decreasing in c , and

$$\frac{\partial U}{\partial l} > 0 \wedge \frac{\partial^2 U}{\partial l^2} < 0,$$

where l stands for leisure and, again, the marginal utility is positive but decreasing. This further implies that labour has a negative and increasing marginal utility. Finally, Inada conditions are also assumed to hold.

The representative household problem consists in selecting the optimal level of consumption (versus saving) and labour (versus leisure) in order to maximize the present value of expected utility:

$$Max_{\{c_{t+s}, a_{t+s}, n_{t+s}\}} V_t = E_0 \sum_{s=0}^{\infty} \beta^s E_s U(c_{t+s}; 1 - n_{t+s}) \quad (3.2)$$

where β is the discount factor: $\beta = \frac{1}{1+\theta}$, where θ is the rate of discount and $l_{t+s} = 1 - n_{t+s}$. The representative household is subject to the flow budget constraint given by

$$\Delta a_{t+1} + c_t = w_t n_t + x_t + r_t a_t \quad (3.3)$$

a_t is the net stock of financial assets at the beginning of t ; r_t is the interest rate on bonds, paid at the beginning of t ; x_t dividend income. At the beginning of t , a_t is given. In period t , households choose c_t, a_{t+1}, n_t , etc.

Assumptions about the properties of the utility function ensure that there will not be corner or negative solutions. Thus, we can formulate the household's problem by the usual Lagrange maximization:

⁵¹ So, $1 - n_t$ stands for leisure, given the 1 unit of endowed time.

$$L = E_0 \sum_{s=0}^{\infty} \{ \beta^s U(c_{t+s}; 1 - n_{t+s}) + \lambda_{t+s} (w_{t+s} n_{t+s} + x_{t+s} + (1 + r_{t+s}) a_{t+s} - c_{t+s} - a_{t+s+1}) \} \quad (3.4)$$

The first-order conditions⁵² of this problem are:

$$\frac{\partial L}{\partial c_{t+s}} = \beta^s U_{c,t+s} - \lambda_{t+s} = 0 \quad (3.5)$$

$$\frac{\partial L}{\partial a_{t+s}} = \lambda_{t+s}(1 + r_{t+s}) - \lambda_{t+s-1} = 0 \quad (3.6)$$

$$\frac{\partial L}{\partial n_{t+s}} = -\beta^s U_{l,t+s} - \lambda_{t+s} w_{t+s} = 0 \quad (3.7)$$

For $s = 0$, we have

$$\begin{cases} U_{c,t} = \lambda_t \\ U_{l,t} = \lambda_t w_t \end{cases}$$

It follows that

$$\frac{U_{l,t}}{U_{c,t}} = w_t \Leftrightarrow U_{n,t} = -w_t U_{c,t} \quad (3.8)$$

Recall that labour provides negative utility, which means that $U_{n,t}$ must be negative⁵³ and decreasing in n . Thus, the higher the wage rate, the more labour will be supplied by the representative household. Intuitively, the higher the opportunity cost of leisure is the more

⁵² Second order conditions for a maximum are also satisfied thanks to the assumptions made for the utility function.

⁵³ To see that this is true, just consider the right-hand side of the equation. The marginal utility of consumption is positive by assumption and the wage rate is positive as well. The minus sign makes this side of the equation negative.

willing the household is to give up leisure time. Once consumption is determined, we can derive labour supply as a function of consumption and the wage rate.

For $s = 1$, we have

$$\begin{cases} \beta U_{c,t+1} = \lambda_{t+1} \\ \lambda_{t+1}(1 + r_{t+1}) = \lambda_t \\ \lambda_t = U_{c,t} \end{cases}$$

It follows that

$$\frac{\beta U_{c,t+1}}{U_{c,t}} = \frac{1}{1 + r_{t+1}} \quad (3.9)$$

This equation implicitly defines the decision rule for setting the level of consumption in each period. It compares the present value of the cost of decreasing consumption today in order to increase it tomorrow with the opportunity cost of consuming today. The economic meaning of this expression is that the higher the interest rate is, the lower present consumption will be when compared with future consumption, *ceteris paribus*. The interest rate is the return on savings and therefore constitutes the opportunity cost of consuming today.

Let us now focus on the supply-side of the model. Firms are agents of households. They produce one single, homogeneous good that can be used as a consumption good or as an investment good. They decide the level of output, investment, and employment. Investment decisions are taken in order to assure the firm's capital stock has the optimal size. Households' savings finance firms' investments.

Firms face competitive markets in both final goods markets and input markets. Therefore, they take the price of the final good, the wage rate and the interest rate as given. They distribute profits to households.

The representative firm seeks to maximize the present value of current and future profits by choosing the level of output, investment, capital stock, labour, and debt finance. Being able to choose the level of debt means that firms choose their financial structure. Formally, the present value of their profits is given by

$$P_o = \sum_{s=0}^{\infty} (1 + r_{t+s})^{-s} \Pi_{t+s} \quad (3.10)$$

Where real profit is given by

$$\Pi_t = y_t - w_t n_t - i_t + \Delta b_{t+1} - r_t b_t \quad (3.11)$$

Here b_t stands for the stock of outstanding debt. It is further assumed that the price of the goods that firms are producing is equal to 1.

The representative firm produces according to the technological possibilities described by the production function

$$y_t = z_t F(k_t, n_t) \quad (3.12)$$

The law of motion of the capital stock is

$$\Delta k_{t+1} = i_t - \delta k_t \quad (3.13)$$

Using equations (3.11) and (3.12), we can rewrite equation (3.10):

$$\Pi_t = z_t F(k_t, n_t) - w_t n_t - k_{t+1}(1 - \delta)k_t + b_{t+1} - (1 + r_t)b_t \quad (3.14)$$

The firm's problem is thus given by:

$$\max_{\{n_{t+s}, k_{t+s+1}, b_{t+s+1}\}} P_t = E_0 \sum_{s=0}^{\infty} (1 + r)^{-s} [z_t F(k_{t+s}, n_{t+s}) - w_{t+s} n_{t+s} - k_{t+s+1}(1 - \delta)k_{t+s} + b_{t+s+1} - (1 + r_t)b_{t+s}] \quad (3.15)$$

The first-order conditions are given by:

$$\frac{\partial P_t}{\partial n_{t+s}} = (1 + r)^{-s} [z_t F_{n,t+s} - w_{t+s}] = 0 \quad (3.16)$$

$$\frac{\partial P_t}{\partial k_{t+s}} = (1 + r)^{-s} [z_t F_{k,t+s} - (1 - \delta)] - (1 + r)^{-(s-1)} = 0 \quad (3.17)$$

$$\frac{\partial P_t}{\partial b_{t+s}} = (1+r)^{-s}[-(1+r)] + (1+r)^{-(s-1)} = 0 \quad (3.18)$$

The most interesting feature of these first-order conditions is that equation (3.17) holds for any value of b , outstanding firm debt. This means that the level of debt does not impact on the value of the firm, for it has no impact on profits. The financial structure of the firm is thus irrelevant, and any value of b is consistent with profit maximization. This result has come to be known as the Modigliani-Miller theorem (1958).

We can derive the firm's labour demand and the demand for capital. For $s = 0$, equation (3.15) is just

$$z_t F_{n,t} = w_t \quad (3.19)$$

Where $F_{n,t}$ stands for the derivative of the production function with respect to n . That derivative is the marginal productivity of labour, which is also the labour demand function. It is negatively sloped, given the assumption of decreasing marginal product of labour. The representative firm will thus hire labour up to the point in which its marginal product equals the wage rate. Equation (3.18) can be supplemented with the supply of labour, given by equation (3.7) to yield equilibrium in the labour market.

Regarding the optimal level of the capital stock, for $s = 1$, equation (3.16) becomes

$$z_t F_{k,t+1} = \delta + r \quad (3.20)$$

This equation means that the optimal level of capital the firm should hire is such that its marginal product equals the marginal cost of hiring capital, determined by the cost of financing it – the interest rate – and the depreciation rate (the capital lost after one period).

Using the theorem of the inverse function

$$k^* = z_t F_{k,t+1}^{-1}(r + \delta) \quad (3.21)$$

Equation (3.20) simply gives the optimal level of capital. In order to assure that the level of capital is at that level, the firm has to invest in every period according to the following equation:

$$i_t = z_t F_{k,t+1}^{-1}(r + \delta) - (1 - \delta)k_t \quad (3.22)$$

So, in each period, the firm should invest to compensate the depreciated capital and, if needed, to adjust their level of capital to the optimal level.

Recall that, given the properties of the production function, equation (3.20) shows that the greater the interest rate r , the smaller the optimal stock of capital is.

In general equilibrium, households will save until the point in which the return on saving equals their rate of time preference, that is,

$$\theta = z_t F_{k,t+1}^{-1} - \delta \quad (3.23)$$

Finally, to close the model we just need to assume a process for technology. As is common, suppose the technology parameter follows an AR(1) process, with a zero-mean, uncorrelated error term:

$$\ln z_t = \rho \ln z_{t-1} + \varepsilon_t \quad (3.24)$$

where $E(\varepsilon) = 0, \forall t \geq 0$, and $0 < \rho < 1$.

Now the model is closed. To fully grasp the way real business cycles depicts business cycle phenomena we need to get the system of equations of the model, its equilibrium, and the laws of motion of variables through time:

$$\frac{\beta U_{c,t+1}}{U_{c,t}} U_{c,t} = \beta E_t U_{c,t+1} (1 + r_{t+1}) \quad (3.25)$$

$$y_t = z_t F(k_t, n_t) \quad (3.26)$$

$$k_{t+1} = i_t + (1 - \delta)k_t \quad (3.27)$$

$$y_t = c_t + i_t \quad (3.28)$$

$$i_t = z_t F_{k,t+1}^{-1} (r + \delta) - (1 - \delta)k_t \quad (3.29)$$

$$U_{n,t} = -w_t U_{c,t} \quad (3.30)$$

$$z_t F_{n,t} = w_t \quad (3.31)$$

$$a_t = b_t \quad (3.32)$$

$$\ln z_t = \rho \ln z_{t-1} + \varepsilon_t \quad (3.33)$$

Equation (3.28) gives the equilibrium condition for the goods market: at each point in time, all production is used either in consumption or in investment. Consumption is implicitly determined by equation (3.25) and investment by equation (3.29). The total production is given by equations (3.26) together with the random process for technology (3.33), the stock of capital (3.27), and equilibrium in the labour market. Equations (3.30) and (3.31)⁵⁴ determine equilibrium in the labour market. Equation (3.32) states the equilibrium condition for financial markets: all firms' debt is owed to households⁵⁵.

The equilibrium equations of the model allow for a computational simulation, which produces what is called “artificial data” consisting of series of the relevant macroeconomic variables – output, investment, number of hours worked (employment), etc – that are then compared with actual data. The closer the moments of these artificial data are to the respective moments in the actual data, the better.

⁵⁴ Supply of, and demand for, labour.

⁵⁵ This is a closed economy setting with just households and firms, and only households can hold bonds.

As might be apparent from the canonical example presented, productivity shocks trigger deviations of the relevant variables from their trend. In response to shocks in technology, optimizing agents may change their decisions. It is the implications of those decisions in terms of the aggregate variables that real business cycles theorists are interested in.

Consider a positive shock to productivity ($\varepsilon_t > 0$). Households will see their income enhanced for a given level of supplied labour (for the time being, suppose labour supply does not change). This is because an increase in z_t increases the marginal productivity of a given amount of labour, and thus the wage rate increases as well (equation 3.31). Given the properties of the utility function, households prefer to smooth consumption⁵⁶. As a result, the increased amount of income – and output – generated by the (positive) technology shock will be split between consumption and savings allowing simultaneously for relatively higher levels of consumption in the present and the future. If we allow for changes in the labour supply, then the household may find it optimal to decrease the number of hours of work. For, after all, the productivity of labour, and so the wage rate, is now higher meaning that the before-the-shock level of consumption can be afforded by working less time.

There is, however, a pressure for labour supply to increase. As a result of the technology shock, labour becomes more productive: each (additional) unit of labour is now capable of producing more output, and, as explained before, the wage rate will be higher. This means that the opportunity cost of leisure increases, so agents will tend to supply more labour and enjoy less leisure time. This implies a procyclical variation in the quantity of labour supplied and employment. This increase causes a further increase in the level of output. This latter effect constitutes an amplifying mechanism: the increase in employment after a technology shock increases the output, which was already above its trend (its long-run value).

Putting it differently, this effect on the labour supply is related to the intertemporal substitutability of labour. From the first-order conditions of the household problem, it follows that labour supply in two consecutive periods depends on the relative real wage. If households expect the wages to be higher in the future, then they will increase the

⁵⁶ The relevant property of the utility function is its concavity.

supply of labour in the future relative to the present. So, in the presence of a positive productivity shock, the economy gets temporarily more productive. That means that the economy is wealthier: households can now afford more consumption and, importantly, can do that while enjoying more leisure time. For the wage rate is higher now given the impact of the productivity shock in the productivity of labour. The intertemporal substitutability of labour, however, exerts a pressure to increase the labour supply. For, as the effects of the shock fade away, the wage rate decreases. As a result, households tend to increase the participation rate in the labour market in order to enjoy the temporarily higher wages which, by the way, can provide households with means to increase savings and also get an extra income while the interest rate is relatively higher.

In sum, there is a tendency to increase the supply of labour and a tendency to decrease it. The final effect, of course, depends on the specific values of the relevant parameters.

Now, as the marginal product of labour increases, so does the marginal product of capital. For a given rate of interest, firms will find it optimal to borrow more in order to acquire more capital (equation (3.20)). And, actually, more funds are available in the capital/financial markets thanks to the increased level of income of households, together with their desire to smooth consumption. In a sense, the fact that households have a preference to smooth consumption over time makes it possible for firms to increase their investment following a positive productivity shock.

The autocorrelation in the law of motion of the productivity shock makes the cycle persistent. There are many more possible amplifying mechanisms in real business cycles literature. There are, for example, versions of the model that consider a time-to-build structure for capital accumulation (Kydland & Prescott, 1982), which implies that only after a pre-specified number of periods will investment become new capital, ready to use as an input for production.

3.4 The Basic Neoclassical Framework and Real Business Cycles

The expression ‘theory of capital’ is outmoded; it is certainly less used today in the top economics journals than it was for the most part of the 20th century. Does this mean that

theories of capital are not as relevant today as they were to previous generations of economists?

Certainly not. It is arguably true that, currently, economists are not as interested in theorising about capital as they were some decades ago. Notwithstanding, theories of capital remain fundamental in economic theorising. The situation is now one in which theories of capital are used in connection to other, perhaps less abstract, topics. As such, fields such as growth theory, distribution and, of course, business cycles are applications of specific theories of capital. In a sense, therefore, at the theory level, the traditional hot topics in economics are derivative from theories of capital. This fundamental positioning of the theory of capital is an idea clearly shared by many economists (Bliss, 1975a, p. vii; Ferguson, 1969, p. 251).

This section aims to retrieve the theory of capital underlying real business cycles models: the so-called traditional neoclassical theory of capital. In so doing, it shows how the propositions of that theory of capital appear in the type of models the last section expounded. Thus, the following paragraphs establish proposition 3.1: ‘Real business cycles theory crucially relies on a specific, traditional version of the neoclassical theory of capital’.

But what exactly is a theory of capital? Saying that it is a theory which deals with capital is clearly insufficient. Such an answer may indeed even complicate things, as there is no consensus on what the nature of ‘capital’ really is. As difficult as it is, it is still possible to advance an abstract definition of what a theory of capital is, a definition on the same footing as, say, the definition of macroeconomics, or the definition of a theory of public finance. That might be, for instance, that,

“Capital theory is concerned with the implications for a market economy, for the theory of prices, for the theory of production and for the theory of distribution, of the existence of produced means of production.”

(Bliss, 1975a, p. 3)

In order to prove proposition P3.1, the exercise that lies ahead reverse engineers the real business cycles model, aiming at studying the way capital is treated in that family of models. The analytical core of real business cycles theory is the basic neoclassical framework, or the basic macroeconomic model in neoclassical economics, also known as

the *neoclassical optimal growth model*. Its proximate theoretical root is the Solow growth model (Solow, 1956, 1957) extended to explicitly include neoclassical decision theory to model, primarily, saving decisions. The explicit consideration of the decision problem of households delivers a model whose equilibrium is optimal in the sense of being the utility-maximizing equilibrium. This development benefited from the contributions of Frank Ramsey (1928), David Cass (1966), and Tjalling Koopmans (1963), but also of William Brock and Leonard Mirman (1972), who extended the optimal growth model through the inclusion of a stochastic parameter. To understand the theory of capital behind real business cycles theory, we need to understand the theory of capital underlying the neoclassical optimal growth model. This is what this section does.

I adopt as a working hypothesis that proposition P3.1 is true, i.e., that the traditional neoclassical theory of capital indeed underlies real business cycles theory. Thus, I study the traditional neoclassical theory on its own, apart from its application to the study of cycles, and then I simply show that its features – assumptions, theoretical resources and results – are present in real business cycles theory.

To expound the traditional neoclassical theory of capital, I take as a starting point the simple neoclassical model of pure exchange. Then, I study how capital and production are introduced in such a setting, a reasoning that allows me to derive the fundamental neoclassical propositions in the theory of capital. Later, the task is simply to show how those propositions, and the theory of capital overall, emerge in the typical real business cycles model.

3.4.1 Foundations of the neoclassical theory of capital

The neoclassical theory of capital extends the marginalist theory of value through the inclusion of capital goods, i.e., produced means of production, into an otherwise exchange-only theory. Therefore, the neoclassical theory of capital has its identifiable predecessors in the wake of the marginalist revolution and in the works of William Stanley Jevons, Carl Menger, and Léon Walras. In a nutshell, their great contribution was their articulation of a conception of value based on the subjectivism of individual preferences and the idea that the value of everything is determined at the margin. The analytical concept meant to capture those valuations is that of ‘utility’, which is in turn

dependent on scarcity. Nowadays, the complete theoretical framework those three economists contributed to is known as ‘pure exchange models’. In these models, production is absent and, *a fortiori*, capital is inexistent. Yet, they provide a framework for the introduction of production and capital, both because they constitute a conceptualizing structure to think about the economy and because their propositions constitute a guide, a metric to check whether the introduction of capital and production matches marginalist principles. Understanding the tenets of pure exchange models is therefore central to the understanding of the traditional neoclassical theory of capital.

The fundamentals: a pure exchange model

Pure exchange models deal with the phenomena of exchange, including the values at which goods are, if at all, traded. They are thus an application of value theory. The usual setting is one with n gross substitute goods which are endowed to individuals by nature – as there is no production, this is the only ‘income’ available. Each individual maximizes her own utility taking advantage from the possibilities of exchange, which create opportunities for arranging a consumption bundle in accordance with her given preferences.

To engage in exchange, any individual must give up some good she was endowed with. Exchange occurs in the market, and therefore, prices are taken as given, and only one price holds for each good. Prices are established in the market through a tatonnement process, and once equilibrium is reached, supply and demand are equal in every market.

From such a setting it is possible to show that (1) the scarcer a good is, the higher its price tends to be; and (2) an increase in the utility provided by some good leads to an increase in its price, *ceteris paribus*. These are the fundamental results of marginalist exchange theory, which shall not be contradicted once capital and production are brought to the scene.

It is possible to look at a pure exchange model in terms of exchange through time, where consumers rearrange their consumption flow. Rearranging that flow involves, of course, exchange: a given individual foregoes a certain quantity of a certain good at period t , in exchange for a given quantity of goods in any future date. Of course, this rearranging, to be acceptable to the individual, shall imply a greater utility. The same is to say that the

total (utility) value of the stream of goods provided by nature must be less (or equal) than the total (utility) value arising from the stream of consumption implied by the rearrangement through exchange. In other words, there must be a ‘return’ in exchange.

The founding idea of the neoclassical theory of capital is easily identifiable in a pure exchange model of the type just summed up. The idea is that by changing a given stream of consumption the total utility may be higher, something which results from the enlargement of the possibilities of consumption in the future thanks to a return (in this case, a return in exchange).

The early neoclassical theory of capital

In the history of economics, the crucial theoretical development that eventually led to the neoclassical theory of capital was the application of the marginalist principles to the study of production in what came to be known as the marginal productivity theory.

The marginal productivity theory is the theoretical instrument that allowed the introduction of production in the marginalist, pure exchange scheme. That is, it enabled the introduction of capital and production into an exchange model in a way consistent with the two propositions mentioned above. Moreover, it allowed the formulation of a specifically marginalist theory of distribution, something that was in the air since the inception of the marginalist revolution and the loss of relevance of the labour theory of value.

The marginal productivity theory was independently formulated in the aftermath of the marginalist revolution by John Bates Clark (1847-1938) and John A. Hobson (1858-1940). Clark’s treatment of the subject is arguably the most influential one; for that reason, this exposition closely follows his work⁵⁷. Clark’s formulation of marginal productivity theory involved a reformulation of the concept of ‘capital’ and of the conception of the production process that were influential until that time. By the late 19th century, and before Clark’s work, the most influential piece in the marginalist theory of capital was by Eugen von Böhm-Bawerk (1891). Briefly, Böhm-Bawerk, was associated with a wage-fund conception of capital and a temporal conception of the production process (see Stigler, 1941, pp. 192–211) and Clark disputes both ((Clark, 1888, pp. 99–

⁵⁷ The essentials of Hobson’s contribution to marginal productivity theory can be found in Hobson (1891).

105, 1894, pp. 66–67). But let us see how exactly Clark conceives capital and capitalistic production.

Capital and production

Let us begin by Clark's concept of capital, which can be found, for instance in Clark (1888, pp. 89–93). Clark notes that capital is of a double nature. On one hand, capital consists in the myriad of goods that are used in production. These are the capital goods – plants, machines, tools, raw materials, unfinished products, etc. On the other hand, capital can also mean the amount of value that resides in those capital goods. This is the '*pure capital*': productive wealth invested in specific capital goods, say plants, machines, tools, raw materials, unfinished products, etc. Capital is, therefore, both material and immaterial; both rigid and fluid; both uniform and diverse. By reasoning in terms of pure capital – as Clark does in most of his writings, especially in his fundamental arguments – we are conceiving of capital as a single, homogeneous factor of production.

Pure capital, contrary to the specific capital goods, is perfectly mobile. In fact, as a substance, or an amount of money, capital can flow towards whatever sectors its owners desire. Of course, the specific capital goods do not feature the same capability, and this is a crucial difference between concrete capital and pure capital.

Capital goods, if put to their proper use, generate a net return: capitalist production is more productive than non-capitalist production. This constitutes the economic incentive to divert money income from consumption goods in order to create new capital. The creation of these capital goods increases the total value of capital in the economy, i.e. the amount of pure capital increases. Once augmented, this pure capital lasts forever. It is a permanent fund of wealth. Obviously, during the production process, deployed capital goods wear out. Buildings deteriorate, machines and tools need partial or total substitution, and unfinished products effectively disappear once their production is over. Contrary to what happened when those capital goods were first created – when pure capital was augmented by the creation of those very same capital goods – this inevitable destruction of capital goods is not, however, destruction of pure capital. That is, while specific capital goods wear out and disappear, the total value of capital remains constant, provided – of course – that nothing else changes. Indeed, the idea that capital goods generate a net income means that, at the end of their productive life, capital goods have

generated more than enough return to guarantee their substitution. In a word, the maintenance of capital does not require any further abstinence (Clark, 1899, p. 80). Capital goods do change over time, both in terms of unity and quality/diversity. As Clark puts it, “the things, then, in which society invests its fund of productive wealth are changeful, while the fund itself is permanent” (1891, p. 301). In a more metaphorical analogy, he states,

“A water-fall consists of particles of water; yet what is said of the fall cannot be said of the water. The fall is stationary; the particles move. The fall is perpetual; the particles of water pass away to the sea and ultimately evaporate. So capital is perpetual; but the things that at any moment compose it soon pass away.”

(Clark, 1894, p. 66)

In a stationary capitalistic economy, production processes feature a constant flow of inputs and a concomitant constant flow of outputs. Production can thus be conceived as a synchronized process. Of course, individual goods, either capital goods or final, consumption goods, require time to produce – ‘Rome wasn't built in a day’. Though true, Clark regards this to be an irrelevant technical feature of production. (Clark, 1899, pp. 78–80). Even if it takes an amount x of time to produce good A , the production process can be set to yield a constant flow of good A , provided there is a constant inflow of the required inputs. The same is to say, at each point in time, a number of processes designed to produce A are started. The fact that a cork tree can only be harvested once every seven years does not imply that one has to wait seven years before getting another harvest; one can simply have distinct sets of cork trees, each set to yield cork in the desired year.⁵⁸

In sum, capitalistic production uses capital goods, whose expression in value terms constitutes pure capital, and it can be studied while ignoring the role of time. The entrepreneur organizes production, picking a combination of capital and labour compatible with the maximization of profits. To study the mechanisms determining which combinations of capital and labour are compatible with profit maximization, the allocation of capital, as well as its returns, I shall turn to the marginalist theory of production and distribution.

⁵⁸ This example is similar to, and indeed inspired by, the one Clark elaborates (1899, p. 79).

The marginal productivity theory of production and distribution

Clark considers the existence of two factors of production, capital and labour. Clark's theory of distribution can be interpreted as the generalization of David Ricardo's ideas regarding the nature of rent. In Clark's texts, however, rent no longer means solely the difference in productivity of land as in Ricardo's. Clark suggests that, just like different lands have different productivities, so do capital goods. For that reason, the income that each specific capital good generates may be called *rent*. Summing all rents of every capital goods, we get the total return on all capital goods. If we see this return as the return accrued from the wealth encrusted in those specific capital goods, then that return is called *interest*. In other words, concrete capital goods earn rents, while pure capital earns interest.

The law of final productivity determines the remuneration of capital goods. Just like the last increment of land determines the differential rent in Ricardo's theory, the added product of the last addition of a capital good to a productive process determines the earnings of that capital good as well as all others:

“In a general form the rent of any instrument equals the amount that it adds to the product of the industrial agents that cooperate with it. The earnings of all capital in concrete forms are gauged by the productive efficiency of those forms.”

(Clark, 1888, p. 44)

Moreover,

“What the owners of capital can force entrepreneurs to pay them, is determined by the final productivity of capital. Employers of capital must pay for the final increment of it just what that increment produces, and they must pay for all other increments at the same rate”

(Clark, 1899, p. 115)

This productive efficiency is not constant on the amount of capital goods deployed, though:

“We are familiar with the so-called law of diminishing returns, by which land under tillage rewards labor and capital less and less bountifully, as more and more labor and capital are used on a given area”

(Clark, 1899, p. 31)

“Let the labor supply remain fixed, and let capital increase, and each increment of the latter, as it enters the productive field, finds that it can create less than any of its predecessors”.

(Clark, 1891, p. 53)

The law of diminishing returns is universal – a ‘universal truth of economics’ as Clark often suggests – and operates through the crowding out of the factors that remain constant, in the case of Clark’s analysis, labour. Each entrepreneur then faces decreasing marginal returns on any factor, provided the other factor is held constant. Therefore, the entrepreneur will employ a factor up until the point where its return decreases just enough to make it not profitable to employ one more unit of that factor. In modern language, the relation between the marginal product of each factor and the amount of that factor used – a relation that Clark represents graphically – is an ‘employment function’.

So, under free competition, each factor is paid according to the respective marginal productivity. If after paying the wages and interest, the entrepreneur finds a residual, then she has made a profit. However, this does not occur under free competition for the existence of a positive profit would trigger entries in the sector where this profit is found, with the result that that profit would be pushed to zero. Factor remunerations thus exhaust the product.

The traditional neoclassical theory of capital

The marginal productivity theory is one of the main building blocks of the traditional neoclassical theory of capital. As we have seen, it accounts for the remuneration of factors of production and explains the determination of the employment level. The other building block is the neoclassical decision theory. This theory deals with the preferences of economic agents. As such, it is used to determine the demand for the various goods and services produced. Furthermore, neoclassical decision theory is also the basis for the explanation of the supply of capital, which is nothing but the savings of economic agents. In this respect, the crucial author to shape neoclassical theory was Irving Fisher (1906, 1907, 1930).

While Clark developed a marginal productivity theory of the interest rate, Fisher, building on Böhm-Bawerk's texts, tried to formulate a 'preference for time' theory of interest. It is postulated that individuals, everything else being equal, prefer present goods to future goods. The existence of a preference for time thus implies that the date at which consumption is done affects the present total utility of that consumption. It follows that one is only willing to give up from consumption in the present if, in exchange, she can get a higher consumption in the future. This is crucial to account for any intertemporal decisions, whether of saving or of investment. Similarly, Fisher formulated the problem in a way that remains influent: the determination of the time paths of consumption given preferences and technology possibilities.

This was the last necessary ingredient for a complete exposition of the traditional neoclassical theory of capital. The neoclassical theory takes the preferences of economic agents, the techniques of production and the endowment of factors of production (i.e., capital, labour and land) as given. Being 'given' means that those are not supposed to change as the economy adjusts towards equilibrium. They are the data of the theory. Then, the theory determines equilibrium, which corresponds to a system of relative prices and the associated level of output/income and its distribution. If the interest is in studying what happens to the economy when one, or more, of the data of the theory changes, then one simply has to compare the equilibrium associated with the new data with the equilibrium associated with the initial, original data.

Crucial to grasp the workings of the theory is to establish that to determine equilibrium, capital must be conceived as a single factor of production, that is, the relevant 'capital' that constitutes the data of the theory is, in Clark's language, pure capital. Of course, and as Clark teaches us, pure capital, or capital in value terms, is simply the aggregated value of the myriad of capital goods in existence. To be sure, capital in value terms is what constitutes the data of the theory, not the physically distinct capital goods.

Now, only by chance is this arbitrarily given bundle of physically distinct capital goods compatible with both the available techniques of production and the preferences of individuals⁵⁹. In equilibrium, however, the capital goods in existence have to be

⁵⁹ By determining which goods are (more) demanded, preferences of economic agents indirectly determine what techniques are used in the economy and thus the capital goods that are demanded.

compatible with the data of the theory. This means that while the value of capital is among the data of the theory, the specific capital goods that add up to that value change as the economy adjusts towards equilibrium, and therefore, cannot be taken as given.

Pure capital, contrary to the specific capital goods, is perfectly mobile. In fact, as a substance, or an amount of money, capital can flow towards whatever sectors its owners desire. Of course, the specific capital goods do not have the same capability.

Now, if pure capital is free to flow to any sector, virtually without any restriction, then it will primarily flow towards sectors in which the return is higher and will abandon the sectors which are relatively less profitable. Of course, this does not mean that machines, unfinished products, or buildings will physically change sectors whenever their current use becomes relatively less profitable. For concrete capital goods are most of the times specific to the production process they are currently deployed in and are not easily reconverted to other uses. What happens instead is that, as the pure capital invested in relatively less profitable sectors is freed in the form of returns to the capital goods, it is deviated towards other, more profitable uses. What flows is pure capital, not capital goods.

Given enough time, free competition, through the workings of supply and demand guarantees that the system features a uniform rate of profit – or interest rate – over the supply price of capital goods. For if that is not so, then there exist profit opportunities left unexplored, and that is inconsistent with equilibrium. As hinted above, that uniform rate of profit is achieved through an endogenous, though still implicit, process through which the less profitable capital goods are relatively less demanded (and so, less produced) than the relatively more profitable ones. In this way, capital changes its form: its physical composition adjusts. Since the new capital goods are produced using the same economic resources that had produced the old ones, this change in form brings no change in value whatsoever. It is worthy to quote Clark at length here:

“In the long run the market rent of most things conforms to a normal standard, as fixed by the element of cost. If the earnings of a ship are larger than those of a mill that costs as much, less mills are built and more ships. The competition of ships with each other then reduces their earnings to the standard that is maintained in other spheres of investment. It is the interest on the pure capital invested in an

instrument of production that determines its permanent or normal rent. Pure capital gravitates to the points of greatest returns; it seeks out and vests itself in concrete forms that, as tested by the rent formula, give the greatest earnings. The result is an equalization of the earnings of pure capital; and this is the primary law that governs the returns of productive wealth” capital and its earnings

(Clark, 1888, p. 125)

A uniform rate of profit thus assures that the specific capital goods in existence are the most profitable ones (given the available technology, and the preferences of consumers) and allows the theorist to reason about capital in terms of a single and homogeneous quantity of value, i.e., as a single factor of production⁶⁰.

“In attaining a simple formula that governs the rent, not only of land, but of every concrete instrument of production, we have incidentally attained an equally simple rule that applies to the earnings of all pure capital, whether it be invested in instruments ordinary instruments of production or in land itself. It is based on the equalizing action of pure capital; its earnings tend toward a universal level.”

(Clark, 1888, pp. 134–135)

“Instruments artificially made have a normal as well as a market rent. This is governed by the cost of producing them. Pure capital flows spontaneously into the forms in which it yields the largest returns, and reduces those returns to the level fixed by other instruments of equal cost. The tendency of interest on pure capital is toward a general level; and this tendency governs the returns of all artificial instruments of production”.

(Clark, 1888, p. 134)

In equilibrium, prices – the dependent variables – are such that the supply and demand are equal for every market in the economy, given the data of the theory (preferences, endowments and techniques of production). Reaching equilibrium takes time, but it is assumed that all conceivable adjustments which the economy has to undergo in order to reach equilibrium are fast enough so that the economy reaches equilibrium

⁶⁰ Similarly, the earnings of labour tend towards a uniform wage rate: “the earnings of capital tend toward equality; and, with certain important reservations, those of labor do the same”. (Clark, 1888, p. 125-126).

before any change in the data occurs. It is for this reason that the equilibrium this theory determines is a 'long-period' equilibrium. In the particular case of capital goods, that manifests itself in a uniform rate of profits.

Now, if the endowment of capital increases, the interest rate decreases and free competition will guarantee full employment. If the interest rate decreases, then entrepreneurs will be pushed to employ more capital in production, that is, to opt for more capital-intensive techniques of production, they always choose the least expensive methods of production. Second, an indirect effect, acting through the consumers, also pushes the economy to more capital-intensive techniques of production. For, if capital is now cheaper, then goods whose production is capital intensive will become relatively cheaper. Thus, consumers will purchase more of those goods whose production process is capital intensive and decrease consumption of other, labour intensive, goods. Thus, the supply of labour-intensive goods decreases in favour of capital-intensive goods. Thus, the economy substitutes capital for labour. This makes sense, as the decrease in the interest rate is accompanied by an increase in wages. This is the indirect mechanism of substitution that leads to an increase in the amount of capital employed in the economy following an exogenous decrease in the interest rate. Once all adjustments take place, a uniform rate of profits emerges and the economy has reached its new (long-period) equilibrium.

The propositions of the traditional neoclassical theory of capital

This is the simple version of the neoclassical theory of capital. Though simple, this framework is sufficient to derive the parables that, some decades after Clark's texts, Samuelson would explicitly identify as characteristic of the neoclassical theory. Given free competition, those are:

1. The real rate of return on capital, conceived as a homogeneous fund of value, is the rate of interest;
2. The real return on capital (the rate of interest) is determined technically, by the diminishing marginal productivity of capital.
3. There is an inverse, monotonic relation between the quantity of capital and the rate of interest.
4. The wage rate varies inversely with the rate of interest

5. The distribution of income is determined by relative factor scarcities and marginal products.

From these parables, it follows, net national product per worker, or the permanently sustainable consumption stream, varies inversely with the rate of interest.

It remains to be shown that these parables emerge in the more recent, and technically more sophisticated because formalized, versions of the neoclassical theory, of which the Solow model is the primary example.

Towards the mathematical theory of capital

Clark's style of argumentation is verbal: he created a verbal theory of capital. From his time on, developments in the neoclassical theory of capital were principally related to the formalization of those verbal arguments, so that the parables were given a mathematical formulation, or they were mathematically deduced. Such effort started in the 1930s, first in Microeconomics, and later in Macroeconomics. Regarding Macroeconomics, Fisher (1906, 1907, 1930) is arguably the precursor of the mathematizing trend in the theory of capital.

The most important element in the mathematical theory of capital is the production function. It is the mathematical artefact that describes the technological possibilities of firms, at the microeconomic level, or for the economy as a whole, at the macroeconomic level. In mathematical terms, it is a function that maps combinations of factors of production – inputs – to outputs.

Historically, production functions emerged in the context of the study of the conditions of production of the firm. At its inception, therefore, the production function was a *microeconomic* construct. But it eventually made its way to macroeconomics, essentially through the development of the mathematical theory of capital.

As Schumpeter notes, the roots of the idea of a production function are in the classical period, in the notion of the 'state of arts' (Schumpeter, 1954, p. 1032). Similarly, the idea of a mapping between inputs and outputs was present in the marginal productivity ideas of Turgot. Yet, the economist who first explicitly invoked the idea of a production function was Wicksteed (see Schumpeter, 1954, p. 1033). Indeed, he even used the mathematical properties of that function, namely the homogeneity of degree one, to show

that the aggregated output of the economy is exhausted by payments to the factors of production – there are no economic profits (Schumpeter, 1954, p. 1051). Other economists such as Edgeworth and Jevons also detailed explicit production functions.

The works of Wicksell, Böhm-Bawerk, and Clark implied the existence, at least at the theory level, of an aggregate, social production function (Schumpeter, 1954, p. 1030). In any case, the use of aggregated production functions was only popularized by the empirical works of Cobb and Douglas, who even heavily influenced the assumptions often made regarding the specific mathematical expression for that function.

The ‘Solow model’ and the later developments in investment theory

How is capital accumulated over time and how does that lead to income growth? This is the question that Solow (1956) set himself to answer. The model he proposed is nothing but the later development in the traditional neoclassical theory of capital. It generalizes that theory to the long-run, when accumulation of factors can occur, and so the stock of capital and the endowment of labour are not given. In the Solow model, therefore, the total value of capital is endogenously determined.

There is yet another difference between Solow’s model and the previous contributions to the theory of capital. A difference that, I submit, can only be fully appreciated once one understands the controversies in the theory of capital, which by 1956 were already building momentum. That difference regards the definition or conception of capital. The previous versions of the traditional neoclassical theory of capital considered the existence of physically distinct capital goods, whose aggregated value was named pure, or real, capital. This was Clark’s formulation. During the first half of the 20th century, however, the traditional neoclassical theory of capital progressively adopted a conception of capital exclusively in terms of a homogeneous aggregate, thus neglecting the physical nature of capital goods. Fisher and Knight were among the authors who particularly fostered that way of thinking about capital⁶¹.

As such, the Solow economy produces a homogeneous good that can be either consumed or accumulated to be used as capital input for the production of further units of the homogeneous good. Capital is thus a produced means of production. As such, in

⁶¹ (Fisher, 1906, 1907, 1930; Knight, 1936a, 1936b).

order to accumulate capital, the economy has to allocate part of its income to investment. The essential ingredient of this model is the production function,

$$Y = F(K, L)$$

Which verifies the following mathematical properties:

1. $f(0) = 0$
2. $f(\infty) = \infty$
3. Homogeneity of degree one
4. First and second derivatives exist and are continuous
5. First derivatives are positive and second derivatives are negative

The first two conditions simply state that inputs are essential for production and that production is productive, that is, additional amounts of inputs will always lead to additional amounts of outputs. Condition 3 is a necessary requirement for perfect competition and implies that all product is exhausted after the payment of wages and interest. Condition 4 assures the continuous substitutability of factors of production, that is, there are infinitely many techniques – combinations of capital and labour – to produce a given quantity of output. Condition 5 means that there are decreasing marginal returns which imply that, in perfect competition, as the use of each factor increases, its rate of remuneration decreases. All neoclassical propositions in the theory of capital are either contained or follow if the production function features these properties.

This is simply the acclaimed Solow (1956) model. It is in this form, adapted to explicitly include the choice problem that households face, i.e., the trade-off between consumption today versus consumption in the future, and a stochastic model describing the time path for technology (Brock & Mirman, 1972; Solow, 1957), that the modelling of real business cycles explicitly departs from. As the idea of an aggregate production function is predicated on the traditional neoclassical theory of capital, this theory of capital accumulation is anchored in the traditional neoclassical theory of capital. This, therefore, confirms the working hypothesis of this section, namely, that real business cycles crucially relies on a specific, traditional version of the neoclassical theory of capital (proposition 3.1).

It remains, however, to be explained how investment comes into the scene. Recall, the theory of capital presented initially abstracted from accumulation, and so investment

was beyond the scope of the theory. The introduction of accumulation and investment followed an aggregated approach. The challenge, then, is to fill this gap with a theory of investment that is compatible with both the traditional neoclassical theory of capital, i.e. consistent with the fundamental propositions of the traditional neoclassical theory of capital, and also consistent with the Solow theory of capital accumulation.

This challenge was taken up, among others, by Dale Jorgenson (1963). His model is concerned with the explanation of the demand for capital, which is a theoretical construct needed to derive the level of investment. Firms demand capital aiming at maximizing the present value of the net worth – the present value of the firm. The production possibilities of the firm are given by a neoclassical production function. For the most part, this is what enables Jorgenson to make this theory consistent with the neoclassical theory of capital along the lines implied by the previous paragraphs.

There is, however, an additional complication. Demand for investment is not directly retrieved from the demand form capital – even when the current stock of capital and its rate of depreciation are given. For a positive demand for capital may be associated with an infinite rate of investment: firms would instantaneously adjust their stocks of capital. This issue in the early neoclassical theory of investment has been addressed by considering that there are costs associated with investment, namely costs associated with the adjustment implicated by changes in the level of capital. The investment building block of real business cycles models, including the one presented in this chapter, is always a particular version of this setting.

In sum, the Solow model constitutes the definitive version of the traditional neoclassical theory of capital. Through a mathematical setting, it derives some of the old neoclassical propositions in the theory of capital, while others are simply assumed through conditions imposed to the mathematical properties of the production function. It is essentially this, traditional, neoclassical theory of capital that real business cycles theory relies on in dealing with capital. In the next section, I shall demonstrate that, still within a neoclassical framework, there is an alternative theory where capital is conceived in a rather different way. Importantly, this theory was around in the early 1980s, when real business cycles theory was being forged. As such, one can conceive the fact that real business cycles theory relies on one theory of capital rather than in another as a ‘choice’.

3.5 The (neo)Walrasian' Way of Treating Capital

Walras's theory of capital

Walras' theory of capital consists in the extension of his general equilibrium model so as to include produced means of production, i.e. capital: "all forms of social wealth which are not used up at all or used up only after a lapse of time" (Walras, 1874, p. 212). These produced means of production are described by a vector of physically heterogeneous goods. Every good used as an input to production is thus taken as a separate factor of production, just as labour or land.

This inclusion of capital in Walras' original system demands the inclusion of additional variables: the rate of net income (i.e., the interest rate), prices of capital goods, quantities of capital goods demanded and the amount of savings.

Income receivers in the economy maximize their utility and might allocate a portion of their income to saving. Walras conceives saving behaviour as a demand for a 'commodity', just like bread or housing. However, this commodity, unlike bread or housing, yields a return, or a perpetual income. As a result, from a mathematical viewpoint, the bundle of goods that maximizes the utility of a given agent may include units of that 'commodity', that is, of saving. Again, from those agents' viewpoint, that 'commodity' is homogeneous, it is simply a quantity of value: as long as it yields a return, it is unimportant whether savings are used to finance capital goods to be used in the bakery industry or in the construction industry.

That quantity of value then corresponds to the aggregated value of the capital goods acquired in the period using those savings. The fact that capital goods are heterogeneous cannot, however, be forgotten.

Walras approach to determine the existence of equilibrium for the economic system as expressed in his equations required, among other things, the amount of capital goods of each kind to be given, i.e. to be the data of the problem. As such, as the economy adjusts towards equilibrium, the quantities of each capital good cannot change. They are among the determinants of equilibrium and are taken to entail sufficiently persistent tendencies so that the economy converges to it. As it happens, one of the properties or conditions of equilibrium is that, given free competition, a uniform rate of profits over

the cost of capital goods will prevail. Now, only by chance, is the initial, arbitrary set of capital goods compatible with the equilibrium conditions as implied by the preferences of agents. Thus, in most conceivable cases, the trajectory towards equilibrium is accompanied by a change in the quantities of the various capital goods that existed in the initial situation.

There is thus a problem here. The data of the theory is not supposed to change as the economy moves towards equilibrium, and if that change does occur then it has to be exogenously determined and it defines a new equilibrium. Mathematically, this inconsistency manifests itself in two ways. First, if the initial stocks of the capital goods are not allowed to vary, the system becomes overdetermined. Second, if net savings are assumed to be zero, the system has no solution: not all markets clear (see Garegnani, 1990).

Hicks and a new conception of equilibrium

It has been argued that Walras himself eventually became aware of these issues in his theory (Eatwell, 1990, p. 252). It was not until the 1930s, however, that a way out of those problems was devised, being Hicks' (1939) work fundamental here.

If the source of problems in Walras' system stemmed from the incompatibility of having a given endowment of each kind of capital good as data of the theory with the condition of a uniform rate of profits, then, by eliminating one of them, the theory, including the conception of capital as a physically heterogeneous set of goods, is preserved.

The condition that was dropped was that of a uniform rate of profits over the supply prices of capital goods. As it happens, this entails a change in the conception of equilibrium. For clearly, the new equilibrium cannot possibly be a long-period equilibrium. Free competition – an assumption that remains – will, given enough time, equalize the rates of profits of capital goods (as described above). Thus, if the equilibrium does not feature such equality, it must be a 'temporary', or short-run equilibrium: sort of an intermediate point of equilibrium between the initial situation and the long-period equilibrium which prevails if there is enough time so that the effects of free completion can be fully manifested, given the data of the theory.

In this way, this type of models preserved the treatment of capital as a set of physically heterogeneous goods. These capital goods earn rents and their value is given by the capitalization of those rents. Moreover, equilibrium implies that there is the same price for the same productive service whether in the same or in different, industries.

There is, however, an additional complication. If the persistence of the forces that define equilibrium is missing, we end up with a theory that determines, first and foremost, short-period equilibria. Now, as the composition of the stock of capital adjusts from period to period, equilibrium might change from period to period, and that may imply changes in the vector of prices, from one period to the other, i.e., each equilibrium might have a different price vector associated. One implication is that (expectations of) future prices may influence current decisions. As a result, future states of the economy, in particular, future prices, may affect the present.

There are two ways in which this can be dealt with at the theory level. First, one can assume some model of expectation formation by economic agents, such as rational expectations. In this type of models, the economy converges to a *temporary equilibrium*. Second, alternatively, one can, so to speak, bring the future to the present by assuming that commodities differ not only in kind but also according to the time period in which they become available. On top of that, one can assume that there exists a market for every such commodity. This is the famous ‘complete markets’ assumption, which appears, for instance in Arrow and Debreu (1954). In these models, all contracts are made in the first period, and everything is thus settled in the beginning, and in the following periods, agents of the economy simply dedicate their time to fulfilling those contracts. In these models, we have an *intertemporal equilibrium*.

3.6 Clark-Knight-Solow versus Walras: Real Business Cycles Theory Taking Sides

The last section describes two possible ways of treating capital within a neoclassical framework. At the surface, those different ways of treating capital manifest themselves in a different conception or definition of capital: as a physically heterogeneous set of goods, or as a single, homogeneous factor of production.

In real business cycles theory the economy produces a single homogeneous good that can be either consumed or invested. The production of that homogeneous good requires homogeneous capital and (homogeneous) labour as inputs. The relation between those inputs and the output is given by a ‘well-behaved’⁶² production function.

The equilibrium of in real business cycles theory (i.e., the equilibrium of the model at each point in time) takes as data the preferences of households (given by a utility function), the technology (given by a production function) and the endowments of factors of production. These include labour and capital. Note that the endowment of capital that is among the data of equilibrium is the aggregate quantity of homogeneous capital. These homogeneous units correspond to an amount of value which is, in turn, encrusted in various, physically heterogeneous capital goods, as it is implied by the way these models are confronted with the data.⁶³ Strictly speaking, therefore, while the aggregate quantity of capital is taken as given for the determination of equilibrium, the quantity of each type of capital good is implicitly left to adjust to the conditions of equilibrium⁶⁴. These models thus determine (a sequence of) long-period equilibria⁶⁵. Thus, as propositions 3.1 puts it, real business cycles theory crucially relies on a specific, traditional version of the neoclassical theory of capital.

⁶² That is, a production function compatible with perfect competition and with decreasing marginal products. This is assured by the mathematical properties described above.

⁶³ A similar reasoning is put forward by Petri (2017, n.23). After calibrating and simulating the real business cycles model, the generated series for the endogenous variables are compared with their real counterparts. As it happens, the series for investment in the model is compared with the actual series of investment, which corresponds to the gross capital formation which is of course the result of the production and purchase of heterogeneous capital goods (machinery, software, buildings, inventory, and so forth).

⁶⁴ Of course, the total amount of capital – the aggregate value quantity – may change every period, but for each point in time, and thus for each equilibrium, that quantity is given: it has been determined by the previous periods saving and investment decisions and any current investment will only affect the stock of capital of the following periods.

⁶⁵ These models are often claimed to address short and medium run economic dynamics. This is not in contradiction with the claim that the equilibrium in these models is a long-period one. In a word, ‘long-period’ is not an antonym of ‘short run’ (or ‘medium run’). Short-run analysis is one that abstracts from the accumulation of factors (both capital and labour). That is in fact what a long-period equilibrium does: it takes as given the total quantity of factors (while allowing for the endogenous determination of the composition of capital). In medium run analysis capital is allowed to adjust, but not all adjustments necessary for a complete equilibrium are allowed to happen (e.g., only circulating capital can adjust). In the long run, the total quantity of capital can change. When the ratio capital/labour has reached an equilibrium (which happens in the long run) the economy is in a very long period equilibrium, or steady state.

The interesting question now is: why was one way of treating capital, and the associated theory, preferred to the available alternative? Some, partial and related answers can easily be suggested.

First, when compared to the physical, disaggregated heterogeneous concept of capital and its associated framework, the neoclassical theory of capital in its traditional versions, specifically, the version which takes the production function as the central concept, constitutes a technically easier alternative to implement in mathematical models. After all, to cite but one example, at the mathematical level, it is simpler to deal with one scalar variable – homogeneous aggregated capital – than with a vector with an arbitrary number of lines – equal to the number of heterogeneous capital goods to consider. I believe that that is precisely what is hinted by Plosser in one of the programmatic papers on real business cycles,

“The most basic model of economic dynamics is the neoclassical model of capital accumulation. While many readers may be familiar with some versions of this framework as a model of optimal economic growth—following the work of Cass, (1966), Koopmans (1963) and Solow (1956)—it is better viewed as a framework for economic dynamics (see Hicks, 1965, p. 4). As such it is natural to consider it *as the benchmark model for our understanding of economic fluctuations as well as growth.*”

(Plosser, 1989, p. 54)

At the same time, the production function happens to lend itself to straightforward empirical analyses. This is echoed, for instance, by Roger Backhouse: “the most dramatic productivity shock for a generation provided renewed scope for using Solow’s aggregative model and the associated notion of total factor productivity to work out why productivity had fallen so far: the need for a practical way to measure contributions to productivity growth trumped concerns with the conceptual precision” (2014, p. 266). Relatedly, an aggregate production function happens to conceptualize the possible sources of output variation, which in itself, facilitates the work of the econometrist. The idea is rather simple. The output is given by a production function, given the available inputs. Thus, variations in output can only be the result of variations in the amount of

inputs available, labour or capital. For illustration, consider the following assessment of the sources of variation of output:

“The key business cycle fact is that, at the higher frequencies, variations in the labor input account for most of the variations in output. The capital stock input, being orthogonal to output at these frequencies, accounts for virtually none of the business cycle fluctuation”

(Prescott, 1988, p. 10)

Lastly, when compared to neo-Walrasian general equilibrium models, models with aggregate production functions also facilitate the kind of empirical analysis that real business cycles theorists envisioned, i.e., calibration followed by simulation.

Second, as explained above, early real business cycles theorists sought to make the study of the short-run fluctuations – the primary object of real business cycles theories – compatible with the propositions of the theory and empirical features of economic growth. From a statistical viewpoint, any time-series can be decomposed into trend and cycle components. Contrary to what this statistical decomposition might suggest when applied to an output series, there is no reason to suppose that a parallel segregation between theories of growth – which account for the long-run dynamics, and thus for the trend – and theories of cycle – which account for the cyclical, or high frequency, components of the output series –, must hold. Hicks (1965, p. 4), for instance, argues that such separation is arbitrary in terms of the actual economic forces driving the trend and the cycle, and therefore, it does not follow that the study of the trend shall be separated from the study of the cycle. The corollary is that theories of growth and theories of the cycle must be consistent or compatible. As such, real business cycles theory, “[unifies] business cycle and growth theory by insisting that business cycle models must be consistent with the empirical regularities of long-run growth” (Rebelo, 2005, p. 2).

Real business cycles theorists thus sought to study fluctuations through a framework coherent with the regularities and the propositions of the theory of long-run growth. The neoclassical optimal growth model was, by the time these theories were being forged, the dominant framework for understanding growth. The apparent empirical success of this framework, together with its wide applicability (see Prescott, 1988), certainly contributed to its dominance. Briefly, the neoclassical optimal growth model

consists in the Solow's (1956) model augmented by the explicit consideration of the neoclassical choice-theoretic framework and the inclusion of a stochastic component to model the technological parameter proposed later by Solow (1957). The neoclassical optimal growth model is thus an offspring of the Solow model, which is in turn, as exposed in the previous section, built on the basic, traditional neoclassical theory of capital. Real business cycles theorists 'chose' to use the traditional version of the neoclassical theory of capital, over possible alternatives, indirectly, through their option to bring the neoclassical optimal growth model to the study of fluctuations.

These two methodological reasons suggest why one theory of capital was preferred. On one hand, the traditional neoclassical theory of capital constitutes a simpler framework, and it lends itself easily to empirical analysis. On the other hand, it can be argued that the choice of one theory over the other was, at most, indirectly given that real business cycles theorists were interested in studying fluctuations in a framework compatible with growth facts, and those growth facts, at the theory level, were associated to a theory which, in turn, was based on one specific theory of capital. As it happens, however, in light of the events reviewed in the next chapter – the Cambridge controversies in the theory of capital - arguing that the model makes things 'easy' or that it allows for the 'compatibilization of growth and cycles theory' can never be a sufficient justification for choosing the traditional neoclassical theory of capital. Though perhaps necessary, the answers just surveyed *are not sufficient* to explain the choice of the particular version of the neoclassical theory of capital that we find in real business cycles theory.

One can only wonder what is the good of a quantity of capital or a period of production which, since it depends on the rate of interest, cannot be used for its traditional purpose, which is to determine the rate of interest.

Piero Sraffa (1962, p. 479)

4 Capital Theory and the Cambridge Controversies

One of the most important theoretical debates in economics in the 20th century was the debate over the neoclassical theory of capital of the 1950s, 1960s, and 1970s. This debate was triggered by an article by Joan Robinson, an economist at the University of Cambridge, in which she reflected upon the traditional notion of capital in neoclassical theory and the complications that such notion created in terms of the measurement of capital (1953).

Eventually, the defence of the neoclassical theory of capital came from a group of economists – both professors and graduate students – associated with the Massachusetts Institute of Technology (MIT) in Boston, United States. Curiously, the specific place in which the MIT is located is called Cambridge. For that reason, in one of the earliest accounts of the debate, Harcourt referred to this debate as the ‘Cambridge controversies in the theory of capital’ (1969, 1972).

Among the most relevant economists participating, from the Cambridge side⁶⁶, we find Sraffa, Luigi Pasinetti, Harcourt, Pierangelo Garegnani, and of course, Robinson. From the MIT side, we find Solow, Samuelson, Christopher Bliss, and Frank Hahn. This debate not only mobilized some of the most prominent economists of its time but gave rise to publications in the most important journals of the field (such as the *Quarterly Journal of Economics*, for example).

This chapter reviews the main contributions to the debate. The purpose is merely to document the main points articulated by both sides, aiming at understanding why the neoclassical side eventually recognized the flaws in the traditional neoclassical theory of capital. For the puzzle this thesis addresses precisely relates to the fact that a theory that

⁶⁶ As observed before, this thesis reserves the labels ‘Cambridge side’ or ‘Cambridge economists’ to the economists at the University of Cambridge, or otherwise belonging to that side of the controversy.

neoclassical economists recognized to be flawed was anyway adopted, by neoclassical economists, to study business cycles – even though an alternative neoclassical capital theory was available. The resolution of this puzzle is the object of the next chapter.

Section 4.1 documents the initial phase of the debate. To better understand the problems with the traditional neoclassical theory of capital that were identified by Cambridge economists, this section also introduces a simple model of the choice of technique. Section 4.2 deals with the neoclassical reactions to the issues identified by Cambridge economists. After trying to save the traditional neoclassical theory of capital, neoclassical economists could not avoid recognizing that their theory was fundamentally flawed. I thus establish the following propositions:

P4.1: The traditional version of the neoclassical theory of capital is pervaded by inconsistencies.

P4.2: The neoclassical efforts to rehabilitate the traditional version of the neoclassical theory of capital turned out to be unsuccessful, leading to the neoclassical side conceding defeat.

Section 4.3 briefly documents the direction followed by neoclassical economists participating in the debate after the fact that the traditional neoclassical theory of capital was flawed was established. That reaction consisted in a theoretical re-direction through the adoption of a new conception of capital coupled with an entirely new theory of capital. As this new theory does not rely on the traditional notion of capital as a homogeneous factor of production, neoclassical economists regarded their ‘new’ theory as immune to the Cambridge critique. This is the final proposition this chapter documents:

P4.3: The initial defeat led neoclassical economists to develop a new theory of capital and this spurred a feeling of victory for the neoclassical side.

Section 4.4 brings this chapter to an end with a summary of the main points.

4.1 Paradoxes in the Traditional Neoclassical Theory of Capital

As we have seen in the previous chapter, the neoclassical theory of capital, in its traditional version, includes two distinct formulations. In the original contributions of Clark, for instance, the dual nature of capital is acknowledged, even though the main

tenets implied by the theory dispense with the physical heterogeneity of capital goods. This happens because the theory aims to determine an equilibrium in which the rate of profit (or interest) over the supply price of capital goods is uniform. As such, every economic adjustment accruing from the physical heterogeneity of capital can be left implicit. As a result, the main propositions of the theory can be stated in terms of pure, aggregated capital.

In the latter contributions, where Solow (1956) is central, the physical heterogeneity of capital is completely disregarded, and capital is conceived as a physically homogeneous set of goods. In a sense, this was the obvious evolution for the neoclassical theory. In the previous versions of the theory, though the physical heterogeneity of capital was acknowledged, it effectively lacked any analytical role. Indeed, the main propositions of the traditional neoclassical theory of capital can be stated, and as Solow (1956) showed, derived, in a setting that completely overlooks the physical heterogeneity of capital goods. Perhaps for that reason, by the 1950s, neoclassical economists believed that the Solow model was analytically equivalent to a more complex model featuring physically heterogeneous capital goods.

One of the reasons why, I believe, Solow speaks of a parable is precisely that analytical equivalence. Solow's parable is a story about a simple world – from the economist's point of view – where there is only one type of capital often rationalized as a 'jelly' that changes form as needed. Of course, no one denies that this constitutes an oversimplification. Yet a simple parable reproduces the same results as the more complex, realistic case of many distinct types of capital goods. In other words, the analytical equivalence between the many capital goods model and the neoclassical parable rationalizes the reliance on the latter theoretical construct.

The analytical equivalence thesis reached its peak with Samuelson (1962), which constitutes a pivotal contribution to the controversies in the theory of capital. In that article, Samuelson waged to build a mathematical model with heterogeneous capital in order to prove that the propositions of the theory of capital as derived by Solow can easily be derived in a more complicated mathematical model, just as Clark had done years before, relying on verbal reasoning only.

The model Samuelson presented can be seen as the logical foundation of the one-commodity models of the Solow type, as it provides a way to, so to speak, derive an aggregated production function with the necessary properties to generate neoclassical results. Not incidentally, it is particularly useful to study the paradoxes in neoclassical capital theory. I now describe that model in its own terms, turning to the debate afterwards.

4.2 A Formal Model for the Neoclassical Theory of Capital

*The problem of the choice of technique*⁶⁷

A technique of production consists in a productive and efficient relation between inputs and outputs. For each level of positive output, it states what inputs are needed and what are the minimal required quantities of those inputs. These techniques are what came to be labelled as ‘blueprints’.

Production supposes the existence of productive units. At the level of capital theory, those remain black-boxes: they are the locus of production, i.e. the locus of the transformation of inputs into outputs, according to a given (set of) blueprint(s). From a theoretical point of view, the neoclassical fundamentals of the theory of capital do not require further specifications regarding what productive units are, how they are organized (besides the technical dimension of the productive process), and so forth.

Let us assume that there are just two categories of products: capital goods, say machines or any other intermediate product, and a final consumption good⁶⁸. These goods are either produced in the same productive unit or in two distinct units.

The production of capital, as well as consumption goods, necessitates both labour and capital. Capital goods are produced means of production, as opposed to labour, which is a primary factor of production. An interesting question to ask in the context of capital theory is what production technique productive units choose.

Let L be the supply of labour and K the supply of capital goods. Both are given. Assuming full employment, then it is necessarily true that

⁶⁷ The model presented in this section is based on Samuelson (1962) and Birner (2002).

⁶⁸ For now, I shall assume that only one kind of capital exists.

$$a_{LM}M + a_{LC}C = L \quad (4.1)$$

$$a_{KM}M + a_{KC}C = K \quad (4.2)$$

That is, the total amount of available labour – the supply of labour – is either employed in the capital goods sector or in the consumption goods sector (equation (4.1)). The same for capital: the total amount of available physical capital is either deployed in the capital producing sector or in the production of the consumption good (equation (4.2)). As such the coefficients of the type a_{ij} correspond to the quantity of the factor i that is being used in the production of product j at a given moment of time. For instance, a_{LM} corresponds to the quantity of labour needed to produce each machine; similarly, a_{LC} is the quantity of labour needed to produce one unit of the consumption good. The interpretation of the coefficients of the capital equation follows the same logic. I shall call these coefficients production coefficients.

It is assumed that $a_{ij} > 0, \forall i, j \in \{i = (K, L); j = (M, C)\}$. That is, production is productive and both factors of production essential for production.

If we assume these coefficients to be constant as well as constant returns to scale, then equations (4.1) and (4.2), output as a function of the inputs is given by:

$$M = \min\left(\frac{1}{a_{KM}}K_M; \frac{1}{a_{LM}}L_M\right) \quad (4.3)$$

$$C = \min\left(\frac{1}{a_{KC}}K_C; \frac{1}{a_{LC}}L_C\right) \quad (4.4)$$

Together, these functions constitute, or define, a technique of production, or a ‘blueprint’.

By assumption, the capital intensity in the production of the capital good is equal to the capital intensity in the production of the final good. In other words, the production technique features production coefficients such that the relative capital intensity is equal to one:

$$\frac{a_{LM}}{a_{KM}} = \frac{a_{LC}}{a_{KC}} \quad (4.5)$$

Assuming perfect competition in all markets, output price equals (marginal and, since constant returns to scale are assumed, average) production cost. Denoting by P the market price for capital goods, and by P_C the market price for the consumption good, we have:

$$PM = a_{LM}W.M + a_{KM}rPM \quad (4.6)$$

$$P_C C = a_{LC}W.C + a_{KC}rP_M C \quad (4.7)$$

Now, for simplicity, let $P_C = 1$, so that all prices are expressed in terms of P_C and the focus is on relative, rather than absolute, prices. Then, equations (4.5) and (4.6) can be simplified:

$$P = a_{LM}W + a_{KM}rP \quad (4.8)$$

$$1 = a_{LC}W + a_{KC}rP \quad (4.9)$$

, where P refers to the price of capital goods.

How can we calculate the final net output of this economy? Assuming that there is no depreciation, using (4.7), and noting that, by definition, $a_{LC} = \frac{L_C}{C}$, and similarly, $a_{KC} = \frac{K_C}{C}$, we have:

$$P_C C = \frac{L_C}{C} W.C + \frac{K_C}{C} rP_M C \quad (4.10)$$

Solving for $\frac{C}{L_C}$, i.e. solving for the net output per head:

$$\frac{C}{L_C} = W + rP_M \frac{K_C}{L_C} \quad (4.11)$$

Thus, output per head equals the wage rate plus the return on capital per head, or capital intensity, in the final goods sector. For simplicity, consider $\frac{C}{L_C} = q$. Then,

$$q = W + rP_M \frac{K_C}{L_C} \quad (4.12)$$

Among other things, the usefulness of capital theory lies in the determination of the income of capital owners (the profit rate, often also referred to as the rate of interest)⁶⁹, r . To that end, we can study the locus relating the wage rate to the rate of profit by using equation (4.12). $W(r)$ is then given by

$$W = \frac{1 - a_{KM}r}{a_{LC} + D_r} \quad (4.13)$$

, which is known as the wage-profit frontier. D_r corresponds to the determinant of the matrix of production coefficients,

$$\begin{bmatrix} a_{LM} & a_{KM} \\ a_{LC} & a_{KC} \end{bmatrix}$$

Given that capital intensities in both sectors are equal, this determinant is always equal to zero, which simplifies the equation of the wage-profit frontier,

$$W = \frac{1 - a_{KM}r}{a_{LC}} \quad (4.14)$$

This equation shows the wage rate as a linear function of the rate of profit/rate of interest. Note that $\frac{\partial W}{\partial r} < 0$, meaning that there is a negative relation between w and r .

Different techniques correspond to different ‘blueprints’. And each blueprint has its own wage-profit frontier. Let us first study the properties of one generic technique, such as the one just presented, before considering a more general setting. Thus, graphically, the wage-profit frontier is a negatively sloped straight line⁷⁰.

⁶⁹ Classical authors defined profits as the revenue left after paying all costs of production except the interest rate and the compensation for risk. In neoclassical theory profits are the revenue left after paying all costs of production, including the interest rate. In neoclassical theory, perfect competition drives profits to zero, while the rate of return on capital *net of risk* tends to equal the interest rate. Since for simplification the model being presented includes no risk, the classical concept of ‘rate of profit’ and the neoclassical concept of ‘rate of interest’ refer to the same thing, and thus can be used interchangeably.

⁷⁰ Graph based on Birner (2002, p. 17).

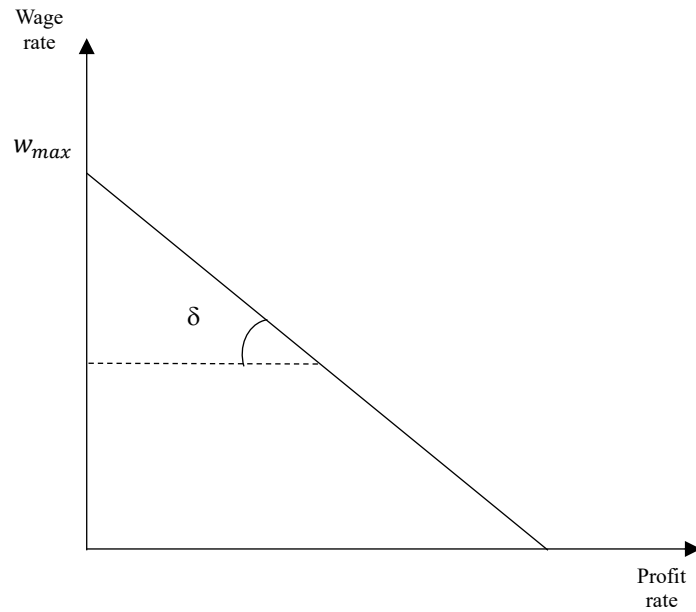


Figure 1: Wage-Profit frontier of one single technique

Each point of the wage-profit frontier corresponds to the values for wage and profit, given the quantity of factors available and given a certain level of output. The limiting case of $r = 0$, corresponds to the situation in which all product is distributed to workers. From equation (4.15),

$$r = 0 \Rightarrow W = \frac{1}{a_{LC}} = W_{max} = q \quad (4.15)$$

Therefore, recalling that $a_{LC} = \frac{L_c}{C}$, the above equation gives the per capita output of final goods associated with this technique. Whatever the level for the rate of profit, the level of output remains the same. For the sake of completeness, the output of the intermediate goods sector can be found by considering $W = 0$ in the wage-profit frontier equation (4.14):

$$W = 0 \Rightarrow r = \frac{1}{a_{KM}} \quad (4.16)$$

, which corresponds to the capital to output ratio in the production of the intermediate good. Given the current assumptions, no conclusion can be drawn about the per capita output-to-capital ratio in the consumption good sector (see Ferguson, 1969, pp. 260–261).

Capital intensity – the ratio of physical capital per head in terms of the consumption good – can also be easily retrieved using equation (4.12):

$$Pk = \frac{q - W}{r} \quad (4.17)$$

Note that this ratio equals the tangent of the angle defined by the wage-profit frontier and the horizontal axis. It then follows that capital intensity is constant, i.e., it is not a function of the prices of factors, r or w . Finally, the negative slope means that there is a negative relation between the wage rate and the rate of profit. These are the properties of a given technique of production⁷¹.

A more general case

In general, the productive unit knows, or has access to, more than one technique: the book of blueprints includes many different pages describing many techniques. If that is the case, the firm faces the problem of choosing which technique to employ. The same is even more plausible for the case of the economy as a whole.

Techniques are different with respect to their production coefficients. Neoclassical theory requires the assumption that the production coefficients for each available technique are such that relative capital intensity is equal to one. Again, this means that the intensity of capital is the same both in the production of the capital good and in the production of the final good. In a word, different techniques feature different production coefficients provided that factor proportions are the same in both sectors.

This ‘more general case’ is often interpreted as one in which capital goods are heterogeneous (see Samuelson, 1962, pp. 196–197; Birner, 2002, p. 15). In this interpretation, each technique is different for it uses a different capital good, which is captured in the difference in production coefficients between techniques. Each technique produces a specific capital good, which is then used in the production of the final good and of the capital good itself.

⁷¹ The reader may wonder why I do not consider the case of a change in the quantity of available factors, so as to establish what that would imply in terms of the wage-profit frontier and its related concepts. The reason is that, for now, we are abstracting from accumulation, which is addressed later.

Mathematically, by analogy to the simplest case, for the generic technique, $\theta = f(a_{LC}^\theta; a_{LM}^\theta; a_{KC}^\theta; a_{KM}^\theta)$, the wage-profit frontier is given by:

$$W = \frac{1 - a_{KM}^\theta r}{a_{LC}^\theta} \quad (4.18)$$

From equation (4.18) it follows that

$$\frac{\partial w}{\partial r} = -\frac{a_{KM}^\theta}{a_{LC}^\theta} \quad (4.19)$$

The actual numerical value of (4.19) will be different for every economically relevant technique. More importantly, given the assumption of equal factor intensities in the production of both goods, each technique's wage-profit frontier will be defined by different values for both a_{KM}^θ and a_{LC}^θ ⁷².

The enlargement of the book of blueprints means that the productive unit now has some possibilities of substitution between capital and labour. In other words, the technique of production is now chosen by the productive unit so as to maximize the profit rate, given the wage rate. As such, as the wage rate changes, the profit-maximizing technique may also change, which may mean the adoption of more (or less) capital intensive techniques.

For the sake of simplicity, I consider only three different (and economically relevant) techniques. The interest in this case, as opposed to a more general one with n different techniques, lies in the fortunate fact that the main propositions of the traditional neoclassical theory of capital are derivable from such a simple setting. Accordingly, this simple case allows me to explain the origin and foundations of neoclassical capital theory while preserving mathematical simplicity. Of course, afterwards, the task must be to show that the propositions hold in the general case. After all, it is the general case that real business cycles theory directly relies on.

⁷² From a mathematical viewpoint, it is possible to conceive a technique that differs from another in the value of a_{KM} , but features the same value for a_{LC} , provided the assumption of relative capital intensity equal to one is satisfied. This case is, however, uninteresting. For every feasible value for the wage rate, the technique with a lower value of a_{KM} dominates the other. That is, the latter technique always provides a higher rate of profit. As such, the other technique can be dropped from the analysis as it is never considered by profit maximizing units.

So, suppose that there are three different techniques of production, say α , β , and π . Note that each one of these techniques is the profit-maximizing one for at least one possible value of the interest rate. Figure 2 plots the wage-profit frontier for each one of these techniques.

For very high values of the wage rate, $w > W'$, α is the profit-maximizing technique, i.e., it is the technique that provides the highest value of r , holding the level of net output constant. Then, for $w = W'$, techniques α and β are indifferent – at that wage rate, the profit rate is the same for both techniques. However, the efficiency assumption implies that α is the chosen technique at this point. For values of the wage rate such that $W'' < w < W'$, technique β is profit-maximizing. For $w = W''$, β is preferred to π . Finally, for $w < W''$, π is profit-maximizing. Graphically, we have⁷³,

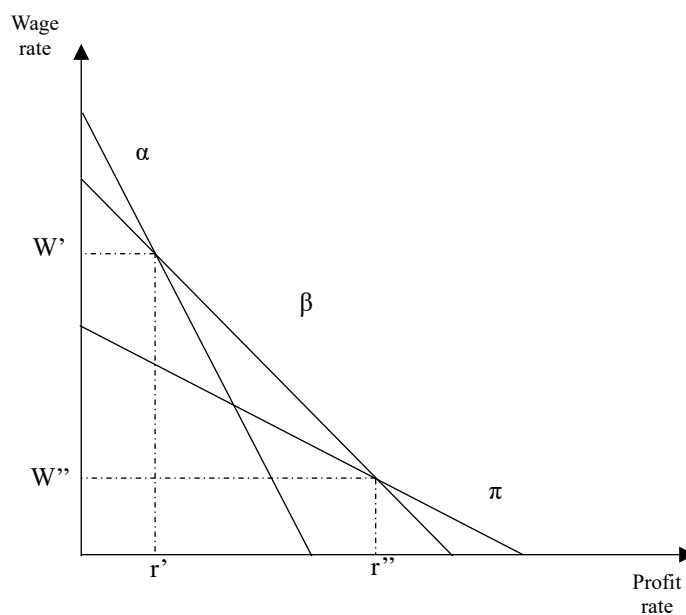


Figure 2: Wage-profit frontier with three different techniques

It follows that the economically relevant combinations of wage rate and profit rate are the ones on the envelope of the wage-profit frontiers. In other words, the economically

⁷³ Based on Birner (2002 p. 17).

relevant points are the set of boundary points of the set defined by the areas below each of the wage-profit frontier for $w \geq 0$ and $r \geq 0$.

In that envelope, the points at which two different techniques are equally profitable, correspond to corners (as they are the product of the interception of two different wage-profit frontiers). Those points are the ‘switch points’, points where the most profitable technique is about to change. The same is to say that, if the wage rate changes, even if just slightly, the productive units will be induced to change their technique of production.

Techniques of production are different with respect to the intensity of the factors they employ. In our language, each technique features particular values for the production coefficients. Considering just the profit-maximizing techniques, the assumption that the relative capital intensity is equal to 1 implies that techniques with a higher value for a_{KM}^θ feature also a relatively lower value of a_{LC}^θ . Economically, a higher a_{KM} relative to a_{LC} means that the technique is more capital intensive in the *weak sense*. By definition, a higher a_{KM} means that the ratio of capital to output in the capital goods sector is higher, while the labour to output ratio in the final goods sector is lower. In other words, the process that is more labour intensive in one sector is less capital intensive in the other (Ferguson, 1969, p. 261).

From equation (4.19), the α technique is the more intensive in capital (in the previous sense), followed by the β technique, which in turn is relatively more intensive in capital than π . The same is to say that, the more capital intensive a technique is, the higher the slope of the wage-profit frontier is.

Mathematically, the intensity of capital, measured in value, pk , for the final goods production is given by⁷⁴:

$$Pk^\theta = \frac{q^\theta - W}{r}, \quad (4.20)$$

which, as shown before, is unique for a given technique⁷⁵. The relation between the intensity of capital in value terms and the level of net output is not immediately apparent

⁷⁴ Analogously with equation (4.17).

⁷⁵ Recall that the intensity of capital in value terms of a given technique is given by the tangent of the angle

here. Both the wage rate and the profit rate are not independent of the level of output, though. The graphic representation may be of help here. Notice that the intercept of each successive wage-profit frontier in figure 2 is smaller and smaller. As shown before, this means that the quantity of output associated with each successive technique is lower. This means that, as we move to techniques less intensive in capital, the level of net output decreases. At the same time, the wage rate decreases and the rate of profit increases. It thus follows that (1) as the rate of profit increases, the intensity of capital in production decreases because the techniques chosen – the profit-maximizing ones – are less capital intensive; (2) as the rate of profit increases, the level of net output decreases; finally, combining the first two, (3) as the capital intensity decreases, output also decreases.

This simple case illustrates the four fundamental propositions of neoclassical capital theory, as formulated by its founding father, Clark (1888, 1899), and also appearing in Fisher (1906, 1907, 1930) and Knight (1933, 1934, 1936a, 1936b):

1. There is a negative relation between the rate of interest/rate of profit and capital intensity evaluated in value terms;
2. There is an inverse relation between the rate of interest and output per head;
3. As the interest rate decreases monotonically, a technique which initially had been profit maximizing is eventually surpassed by other and from that point on, as the rate of profit decreases more and more, previously profit-maximizing techniques never become the profit maximisers again;
4. Finally, for each value of capital intensity, or capital per capita, there is one and only one associated level of net output. In other words, q is a function of pk ;

The model this section presents is, from my vantage point, instrumental in two complementary ways. First, it hints at the analytical equivalence between one-commodity models and models that assume heterogeneous capital, a point that is raised during the controversies in the theory of capital. Second, as we shall see, this simple model constitutes a privileged laboratory to analyse the problems in the traditional neoclassical theory of capital, and so it helps in the understanding of the issues at stake, which were,

defined by the associated wage-profit frontier with the abscissa axis. As such, the capital intensity in value terms only changes if the slope of the wage-profit frontier changes, i.e. when the profit maximizing technique changes.

by the way, technically sophisticated at that time. The next paragraphs deal directly with the debate.

4.3 Conundrums in the Traditional Neoclassical Theory of Capital

This section gathers the main arguments articulated by the economists associated with the Cambridge side of the debate against the traditional neoclassical theory of capital. The main purpose is to show that ‘the traditional version of the neoclassical theory of capital is pervaded by inconsistencies’, which constitutes proposition P4.1.

Recall that among the data of the traditional neoclassical theory of capital we have the quantity of capital measured as an amount of value (pure capital in Clark’s language). While this quantity is taken as given, the specific capital goods that add up to that value are determined by the theory, meaning that the set of capital goods that exist in equilibrium may be different than the ones of the initial situation. There may be, therefore, an implicit adjustment of the composition of the capital goods set of the economy, such that the new bundle is compatible with the available techniques of production and with consumer preferences – i.e., compatible with equilibrium. This change in capital goods brings no change in the total value of capital whatsoever: the value of capital goods in the initial situation is the same as in the final, equilibrium situation. This is what it means to say that capital is among the data of the theory.

By determining equilibrium, this theory determines prices, including the prices of factors of production (capital and labour), thus determining income and its distribution. Both wages and interest are determined by the same kind of technical mechanism. Let me focus on capital. Capital employed in production has a net return, and the entrepreneur, who organizes production, pays a rent to whoever happens to own that capital. In relative terms and applied to pure capital, that payment is the interest rate. Entrepreneurs bid down these interest payments as much as they can, while capital owners rent their capital to whoever offers a higher payment. The conjunction of these two forces leads interest to be established at the level of the final productivity of capital: the productivity of the last unit of capital to be employed. For technical reasons, the final productivity is not constant with the amount of capital employed: it is, instead, decreasing. As a result, the quantity

of capital employed depends on the prevailing interest rate, which is taken as given by the entrepreneurs under free competition.

Now, how can we define, and measure, the quantity of capital? Robinson (1953) tells us that there are two alternatives. As capital goods earn rents over a sequence of periods of time, the value of a specific capital good is simply the present value of all the rents that that capital is going to earn over the course of its use. Alternatively, one can adopt a ‘cost of production approach’. Accordingly, the value of a given capital good is simply given by its cost of production, measured, for instance, in wage-units. It is more or less easy to discuss the relative merits of each of these two ways of obtaining the value of a given quantity of capital. But, despite any criticism one might make, it is striking that both measures depend on the interest rate. The ‘present value approach’, as it consists of the sum of flows of money accruing at different periods in the future, requires, of course, the use of some interest rate. Regarding the ‘cost of production approach’, since capital goods take time to be produced, and more importantly, are not produced by labour alone, the cost of capital goods includes the cost of capital goods that are used as inputs, and so, that were produced earlier. It follows, therefore, that summing all the costs involved in the production of a capital good is a summation of flows of money – wages – occurring at different time periods of the past. Again, this requires an interest rate.

Summing up, the traditional neoclassical theory of capital takes the quantity of capital as a datum of the theory, which is in turn supposed to determine, among other things, the interest rate, which is required to get the quantity of capital in the first place. The traditional neoclassical theory of capital thus features a logical fallacy – a circular reasoning⁷⁶. In this respect, the opening quotation of this chapter says it all,

“One can only wonder what is the good of a quantity of capital or a period of production which, since it depends on the rate of interest, cannot be used for its traditional purpose, which is to determine the rate of interest.”

(Sraffa, 1962, p. 479)

⁷⁶ There is still another alternative to measure the quantity of capital. That would simply take the value of each capital good to be given by its respective market price. As it happens, this also brings a circularity problem to the theory. Market prices, including the prices for capital goods, are one of the things the theory is supposed to determine, and therefore, cannot be taken as given.

Sraffa played a central role in the identification and articulation of the problems associated with the traditional neoclassical theory of capital. Although it was Robinson (1953) who first publicly called the attention of the discipline to the problem of finding a consistent measure of capital within the framework provided by neoclassical theory, Sraffa's *Production of Commodities by Means of Commodities* (1960) definitely put those issues on the agenda (Pasinetti & Scazzieri, 1990, p. 141). In particular, he stressed the implications that the difficulties in measuring capital might have to the technical apparatus of neoclassical theory. This way, the debate started to be centred around the technical dimension of the neoclassical theory of capital, where measurement issues take the form of, or are manifest as what has been called, 'paradoxical behaviour' (see Pasinetti & Scazzieri, 1990). Indeed, most of the contributions to the Cambridge controversies consisted in the identification, characterization, and discussion of that paradoxical behaviour.

The proposition that there exists paradoxical behaviour in the traditional neoclassical theory of capital simply means that variables such as 'capital intensity' or the 'interest rate' can behave in ways that are problematic to the neoclassical theory. Problematic as they would not only contradict the propositions of the traditional neoclassical theory of capital, but also the neoclassical propositions of the theory of value, namely, that the scarcer a good is, the higher its price tends to be.

It is time to call these paradoxes by their names. One is the 'reswitching' of techniques of production; the other is 'reverse capital deepening'. Both were identified by Robinson (1953) – though she did not attach great importance to them – and also by Champernowne (1953).

Reswitching and Reverse Capital Deepening

In the choice of technique model presented earlier, we saw that, as the rate of interest increases, the techniques of production that become successively more profitable are less intensive in capital. This result is in line with the propositions of the traditional neoclassical theory of capital; the same is to say, that it is compatible with a decreasing demand function for capital: as the interest rate increases, less capital is demanded.

Recall that we have, for the simple case of three techniques of production⁷⁷:

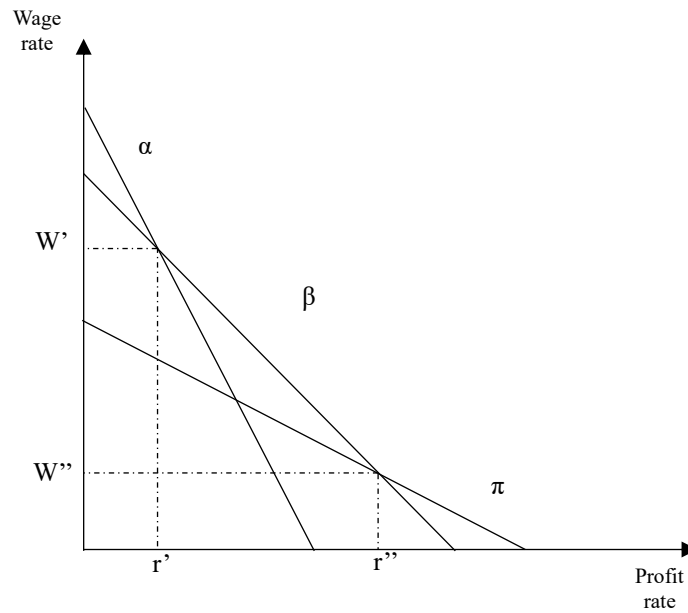


Figure 3: Wage-profit frontier with three different techniques

Now, it is easy to conceive a shape for the wage-profit frontier such that the relations just mentioned between capital intensity and interest rate, and between the interest rate and demand for capital, do not hold. Graphically, we would have:

⁷⁷ Based on Birner (2002, p. 17).

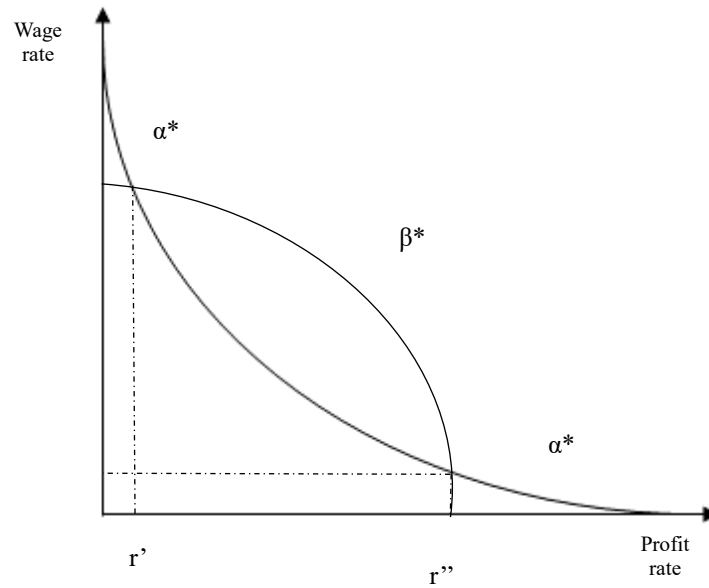


Figure 3: Non-linear wage-profit frontiers

In this case, for a profit rate below r' , technique α^* is the chosen one. For values for the interest rate between r' and r'' , technique β^* is the profit-maximizing technique and so the preferred one. And finally, for values for the interest rate greater than r'' , we have, again, technique α^* as the profit-maximizing one, and thus the one to be deployed. Thus, the same technique, α^* , is the profit-maximizing technique at two different ranges of values of the interest rate, while in between another technique, β^* , is the chosen one. As the interest rate decreases, the economy switches from α^* to β^* , and then back from β^* to α^* . This is the *reswitching of techniques*. Likewise, when the economy switches from technique α^* to β^* , the capital intensity in the economy decreases as the interest rate decreases. This relation is referred to as *capital reverse deepening* and is inconsistent with (well behaved) employment and demand functions for capital. For, according to the traditional neoclassical theory of capital, with a lower interest rate, the marginal productivity of capital, in equilibrium, must be lower as well, which in turn requires an increase in capital intensity (and so demand for capital would increase). This straightforward graphical analysis just presents a case where that might not happen.

But, what is the economic meaning and significance of wage-profit frontiers of the type depicted in the graph? First and foremost, there is no monotone relation between capital intensity and the interest rate. The same is to say, there is no ‘well-behaved’ employment function for capital, and thus no negatively sloped demand function for capital. That is,

“[i]t has always been taken for granted . . . , simply by analogy from other fields of traditional economic theory, that when a change in the physical capital goods themselves takes place, as an effect of a change in the rate of profit (i.e., at switching points), the ‘quantity of capital’ per man required by the new method of production will change in an inverse monotonic relationship to the rate of profit.”

(Pasinetti, 1966, p. 513)

“The theoretical implications [...] are rather far-reaching, particularly with reference to one of the most vexed questions in capital theory: the question of whether – at any given state of technical knowledge – there is any relationship between changes in the rate of profit and changes in the ‘quantity of capital’ per unit of labor .

(Pasinetti, 1966, pp. 512–513)

Let us now study how reswitching and reverse capital deepening obtain in the baseline model presented before. To produce this result, it is sufficient that one wage-profit frontier is non-linear (see Harcourt, 1972). The linearity in the original model was guaranteed by assuming the determinant of the matrix of production coefficients to be zero. Economically, this means that for any given technique, the two sectors have the same capital intensity. It therefore follows that paradoxical behaviour occurs when the assumption of equal capital intensities in both sectors, for any technique, is dropped.

Summing up, reverse capital deepening raises the possibility of a positive relation between the rate of interest and capital intensity while the reswitching of techniques suggests that a given technique may be the profit-maximizing technique at two different values for the interest rate, while in between other techniques are profit-maximizing. In turn, this means that the map of capital intensity to output is not a function – there is no production function.

Ultimately, reswitching of techniques and reverse capital deepening question the idea of the interest rate as the price of capital, as well as the negative relation between the interest rate and capital intensity. The use of an inconsistent definition of capital is the source of problems. This proves proposition P4.1, which asserts that ‘the traditional version of the neoclassical theory of capital is pervaded by inconsistencies’.

4.4 Neoclassical Replies and the Cambridge Victory

This section surveys the principal defences of neoclassical theory offered by economists at the MIT. As we shall see, the initial reaction was one of circumventing, or even negating altogether, the points raised by the Cambridge side. Eventually, however, as more interventions from the Cambridge side further reinforced the early arguments, it became clear that there were indeed flaws in the traditional neoclassical theory of capital. Neoclassical economists could not help but to admit defeat. As proposition P.4.2 states, the neoclassical efforts to rehabilitate the traditional version of the neoclassical theory of capital turned out to be unsuccessful, leading to the neoclassical side conceding defeat.’

Although the debate heated up in the 1960s, after the publication of *Production of Commodities by Means of Commodities* (1960), Robinson’s (1953) article generated direct responses. Recall, Robinson (1953) develops a case pointing out the problems that an aggregated measure of the stock of capital creates for neoclassical capital theory. Intentionally or not, she set the challenge: would it be possible to construct a measure of capital that avoids the logical circularity she had identified?

Champernowne (1953) accepted the challenge. His aim was to provide a measure of capital that, by avoiding the problems noted by Robinson, would enable the consistent use of production functions to derive distribution as a function of factor scarcities and their respective marginal productivities (p. 112). In particular,

“[t]his measure removes the more glaring difficulties in the way of regarding aggregate output as a function of the amounts of labour and capital employed”

(Champernowne, 1953, p. 115)

This measure of capital would, so to speak, separate the changes in its total value that result merely from changes in distribution from the changes that accrue from changes in

the quantity of physical capital (Champernowne, 1953, p. 115; Harcourt, 1972, pp. 30–32). Briefly, Champernowne proposes a ‘chain index’ measure of the quantity of capital: the ratio of the quantities of capital of two techniques which are both competitive at the same r (switch points) is given by the ratio of the respective points calculated at that interest (see Champernowne, 1953, p. 116).

This measure, however, does not solve, in general, the problem. Champernowne himself recognizes this:

“Contrary to intuitive expectation, our assumptions do not ensure that a graph of [the production function] is a single-valued curve sloping upwards to the right”

(Champernowne, 1953, p. 118)

A single-valued production function can only be guaranteed if logically dubious assumptions are made (Champernowne, 1953, p. 118). In a word, Champernowne’s analysis strengthens Robinson’s case.

Another contribution, still before Sraffa’s influential book was published, was Solow (1956). This article was pivotal in framing one of the argumentative strategies that the neoclassical side would eventually adopt in the course of the debate. Not incidentally, it constituted the definitive neoclassical treatment of the traditional theory of capital. All the ingredients of the traditional neoclassical versions of the theory are there: the main assumptions and results. Solow merely provides a mathematical treatment of an existent, yet not completely formalized, theory⁷⁸.

There is, however, an important difference between Solow’s model and its theoretical predecessors, a difference connected to the issue raised by Robinson three years earlier: the unit of measurement of capital⁷⁹. Solow easily overcomes the problem of measurement by assuming capital to be a homogeneous commodity. So, it can be measured using its own physical units. No circularity problem arises here, and the traditional parables in the theory of capital are easily derivable.

⁷⁸ Solow’s (1956) does introduce something new and that is the notion of a steady-state equilibrium which does not correspond to the long-period equilibrium of the previous versions of the theory. In any case, however, the conception of capital as a single factor remains and the traditional neoclassical propositions in the theory of capital follow from the model, and it is in this sense that this paragraph shall be read.

⁷⁹ Swan (1956) could also be mentioned in this respect (Harcourt, 1972, pp. 34–39).

Sraffa, Levhari and the symposium

In 1960, Sraffa finally publishes *Production of Commodities by Means of Commodities* (1960), a book that definitely puts the problems of reswitching and reverse capital deepening in the agenda. And, of course, the neoclassical side had to react. This reaction encompassed two different, complementary strategies.

First, Levhari (1965) proposed that, at the macro level, the presence of paradoxical behaviour would not happen. He showed this for a specific type of economy. In that economy, every commodity is used in the production of every other commodity, either directly or indirectly. Mathematically, the input-output matrices for every technique of this economy are assumed to be indecomposable.

More important is Samuelson (1962). Solow (1956) had formally shown that the traditional neoclassical theory of capital is derivable in a one commodity model, eliminating any role for the notion of physically distinct capital goods. However, Clark's theory, and most capital theory that followed it, was built on the idea that, while capital is homogeneous with respect to its value, it is heterogeneous in terms of its physical dimension.

Given the problems raised by Robinson (1953) and Sraffa (1960), the traditional neoclassical theory of capital can only be saved if one can show that the results derived by Solow (1956), and by Clark and others before him, are equally derivable in a heterogeneous capital goods setting. For, if so, then the one-commodity model, a model that 'solves' the measurement issues, is analytically equivalent to the more complicated heterogeneous capital model:

“Repeatedly in writings and lectures I have insisted that capital theory can be rigorously developed without using any Clark-like concept of aggregate ‘capital’, instead relying upon a complete analysis of a great variety of heterogeneous physical capital goods and processes through time.”

(Samuelson, 1962, p. 193)

In his 1962 article, Samuelson develops the concept of a ‘surrogate production function’:

“I shall use the new tools of the Surrogate Production Function and Surrogate Capital to show how we can sometimes predict exactly how certain quite

complicated heterogeneous capital models will behave by treating them as if they had come from a simple generating production function (even when we know they did not really come from such a function)”

(Samuelson, 1962, p. 194)

The model Samuelson uses to derive this surrogate function is the simple model of the choice of technique already detailed in this chapter⁸⁰.

So, by 1965, the issues raised by Robinson and Sraffa were dismissed by neoclassical economists, especially by Solow, Samuelson, and Levhari. The surrogate capital model appeared to have shown that Solow’s strategy to measure capital creates a model that is analytically equivalent to the heterogeneous case. Moreover,

“This theorem [Levhari’s], if true, would have falsified Sraffa and would have given full generality to Samuelson’s ‘surrogate production function’”

(Pasinetti, Fisher, Felipe, McCombie, & Greenfield, 2003, p. 227)

This was the state of the debate by 1965 when the first world congress of the Econometric Society took place in Rome. In that conference, Pasinetti presented a paper disproving Levhari’s theorem, by means of a concrete example. The article that was eventually published in the following year (Pasinetti, 1966), in a symposium in the *Quarterly Journal of Economics*, was an improved version of the one presented in Rome (Pasinetti, 1966, p. 501n). The other articles in the symposium all elaborate on the flaws of Levhari results.

Yet the most relevant article, as far as the fate of the debate is concerned, is Samuelson (1966), which he illustratively titles ‘A summing-up’. In this article, Samuelson basically surveys the debate, the main arguments put forward, and recognizes, following the results Pasinetti (1966) had achieved, that paradoxical behaviour in the traditional neoclassical theory of capital is indeed possible,

⁸⁰ Some years later, Garegnani (1970) would show that Samuelson’s model assumes homogeneous, rather than heterogeneous, capital. Thus, his analytical equivalence is simply the demonstration that equal assumptions deliver equal results and not a proof that using a one commodity model solves the problem of paradoxical behaviour without dropping any crucial element that can only be present in more complex models.

“The phenomenon of switching back at a very low interest rate to a set of techniques that had seemed viable only at a very high interest rate involves more than esoteric technicalities. It shows that the simple tale told by Jevons, Böhm-Bawerk, Wicksell, and other neoclassical writers - alleging that, as the interest rate falls in consequence of abstention from present consumption in favor of future, technology must become in some sense more ‘roundabout,’ more ‘mechanized,’ and ‘more productive’ - cannot be universally valid.”

(Samuelson, 1966, p. 568)

Samuelson even shows why paradoxical behaviour might occur by means of a simple numerical example inspired by the Austrian framework (see 1966 p. 569-574). Later, he acknowledges a consequence of paradoxical behaviour that challenges conventional economic reasoning:

“going to a lower interest rate may have to involve a disaccumulation of capital, and a surplus (rather than sacrifice) of current consumption, which is balanced by a subsequent perpetual reduction (rather than increase) of consumption as a result of the drop in interest rate.”

(Samuelson, 1966, p. 581)

In the face of all this, Samuelson cannot help but recognize that the traditional version of the neoclassical theory is flawed. And,

“If all this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life.”

(Samuelson, 1966, p. 583)

This article, together with Garegnani (1970), constituted the final nail in the traditional neoclassical theory of capital – at least, as far as this debate is concerned. Hahn, who would become a central figure on the neoclassical side of the debate in later phases, put it as clearly as possible:

"[production functions] cannot be shown to follow from proper [general equilibrium] theory and in general [are] therefore open to severe logical objections."

(quoted in A. Cohen & Harcourt, 2003, p. 206)

In a word, neoclassical economists lost the battle and they did concede that. Their efforts in rehabilitating the theory were eventually shown to be unsuccessful. This inevitably led Samuelson, arguably the figurehead of the MIT side of the debate, to admit the flaws in the traditional neoclassical theory of capital. The Cambridge side, by 1966, was winning the debate. In a word, this section demonstrated proposition P4.2: the neoclassical efforts to rehabilitate the traditional version of the neoclassical theory of capital turned out to be unsuccessful, leading to the neoclassical side conceding defeat’.

4.5 The Neoclassical Rehabilitation

The symposium, and Samuelson’s article, in particular, marked a shift in the debate (see, for instance, A. Cohen & Harcourt, 2003). Both the focus of the contributions and the participants changed. In terms of the scope of the contributions, neoclassical economists, followed by the economists of the Cambridge side, started to bring concepts and insights from the recently developed general equilibrium theory, including a new conception of economic equilibrium. Regarding the participants, Solow, Samuelson and their MIT colleagues and graduate students, were replaced by, ironically, scholars who were somehow connected to the Faculty of Economics and Politics of the University of Cambridge: Bliss – a former economics student at Cambridge, who took a visiting teaching position at the MIT in 1962-1963, and spent most of his career at the University of Essex and at the University of Oxford; and Hahn – who took his doctoral degree at the London School of Economics (under the supervision of Kaldor and Robbins), became a fellow of the Churchill College, Cambridge, and was later appointed professor at the Faculty of Economics and Politics⁸¹.

Garegnani's (1970) article played a pivotal role in this new phase of the debate. In this article, Garegnani studies the implications of re-switching and reverse capital deepening to the stability of equilibrium. Starting from a long period equilibrium position,

⁸¹ I cannot resist to share a curious story. During a research visit to the University of Cambridge, I met a professor who had been a student at the Faculty of Economics and Politics – as it was named then – at the time Hahn was appointed to his professorship. He went to Hahn’s inaugural lecture, “On the notion of equilibrium in economics”, in 1973. That professor told me that, sitting right behind him, was an important Faculty member who, every five minutes or so, would just spit the words, ‘this is bulls...!!!’. As we shall see, the idea of equilibrium was crucial in the latter phase of the debate.

he asks what follows an exogenous increase in savings. First and foremost, this represents an increase in the supply of capital. Taking this new amount of capital supplied as given, the new long-run equilibrium position will feature a lower interest rate. Thus, an increase in the supply of capital is followed by a decrease in the interest rate. This is the story told by the traditional neoclassical theory of capital. If reverse capital deepening is a possibility, however, then a decrease in the interest rate may imply a decrease – rather than an increase – in the demand for capital: initial excess of capital supply gets even bigger.

The neoclassical response to Garegnani came from Bliss (1970). By relying on the existence proofs for the general equilibrium model of Arrow and Debreu, he notes that the necessary conditions for equilibrium are satisfied in Garegnani model. As a result, Garegnani's concerns about the implications of reverse capital deepening to equilibrium are not relevant.

More important than the relative power of the arguments in confrontation here is the fact that Bliss decisively changes the terms of the debate. For, while Garegnani is arguing in terms of long-period equilibrium, as was the case in all contributions to the debate until then, Bliss bases his argument on a setting featuring a new, different conception of equilibrium: short-period equilibrium.

This new conception of equilibrium was proposed by Hicks (1939), but it was with the general equilibrium theory of Arrow and Debreu (1954) that it was fully developed. Perhaps even more relevant than this change regarding equilibrium is the associated change in the concept of capital.

As explored earlier, this new notion results from dropping the assumption of a uniform rate of profits over the supply prices of capital goods. In this class of models, capital is no longer a factor of production. Rather, capital now names a vector of physically distinct commodities, as Walras had proposed. For this reason, from this point on, the debate focused on Neo-Walrasian models.

The economists from the Cambridge side kept believing that there were problems with the 'new' theory of capital, but their arguments were not as persuasive as before. Viewing capital as a vector of heterogeneous commodities appears to free the neoclassical theory from the concept of capital as a single factor (i.e., homogeneous or an aggregated

amount of value). This, together with the change in the notion of equilibrium, may have contributed to the declining interest in the debate and to the inefficacy of the Cambridge attacks on this new theory of capital (A. Cohen & Harcourt, 2003; Fratini, 2019; Garegnani, 2012; Lazzarini, 2015; Petri, 1978). As a result, neoclassical economists did think they won this second phase of the debate. Which establishes proposition P4.3: ‘The initial defeat led neoclassical economists to develop a new theory of capital and this spurred a feeling of victory for the neoclassical side.’

4.6 A Summing up

The so-called Cambridge controversies in the theory of capital were ignited by some economists from Cambridge who diagnosed some flaws in the traditional neoclassical theory of capital. At the root of those flaws is a problem related to the way capital should be defined in neoclassical theory. Briefly, Robinson and Sraffa showed that any definition of capital implies a measurement strategy that requires an interest rate to be computed. The interest rate, however, is among the variables the theory is supposed to determine, and thus cannot be among the data of the theory.

At the technical level, as we have seen, this problem is manifest in what came to be called paradoxical behaviour, i.e., cases in which the traditional neoclassical propositions in the theory of capital were contradicted, even within a neoclassical model. These cases were labelled reswitching and reverse capital deepening and this eventually led neoclassical economists to recognize the limitations in their theory.

So far as the debate is concerned, when facing the problems with the traditional neoclassical theory of capital, the neoclassical side of the debate took refuge in a different conception of capital – a vector of heterogeneous commodities – which was coupled to an entirely different theory of capital and to a different conception of equilibrium. As, at least apparently, this new conception of capital avoided the measurement issues, neoclassical economists thought this new theory to be immune to the traditional Cambridge critique. This arguably spurred a feeling of victory within neoclassical spheres, and the debate eventually withered. And, somehow, the defeated theory of capital came to prominence in macroeconomic modelling in the early 1980s. Chapter 5 addresses that.

“Wittgenstein: The question is: Why are people afraid of contradiction? It is easy to understand why they should be afraid of contradictions in orders, descriptions, etc. outside mathematics. [...] Why should they be afraid of contradictions inside mathematics?”

Turing: Because something may go wrong with the application [of the mathematics] ... And if something does go wrong – if the bridge breaks down – then your mistake was of the kind of using a wrong natural law. [...] You cannot be confident about applying your calculus until you know that there is no hidden contradiction in it.

Wittgenstein: But nothing has ever gone wrong that way yet...”

Quoted in ‘Alan Turing: The Enigma’ by Andrew Hodges (1983, p. 196-197)

5 A Puzzle in the History of Economic Ideas

The reader may be struggling to reconcile the last two chapters. The first presents the theory of capital that underlies the real business cycles paradigm, whilst the second describes a debate that, just a few years before the dawn of that paradigm, proved that theory of capital to be wrong. As we have seen, Samuelson, the figurehead of the MIT side of the debate, raised the neoclassical white flag, admitting the validity and relevance of the Cambridge criticisms. From that moment on, the neoclassical side of the debate elaborated on a new framework for capital, at first sight immune to the Cambridge critique. Paradoxically, though, the real business cycles paradigm builds precisely on the discredited version of the neoclassical theory of capital.

This chapter is devoted to addressing this enigma, summed up in the following proposition:

P5.1 The capital theory debate had little, if any, impact in the mainstream circles after the 70s, including in real business cycles theory, even though,

during that debate, the neoclassical side recognized the flaws of its capital theory.

The observation contained in proposition P5.1 immediately leads to the central question this chapter needs to deal with: how can it be that a theory *recognized to be flawed* remained in use, and was even pushed to the core of economic theorising, including, specifically, business cycles theorising?

Thus, at first sight at least, rendering P5.1 intelligible involves dealing with two sorts of issues. *First*, why did neoclassical economists opt for the traditional neoclassical theory of capital as part of the framework to study cycles? This question was already addressed in chapter 3. There, recall, the explicit reasons, if not actually justifications, for choosing the traditional neoclassical theory of capital were related to the fact that these economists wanted to use the neoclassical optimal growth model. This was because (1) real business cycles proponents aimed to unify the study of growth and the study of cycles; and (2) the neoclassical optimal growth model facilitated the empirical approach these economists had in mind. However, these reasons are blind to the flaws of the traditional neoclassical theory of capital pointed out during the Cambridge capital controversies (chapter 4). In the light of these controversies, therefore, the reasons explored in chapter 3 turn out to be insufficient, that is, the choice of the traditional neoclassical theory of capital does not logically follow from those reasons.

And this brings us to the *second* issue, to be addressed in this chapter. How can it be that a flawed theory was contemplated as a possible choice by real business cycles theorists? Whilst many commentators remark that the current mainstream simply ignores the capital controversies, there remains significant disagreement as to why this is the case. My suspicion is that the true nature of the debate – i.e., the ultimate source of disagreement between Cambridge and neoclassical economists – needs to be further analysed in order to understand the current indifference towards the debate. To this effect, an analysis of various accounts of the debate proves useful. The argument advanced below is that an ontological point of view provides a satisfactory answer to our second issue. My central theses are, therefore, the following:

P5.2 The Cambridge capital controversies can be reinterpreted as a clash ignited by the belief that neoclassical theory was unable to address reality as it really is.

P5.3 Since the essence of the Cambridge opposition was ontological but the debate never explicitly addressed ontology, the Cambridge criticisms were not deadly, which accounts for the survival of the traditional neoclassical theory of capital.

The rest of this chapter is organized as follows. Section 5.1 recaps the puzzle expressed in proposition P5.1. Section 5.2 delves into the literature about the capital controversies. The goal here is to understand if there exists an explanation for the apparent indifference of the discipline to the capital theory debate. As I shall establish in due course, the most promising account of the debate for the purpose of solving the puzzle suggests that each side holds a different vision about the nature of economic explanation. Section 5.3 goes on to scrutinize the latter hypothesis in the light of recent developments in the philosophy of science and of economics. The main point in this section is that underpinning those methodological disagreements about the nature of economic explanation are different worldviews – different ontologies. The latter part of this section is dedicated to gauging what those worldviews are. Section 5.4 concludes.

5.1 The Puzzle Restated

As chapter 3 demonstrated, the theoretical paradigm of real business cycles builds on “[t]he neoclassical model of capital accumulation, augmented by shocks to productivity” (Stadler, 1994, p. 1753). This ‘neoclassical model of capital accumulation’ corresponds to the Solow (1956) growth model, extended by the explicit inclusion of optimal choice theory, which furnishes the micro-foundations for consumption/saving behaviour. This micro-founded growth model is often referred to as the optimal growth model.

From a mathematical point of view, the homogeneity assumption present in the Solow (and thus, in the optimal growth model) allows capital to be measured in its own physical units. This deflects the necessity of relying on the prices of different capital goods to get an aggregate measure of the quantity of capital. More importantly, it makes

that measure independent of the interest rate – the source of problems identified during the Cambridge controversies⁸².

But make no mistake: “the famous Solow parable (Solow, 1956) in which all equilibrium paths seek the steady-state depends on just those possibilities of aggregation which reswitching examples show not to be available” (Hahn, 1982, p. 373)⁸³. In other words, the neoclassical theory of capital as framed by parable-like models produces results regarding the behaviour and relations between the endogenous variables – capital intensity, output, and interest rate – for which sustainable logical foundations are nowhere to be found. Thus, “[B]y means of the neoclassical theory of the choice of technique, [Cambridge economists] have established that capital aggregation is theoretically unsound” (Hahn, 1975, p. 363). The formal equivalence that would render the use of a parable-like version of the theory legitimate is therefore unwarranted. This is the problem with the capital theory underlying the real business cycles framework, formulated in proposition P5.1:

The capital theory debate had little, if any, impact in the mainstream circles after the 70s, including in real business cycles theory, even though, during that debate, the neoclassical side recognized the flaws of its capital theory.

In the context of the controversy, the awareness of the problems with the traditional version of the neoclassical theory led to a *theoretical redirection* of the neoclassical side of the debate. As Hahn observes, the Cambridge criticisms apply to a certain theory of capital, but within the neoclassical thought there is an alternative theory:

“The result has no bearing on the mainstream of neoclassical theory simply because it does not use aggregates. It has a bearing on the vulgar theories of textbooks. But textbooks are not the frontier of knowledge.”

(Hahn, 1975, p. 363)

⁸² In his unusual account of the Cambridge controversies, Amartya Sen (1974) scripts a dialogue between two characters, who talk about the debate. One of the characters eventually observes that “Venerable Solow may make peculiar assumptions, but he never makes a mistake. He not only assumed a homogeneous capital good but simply one good in the economy, which eliminates the problem of the relative price of capital and consumer goods, which must be unit.” (p. 329).

⁸³ Note that this is stated by an economist on the neoclassical side.

For Hahn, and certainly for other neoclassical economists, the frontier of knowledge by the late 1960s and early 1970s is general equilibrium theory, to which Hahn himself contributed⁸⁴. In this theory, capital is not a factor of production. Instead, it consists of a myriad of physically distinct inputs to production. This is the old Walras conception of capital, revived in what came to be known as the neo-Walrasian programme. Interestingly, in this alternative theory, “there is no support [...] for the proposition that an input to production will be cheaper in an economy where more of it is available” (Bliss, 1975b, p. 85). Not only measurement issues are avoided, but the problematic results associated with Clark’s parables do not (generally) hold. This fact leads Hahn to say that,

“unless one wishes to claim that aggregation is essential if a theory is to be called neoclassical, so that Arrow-Debreu for instance are not neoclassical, none of this [Cambridge critiques] has any bearing on the main issue [...]. Sraffa performed a service in showing how neoclassical arguments can be used to show neoclassical aggregation parables to be in logical difficulties. But that cannot help with a critique of marginal theory.”

(Hahn, 1982, p. 373)

At this juncture, one cannot help but wonder why a discredited theory of capital survived⁸⁵. For, from the viewpoint of neoclassical economists, the problem with the capital theory underlying the real business cycles framework could have been overcome by using another neoclassical theory of capital. How can it be that, despite there being an alternative, a concept and indeed a whole theory *recognized to be flawed* remained in use, and was even pushed to the core of economic theorising, including, specifically, business cycles theorising?

5.2 Towards an Explanation: Accounts of the Debate

Why have generally acknowledged insights of the Cambridge capital controversies been forgotten just some years after the debate? Having engaged the most influential

⁸⁴ This theory was initially formulated by Léon Walras and subsequently developed by Sir John Hicks (1939), Edmond Malinvaud (1953), Kenneth Arrow and Gérard Debreu (1954), and Lionel McKenzie (1954).

⁸⁵ Note that this argument extends beyond the field of business cycles. For example, the very same theory of capital is used in the endogenous growth theory that emerged in the late 1980s.

economists of the '60s and early '70s, the debate faded away and left no significant traces in the discipline or at least in its hugely dominant mainstream. It is now time to have a look at what the relevant literature has to say about this.

Many commentators have provided wide-ranging analyses of the capital controversies. From some of them, I suggest, it is possible to retrieve an argument, sometimes implicit, as to why the debate seems to have been forgotten. The following paragraphs are concerned with this literature, which offers essentially three major, not incompatible hypotheses as to why the debate failed to exert any enduring impact in the discipline: (1) ideology; (2) simple misunderstandings; (3) unclarified methodological disagreements. I shall analyse each of these hypotheses in turn.

Ideology

The association of ideology to the Cambridge controversies is present from their inception. Ideology is implicitly mentioned in the first comprehensive account of the episode, written by Harcourt, a Cambridge scholar:

“It is my strong impression that if one were to be told whether an economist was fundamentally sympathetic or hostile to basic capitalist institutions, especially private property and the rights to income streams, or whether he were a hawk or a dove in his views on the Vietnam War, one could predict with a considerable degree of accuracy... which side he would be on in the present controversies.”

Harcourt (1972, p. 13)

In a review of Harcourt's book, Joseph Stiglitz - who is methodologically orthodox but politically on the left – conversely asserts that ideology “may have some limited influence on the questions we ask, but not on how we go about answering them” (Stiglitz, 1974, p. 901). Problems of logical consistency, he continues, are not ideological issues (1974, p. 901).

More recently, in a joint work with Avi Cohen, Harcourt returned to the issue of ideology in the Cambridge controversies providing what amounts to a re-statement of his initial account:

“With neither side able to deliver a knockout punch, issues of faith and ideology entered the ring with claims about the significance of the results and competing

visions of economics. When one-commodity results are not robust in more general models, the lack of definitive evidence leaves room for ideology to play a role in the decision to hang on to a theory or vision. The intensity and passion of the Cambridge controversies were generated not by abstract technical questions about Wicksell effects, but by strong ideological undercurrents like the ethical justification of the return to capital”

(Cohen & Harcourt, 2003, p. 210)

Many other economists from the Cambridge side share this view that ideology was relevant for the controversies. On the neoclassical side, in turn, while some agree with Stiglitz, others believe that ideology is necessary to explain the controversies. Solow, in his Nobel lecture, recollects the controversies as a (scientific) trap, for

“that whole episode now seems to me to have been a waste of time, a playing-out of ideological games in the language of analytical economics. At the time I thought – and the literature gave some reason to think – that part of the argument was about marginalism, about smooth marginalism. So I wanted to be able to show that the conclusions of the theory and of its empirical implementation were not bound to that very special formulation. I guess it was worth doing, but it certainly did not pacify anyone.”

(Solow, 1988, p. 309)

On a different occasion, Solow reflects on the reasons for the permeability of discussions on capital theory to ideology,

“One can legitimately wonder why capital theory lends itself so easily to violent, unproductive, and confused controversy. I think there are two reasons for this, one intrinsic to the subject and one incidental, or at least intellectually incidental. Let me mention the incidental reason first, because we will not have to return to it. It is an ideological reason. One of the perennial questions of 19th century capital theory was: ‘Why is the interest rate positive?’ Obviously, this is only a hair's breadth from asking: ‘Why do capitalists earn an income, and is it just that they should do so?’ In one sense 19th century capital theory had the social function of providing an ideological justification for profit.”

(Solow, 1963, p. 10)

More recently, another mainstream economist, Paul Romer, refers to the controversies in a particularly accusatory tone:

“Economists usually stick to science. Robert Solow (1956) was engaged in science when he developed his mathematical theory of growth. But they can get drawn into academic politics. Joan Robinson (1956) was engaged in academic politics when she waged her campaign against capital and the aggregate production function. Academic politics, like any other type of politics, is better served by words that are evocative and ambiguous, but if an argument is transparently political, economists interested in science will simply ignore it”

(Romer, 2015, p. 89)

What to make of all this? The first thing to note is that ‘ideology’ – a protean concept⁸⁶ – is being used in a different sense by Cambridge economists and by neoclassical economists. For Cambridge economists, ideology is present on both sides of the controversy: each side is committed to a set of ideological propositions, which are part of the scientific enterprise. In contrast, Solow and Romer argue that *the other side* is guilty of ideology. In other words, they are employing the category to imply that the approach they oppose is outside the realm of science.

Relatedly, both sides also disagree as to the actual role of ideology in the debate. Cambridge economists seem to hold that economists’ ideologies are not perceived by them as preconceptions or misconceptions, whereas Solow and Romer imply that Cambridge economists are using economics to push for their ideological agenda.

Given the above, can ideology be at the root of the current indifference of the discipline towards the capital controversies? My suggestion, based on the accounts briefly surveyed here, is that that hypothesis is not sustainable. Cambridge and neoclassical economists employ different conceptions of ideology and disagree on its role in the controversy, and therefore, on the fate of the debate. Second, not all neoclassical

⁸⁶ Let me give two examples. For Marx, ideology is a false representation of reality that serves the interests of whoever happens to be powerful. For Schumpeter, in turn, ideology is constituted by the preconceptions of human beings which enable them to interpret and make sense of the world around them. As such it is inevitably present in the scientific process, but perhaps kept in check by the analytical methods of science (see Schumpeter, 1949, 1954).

economists agree with the diagnosis that the controversy is ideological in nature. Stiglitz is quoted above stating precisely that, but he is not alone. Samuelson, for instance, noted that

“[Sraffa’s] reputation tends to get tied up with ideological jockeyings within our profession. Perhaps this is inevitable but I regret it – for, ideology aside, mainstream economists of the mathematical or literary persuasion can benefit much from Sraffa's contributions and also from the problems that his works pose for further investigations.”

(Samuelson, 2000, p. 25)

For Samuelson, although ideology could be somehow related to the controversies, Cambridge economists, Sraffa in this case, made valid scientific points to which neoclassical economists must pay attention. Indeed, the most influential critiques of the neoclassical theory – which came to be named reswitching and capital reverse deepening – constitute *immanent* critiques of the neoclassical theory of capital and so they can hardly be framed as ideological. Unless, of course, one regards neoclassical economics as an ideological enterprise, which is obviously not the position of neoclassical economists. The question then becomes: why is a scientific, indeed mathematical, point, which constitutes an immanent critique of the traditional neoclassical theory of capital, conflated with an exercise of ideology? Contrary to Samuelson, almost all the neoclassical economists that insist that the controversies were ideological (i.e., non-scientific) do not engage in the debate. One could, therefore, be tempted to see in such an interpretation of the debate a rhetorical device to discredit non-orthodox economists.

In any case, the foregoing suggests that the idea that ideology was involved in the controversies does not move us any further in understanding the particular problem we face, i.e., the fact the real business cycle paradigm is built on a flawed theory of capital while an alternative was available.

Simple misunderstandings

The very idea, held by some economists, that the debate was an ideological quarrel may be considered a misunderstanding. This subsection is not concerned with misunderstandings about how the debate should be interpreted, however. Rather, it

focuses on the possibility that, for whatever reason, neoclassical economists misunderstood the arguments made by the Cambridge side and as a result concluded that paying attention to the debate was not worthwhile.

The literature, especially on the Cambridge side of the controversy, often claims that during the transition from the first to the second phase of the debate there was a change in the concept of equilibrium in neoclassical theory. This change and its implications, so the argument goes, was misunderstood or not grasped by neoclassical economists (Lazzarini, 2015, p. 257). The redefinition of equilibrium in question refers to the theoretical reorientation of the neoclassical theory of capital after the 1966 symposium in the *Quarterly Journal of Economics*. This theoretical reorientation brought general equilibrium theory to the fore, with its new conceptualization of ‘capital’.

The roots of this distinct theory of capital go back to Walras, but Hicks was pivotal in positioning it at the centre of mainstream economic theorising. Looking back at Hicks’ contributions and its implications for economic theory, Garegnani observes that

“If the disappearance of the normal position as such in ‘Value and Capital’ had those effects of obscuring in the later stages of the controversy the essential terms of the neoclassical problem of capital, the misinterpretation of the normal position as a stationary state, which has been the cause of its effective disappearance, has had important direct effects on subsequent pure theory even beyond its indirect effects of obscuring the capital controversy.”

(Garegnani, 2012, p. 1429)

What are the implications of this alleged misunderstanding for the fate of the debate? If Garegnani is correct, neoclassical economists were simply unable to grasp the flaws in their ‘new’ theory and, therefore, the points raised by the Cambridge side. As a result, the debate became irrelevant to them, which is why it does not influence current economics.

The problem with this answer, for our present purposes, is that it locates the misunderstandings in the second phase of the debate. At this point of the controversies, and indeed as a consequence of them, the flaws in the traditional version of the neoclassical theory of capital were accepted by both sides. This is why neoclassical economists refashioned their theory of capital. Our puzzle is that the insights from the

first phase of the debate are completely forgotten. Moreover, the claim that the second phase of the debate is pervaded by misunderstandings, implying that neoclassical economists did not grasp the Cambridge critique of the new version of the theory of capital, makes it even more striking that a sustainable theory (in the eyes of neoclassical economists) did not prevail over a flawed one.

Unclarified methodological disagreements

The Cambridge controversies are often taken to be the seminal moment for post-Keynesian economics (see Hamouda & Harcourt, 1988). A similar idea appears in Mata (2004, p. 242), according to whom the ‘historical narratives’ on those controversies, more than the debate itself, generated a sense of group identity for post-Keynesians⁸⁷. It is in this light that Sheila Dow’s ‘*Methodological Morality in the Cambridge Controversies*’ (1980) and Cohen’s ‘*The Methodological Resolution of the Cambridge Controversies*’ (1984) are best interpreted.

In her article, Dow uses the Cambridge controversies as a case study on how orthodox economics deals or reacts to criticisms. Noting that this debate had little impact on the mainstream, she suggests that “[a]n explanation for such an outcome can be drawn from the field of philosophy of science, and from Kuhnian theory, in particular” (p. 372). She sees the economics of Sraffa and his colleagues as an attack to orthodoxy grounded on an alternative paradigm:

“It seems clear that the proponents of the Cambridge critique viewed themselves as engaged in an exercise of extraordinary science. The object of criticism was the very foundation of neoclassical marginal analysis; accommodation to the criticism would have required a complete overhaul of the economic orthodoxy. The logical inconsistency in neoclassical theory which allowed the possibility of reswitching and capital reversal should have been just the type of ‘discovery’ to provoke a crisis, and thus start the scientific revolution”

(Dow, 1980, p. 374)

⁸⁷ Among the economists belonging to, so to speak, the first generation of post-Keynesians, we count not only economists based in Cambridge, particularly those participating in the controversies, but also American-based economists influenced by Robinson and Sraffa (see Mata, 2004, pp. 250–251). Perhaps the most influential figure of the American post-Keynesians group is Paul Davidson.

So, Kuhn's philosophy of science seems useful to analyse the episode, particularly the fact that the mainstream successfully resisted the attack – the alternative paradigm did not become dominant. But what is that alternative paradigm? The alternative paradigm is best characterized, Dow argues, by a rejection of formalism:

“The dichotomy between formalistic and nonformalistic systems is clearly drawn in economics. On the one hand, there is the formal orthodoxy in the shape of neoclassical economics; in general the habitat of those who espouse the view that economics is a positive science. On the other hand, there is the Keynesian/institutional/historical approach, which rejects formalism in order to incorporate, and make explicit, subjective analysis.”

(Dow, 1980, p. 370)

Dow further observes that to facilitate communication with the mainstream, Cambridge economists focused on mathematical (or formalist) critiques to neoclassical theory, even though disagreements were, in the main, not of a formal nature (Dow, 1980, p. 375). The non-formalist critique is not taken seriously (Dow, 1980, p. 370); any critique not expressed in technical, formalist terms is ignored (Dow, 1980, pp. 376–377). Correspondingly, neoclassical responses focused on formalist aspects. As the formalist critiques were eventually addressed through general equilibrium theory – or so neoclassical economists argue – neoclassical theory survived, perhaps stronger than before. What would effectively ‘kill’ neoclassical theory would be the complete rejection of the methodology of the mainstream. Accordingly, Cambridge economists should have targeted the superiority of formalism, the methodological essence of the mainstream. Dow concludes:

“The lesson to be drawn from this experience is that it is crucial for the success of scientific revolutions that alternative paradigms be expressed in terms of models and tools quite distinct from those of the ruling paradigm. This would allow the alternative paradigms to be judged as separate systems, not as modifications of existing paradigms. In particular, promoters of alternative paradigms should explicitly exempt themselves from moral pressure to adopt the ruling methodology.”

(Dow, 1980, p. 378)

Cohen (1984) takes up the challenge of rendering the methodological differences between the two sides of the controversy explicit: in other words, of articulating the Cambridge post-Keynesians' methodological stance. Following Dow (1980), Cohen locates the proximate source of disagreement between Cambridge post-Keynesians and neoclassical economists on different views on the role of formalism in economics. In neoclassical economics, the preferred model of explanation is the hypothetic-deductive model. Very briefly, according to the hypothetic-deductive model, a proposition is 'explained' if and only if it can be derived from other propositions, which must include a universal law (i.e., empirical regularity) and a set of initial and boundary conditions. This sort of explanation is typical of the mathematical modelling of neoclassical economics. On the contrary, economists of the Cambridge side, Cohen claims, viewed explanation as concerned with causal mechanisms rather than (finding or assuming) empirical regularities.

Cohen (1984) argues that in the capital controversies differences of opinion regarding what constitutes a good explanation repeatedly surface, and these differences explain the lack of resolution of the debate:

“The point of logic that makes the neoclassical theory unacceptable to post Keynesians is not the mutually agreed upon possibilities of reswitching or capital reversal, but the lack of an adequate causal mechanism for explaining the process of change. Without the specification of such a causal mechanism, no amount of empirical evidence will convince a post Keynesian [...] a post Keynesian finds the neoclassical comparison of equilibrium positions and the causal mechanism of the aggregate production function to be inadequate methodological tools for the task of explaining the historical process of accumulation.”

(p. 623, 626)

Taken together, Dow (1980) and Cohen's (1984) argument is that the Cambridge controversies in the theory of capital involved a clash between two different methodological outlooks. The Cambridge side implicitly held a different conception regarding explanation in economics, but merely provided an immanent critique of the neoclassical theory of capital. As the technical issues pointed by the Cambridge side were, at least in the eyes of neoclassical economists, successfully fixed, the lack of resolution

(and thus, of impact) of the debate can be attributed to the failure of making the methodological critique clearer⁸⁸.

My thesis is that this account of the controversies can be seen in a new light if contemplated from the perspective of recent developments in the philosophy of science and economics.

5.3 Ontology and the Cambridge Controversies

In Chapter 2, the discussion centred around the possibility, role, and manner in which ontology can be used in economics. As I recalled, methods of scientific analysis, including models of explanation, have conditions of applicability – among those, conditions referring to the nature of the object of study, i.e., ontological conditions. Thus, the acceptance, use, or advocacy of any method of analysis carries with it, even if unknowingly and only implicitly, a commitment to the world being in a certain way.

These considerations allow us to go one step beyond Cohen (1984). If the latter is correct in locating the source of disagreement between the two sides in the Cambridge controversies in different accounts on what constitutes a proper approach to economic explanation, then the root of the disagreement is ultimately located in differences in terms of the ontological commitments of the economists on each side. The mainstream's commitment to deductivism is a commitment to a certain set of ontological propositions, which, in turn, are (implicitly) disputed by Cambridge economists. Accordingly, the debate can be seen as a clash ignited by an ontological unease, which we may formulate as follows:

P 5.2 The Cambridge capital controversies can be reinterpreted as a clash ignited by the belief that neoclassical theory was unable to address reality as it really is.

The intuition that matters of ontology were in some way involved in the Cambridge capital controversies is not entirely new. One can perhaps detect it in Cohen and Harcourt's (2003, pp. 207–209) claim that distinct visions fuelled the controversy. The neoclassical 'vision', as identified by Cohen and Harcourt (2003), turns on "the

⁸⁸ Because, it is argued, making that clear would effectively challenge neoclassical theory (Dow, 1980, p. 379).

lifetime utility-maximizing consumption decisions of individuals as the driving force of economic activity, with the allocation of given, scarce resources as the fundamental economic problem” (p. 208). In turn, “the ‘English’ Cantabrigians argue for a return to a classical political economy vision. There, profit-making decisions of capitalist firms are the driving force, with the fundamental economic problem being the allocation of surplus output to ensure reproduction and growth” (p. 208). In my interpretation, however, these propositions do not refer to the nature of the subject matter of economics. Rather, they correspond to distinct theories of how the capitalist system works; they can, of course, be read as metatheories, but only in so far as they are interpreted as theories about what other economic theories should respect or theorise about. The vision Harcourt writes about correspond to judgements as to how the economy, or the capitalist system, as an object of study, must be conceived of, *given* its ontological nature.

A clearer example of the role of ontology in the Cambridge controversies is Nuno Martins (2014, pp. 60–61), where a concise account of the ontological disagreements in the debate is provided. This work benefits from Martins’ contribution, taking it as a guide for the elaboration of the argument of this chapter, as well as for locating the relevant evidence. The aim of this chapter is to identify, articulate and systematize the ontological positions of each side of the debate as well as to clarify the ways in which these ontological positions surface. I seek to identify precisely what arguments or argumentative strategies commit each side to their ontological positions. Therefore, the idea that ontology is present in the controversies is here articulated in a more detailed way and supplemented by compelling pieces of evidence. Furthermore, this analysis is in due course used to render our puzzle intelligible.

In a nutshell, my interpretative strategy is as follows. First, I retrieve, and briefly elaborate upon, the ontological commitments of deductivism. If Cohen (1984) is correct, these commitments should surface on the neoclassical side of the debate. Investigating whether that is the case is my second step. Then, I go on to inquire whether we can identify in the Cambridge position and arguments traces of a challenge to those commitments. Once this is done, an account of the Cambridge controversies emerges. The final objective is to confirm that this account clears up our puzzle.

5.3.1 Ontological commitments of deductivism

Deductivism, recall, is the model of explanation according to which a proposition is explained if and only if it can be derived from a set of other propositions, where at least one of them must be a general law, and the others a set of initial and boundary conditions. Therefore, deductivism is the “type of explanation in which regularities of the form ‘whenever event x then event y ’ (or stochastic near equivalents) are a necessary condition” (Lawson, 2003, p. 5). Its feasibility relies upon the discovery or use of regularities at the level of events. Thus, deductivism “assumes or posits or constructs regularities (deterministic or stochastic) connecting actualities such as events or states of affairs” (Lawson, 2003, p. 13). Systems in which those type of regularities occur are often called *closed systems*. The application of deductivism is therefore appropriate if the object of study is a closed system.

Stating that deductivism is committed to closed systems is still too general. It would, for example, be interesting to know what the necessary or sufficient ontological conditions of possibility for a closed system to obtain are. Lawson writes:

“The most obvious scenario in which a prevalence of such closures would be expected is a world 1) populated by sets of atomistic individuals or entities (an atom here being an entity that exercises its own separate, independent, and invariable effect, whatever the context); where 2) the atoms of interest exist in relative isolation (so allowing the effects of the atoms of interest to be deducible/predictable by barring the effects of potentially interfering factors)”

(Lawson, 2015b, p. 143)

There is a lot to unpack from this passage. Let us begin with the idea that a closed system is *atomistic*. According to this view, such system “is made up entirely of externally related entities so that all things exist and act in ways that are quite independent of any relationships in which they stand” (Lawson, 1997, p. 166). Those relations are external in that they are not constitutive of the nature of the atoms. Nevertheless, external influences on the system can affect its behaviour in many distinct ways. Thus, the second feature of a closed system Lawson identifies above is that the system needs to be in relative isolation. Therefore, a closed system is typically one in which the external influences that could eventually affect the behaviour of the system are ruled out. This condition we call *isolationism* (Lawson, 1997, p. 84). In sum, the necessary and sufficient

conditions for (deterministic or stochastic) event regularities of the type ‘whenever x then y ’ to be guaranteed, and therefore for a system to be closed, are that (1) the system is composed by atoms and (2) it is in relative isolation from external influences. A closed systems ontology, therefore, is also an ontology of atomism and isolationism.

At this juncture, then, it is necessary to gather evidence that during the debate neoclassical economists were pushing an agenda of, or were influenced by, a closed systems ontology. Second, it is necessary to establish that the Cambridge economists were in fundamental disagreement with that worldview, which explains their opposition to neoclassical theory and hence the debate. The next few paragraphs address these issues.

5.3.2 Ontology, deductivism and the Cambridge controversies: the neoclassical side

The previous paragraphs established that deductivism is committed to an ontology of closed systems – systems where event regularities are guaranteed – which in turn means that the object of study is atomistic and relatively isolated. Can we find evidence that neoclassical economists participating in the debate were pushing an agenda of, or were influenced by, a closed systems ontology? This section picks some pivotal moments of the debate in order to reassess the main neoclassical arguments and argumentative strategies. The aim now is to uncover, articulate, and analyse the true nature of the neoclassical position in the debate and so it is now the time to consider the neoclassical arguments in a perspective different than that of chapter 4.

Closed systems and the neoclassical theory of capital

In the above, I determined the conditions of possibility for closed systems to obtain. Now I want to argue that a commitment to a closed systems ontology is easily retrieved from the most influential arguments and strategies of neoclassical economists during the debate.

Yet, before delving again into the debate a few comments on the nature of the neoclassical theory of capital ought to be made. First, note that the traditional versions of the neoclassical theory of capital deduce that, among other things, whenever the interest rate goes up, capital intensity goes down, *all other things constant*. Clearly then,

neoclassical theory deduces regularities of the type ‘in conditions z , whenever x , then y ’. Conditions z are all elements of the theory that, if changed, could disturb the relation between x and y (technology or preferences, for instance). The qualification ‘*all other things constant*’, otherwise known as the ‘*ceteris paribus*’ condition, corresponds to the assumption, or constraint, that conditions z do not change when x and y are changing. An implicit assumption here is that whatever has not been specified by the theory – what is not part of z – does not change and thus does not disturb the regularity at issue (or, if it does, it does not affect the relation at issue). The theoretical system is thus shielded from possible external influences that could change the structure of the regularity in question.

Similarly, the internal closure condition emerges from the fact that, everything else constant, whenever the interest rate decreases, for example, firms mechanically or automatically opt for more capital-intensive techniques of production. Furthermore, since this regularity is supposed to hold for the economy as a whole and is usually derived from a representative agent setting, the theory is committed to atomism. Clearly, then, the traditional neoclassical theory of capital supposes an ontology of closed systems.

It was with this background that Robinson’s (1953) complaints, in Harcourt’s expression (1972, p. 15), triggered the Cambridge controversies in the theory of capital. Inevitably perhaps, the direct responses of those who, at least in part, took Robinson’s case seriously, was manifold.

Some tried to come up with a way of measuring the capital stock such that no logical flaws of the type pointed out by Robinson (and, later on, by Sraffa, whose ideas had inspired Robinson’s original point) would ensue. Champernowne (1953) exemplifies this effort. He tried to work out a way of measuring the stock of aggregated capital which would separate changes in its total value that result merely from changes in distribution from changes that accrue from changes in the quantity of physical capital (Champernowne, 1953, pp. 115–116; see also Harcourt, 1972, pp. 30–32). This ‘measurement solution’ actually failed, as Champernowne himself recognized (1953, p. 118).

Others tried to understand what assumptions would have to be made at the theory level in order to avoid paradoxical behaviour – a symptom of the logical complications of the way capital is measured. Thus, Solow (1955) asked:

“When if ever can the various capital inputs be summed up in a single index-figure, so that the production function can be ‘collapsed’ to give output as a function of inputs of labor and ‘capital-in-general’?”

(Solow, 1955, p. 102)

His conclusion was that “the marginal rate of substitution of one kind of capital good for another must be independent of the amount of labour in use” (Solow, 1955, p. 102). As it happens, a straightforward way to assure just that is to assume that the stock of physical capital is constituted by physically homogeneous capital goods. For, in that case, capital goods, because they are identical, are perfect substitutes, regardless of the level of employment of labour. This response to Robinson paved the way for Solow’s subsequent and influential contributions, particularly Solow (1956), which constitutes the ultimate version of the traditional neoclassical theory of capital and made Solow the direct successor of Clark as far as the theory of capital is concerned.

The elephant in the room is, of course, the rigidity of the assumptions required to create a theory from which paradoxical behaviour was totally removed. To establish the case that there can be a neoclassical model verifying Clark’s parables (i.e., immune to paradoxical behaviour), one needs to establish the analytical equivalence between the simple, parable-like model, and a more general and realist setting of many, physically distinct, capital goods. Samuelson (1962) – and, later, Levhari (1965) – worked on that analytical equivalence. In Samuelson’s words,

“the ‘Surrogate Production Function’ can provide some rationalization for the validity of the simple J. B. Clark parables which pretend there is a single thing called ‘capital’ that can be put into a single production function and along with labor will produce total output (of a homogeneous good or of some desired market-basket of goods)”

(Samuelson, 1962, p. 194)

Levhari (1965), a graduate student of Samuelson’s at the MIT, supplemented his teacher’s piece by establishing that, though paradoxical behaviour could exist at the disaggregated, firm-level, it would never arise at the macroeconomic, aggregated level, provided a number of conditions were satisfied. This came to be known as the non-reswitching theorem.

Solow (1956), Samuelson (1962) and Levhari (1965) constitute a three-part case against the Cambridge attack to the neoclassical theory of capital⁸⁹. If valid, this three-part case would show that the paradoxes are, after all, not relevant at the theory level: a simple one-commodity/one-sector model of the macroeconomy would produce the same results of a model with heterogeneous capital if certain mathematical conditions hold. This constitutes the ‘parable reaction’ of the neoclassical economists to the Cambridge attack.

Other economists sought to understand exactly what conditions would preclude paradoxical behaviour in neoclassical capital theory. Charles Ferguson and Robert Allen (1970)⁹⁰ provide perhaps one of the most telling examples:

“We show that it is virtually impossible to invalidate the neoclassical relation between relative factor price and relative factor usage (a) when reswitching occurs at rates of interest higher than a certain critical rate, and (b) when relative commodity price falls as entrepreneurs readopt some previously used technique.”

(1970, p. 97)

But neoclassical economists followed yet another route beyond coming up with new ways to measure capital and finding the assumptions that would preclude paradoxical behaviour. They raised doubts regarding the empirical likelihood of paradoxical behaviour. Samuelson (1966, p. 582) was perhaps the first economist to hint at the ‘empirical dimension’ of the problem. But Bruno, Burmeister and Sheshinski (1966) state that the problem is ultimately an empirical one:

“There is an open empirical question as to whether or not reswitching is likely to be observed in an actual economy for reasonable changes in the interest rate.”

(1966, p. 545n)

⁸⁹ Another example is Swan (1956). In his paper, Swan simply dodged the capital measurement problem by assuming that physically distinct capital goods are “both infinitely durable and instantaneously adaptable” (p. 345). Though, strictly speaking, not homogeneous, capital goods of the Swan model are ultimately theoretically equivalent to Solow’s homogeneous capital goods.

⁹⁰ This model assumes heterogeneous capital, but nevertheless has an aggregate measure for its stock and so still constitutes a version of the traditional neoclassical theory of capital.

Ferguson's (1969) book on capital theory provides yet another example. He writes that “capital intensity uniqueness is an econometric question susceptible of resolution in a probabilistic sense” (1969, pp. 269–270). More generally,

“The crucial point to emphasize is that the validity of neoclassical theory is an empirical, not a theoretical, question. At the time of this writing, there have been some, but limited, advances toward the construction of statistical models by means of which the empirical validity of neoclassical theory may be assessed”

(1969, p. 258).

In other words, it was established that Clark’s parables involve paradoxes. This implies that the predictions of traditional neoclassical theories of capital are *prima facie* unwarranted. Neoclassical economists subsequently start to claim that the issue is empirical rather than theoretical. If econometricians showed that regularities of the traditional neoclassical theory of capital hold, then the theory would face no major problem⁹¹. After all, if the simple one commodity/one sector model is capable of reproducing the relevant regularities, why bother with anything else?

Later, when Cambridge economists started to derive the implications of reverse capital deepening and reswitching to the stability of equilibrium, neoclassical economists engaged in a theoretical change of direction. They recruited the neo-Walrasian general equilibrium theory which involves no aggregation. As such, the problems identified earlier by the Cambridge economists seem to be irrelevant to this version of neoclassical theory.

What emerges from all these strategies? What is common to them? What to make of all this? When neoclassical economists understood that the predictions of their theory were not assured, they tried to reinforce theoretical closure in order to make the theory capable of generating their preferred results. All arguments and strategies exposed above boil down to attempts to guarantee that, given a set of prevailing conditions x , only one outcome, y , is possible. In other words, neoclassical economists were not prepared to drop

⁹¹ There is a general agreement that traditional, aggregate production functions perform well in empirical testing, something that, following the neoclassical positions the debate, would legitimize the traditional neoclassical theory of capital. As it happens, however, that apparent success has been linked to the fact that aggregate production functions display an implicit accounting identity (see Felipe & Fisher, 2003; Felipe & McCombie, 2014).

the traditional regularities from their theories, which were formulated so as to ensure the results or predictions they wanted to preserve.

This emphasis on prediction is, moreover, confirmed by the strategy of dismissing the Cambridge points by stating that the problem, rather than theoretical, was empirical. If the problem is ultimately empirical, then either the regularities are empirically confirmed and therefore the paradoxes are nothing but theoretical curiosities; or the regularities are not confirmed, which poses serious problems to the theory. In any case, in so formulating the problem, neoclassical economists are already committed to the idea that regularities, in general, exist. This idea, coupled with the theoretical emphasis on deriving covering law statements, is sufficient to commit the neoclassical economists to an ontology of closed systems.

Finally, a word on the theoretical redirection towards general equilibrium theory and Walrasian capital. This theory, though it does not produce the results upon which capital reverse deepening and reswitching hinged, pursues the agenda of closed system theorising. Starting from a set of axioms, it derives a set of economic propositions in which mechanistic and atomistic conceptions are apparent. Agents are treated atomistically and internal closure is assured by a utility function (see Montes, 2003b, pp. 740–741). Similarly, the external closure condition is assured by simply assuming that only what is contained in the theory can affect its predictions. The necessary conditions for closure are present in general equilibrium theory and those conditions commit the theory and its proponents to an ontology of closed systems. The theoretical change of direction brought about by the adherence to general equilibrium does not amount to an ontological change of direction. A commitment to a closed systems ontology remains.

5.3.3 Ontology and the Cambridge side

Can we identify in the arguments formulated by the Cambridge economists an opposition to this? After reviewing the neoclassical positions in the debate and uncovering their underlying commonality, this section takes another look at the Cambridge position. In particular, the focus here is on whether Cambridge economists were motivated by an ontological opposition to the mainstream and whether they directly challenged that worldview.

Contrary to the neoclassical side, agreement among Cambridge economists is harder to find at the surface. Not only do different economists place emphasis on different critiques of the neoclassical theory, but substantial divergences persisted, leading even to the emergence of a ('small-scale') controversy among Cambridge scholars regarding the proper place of equilibrium in economic theorising (see Cohen and Harcourt (2003, p. 204). Even so, all Cambridge economists rejected the neoclassical theory of capital (in all its versions) and saw the anomalies arising from reswitching and capital reverse deepening as demolishing the traditional neoclassical theory of capital.

In order to address the difficulties arising from the relative disagreement among Cambridge scholars, the study of the Cambridge positions in the debate is done in two steps.

The first step focuses on Sraffa's thought. Sraffa is arguably the intellectual inspiration of the Cambridge economists participating in the debate. Robinson attended his lectures in the late 1920s; he would later become the mentor of economics research students (and some of them would , later, contribute to the debate); in 1936, the issues associated with the measure of the capital stock in neoclassical theory were pointed out by Sraffa to Robinson in a private letter; his book, *Production of Commodities by Means of Commodities* (1960) was central to the controversies, and Sraffa saw it as a prelude to a critique of economic theory. Sraffa was not the most prolific of economists, however. His book was in development for more than 30 years (Fratini, 2019, p. 11). During that period, he did not publish any work; his previous work is from the 1920s. Yet, despite having published so little, Sraffa left an immense archive, which includes preparatory notes for lectures, notes about the articles and books he was reading, and some of his own thoughts. This archive has recently been made public and is now available online⁹².

The second step focuses on Robinson. For the most part, the other Cambridge scholars not reviewed here shared the main tenets of Sraffa and Robinson (as depicted

⁹² Up until very recently, to consult Sraffa's unpublished manuscripts one would have to visit the Janus Library at the Trinity College, Cambridge, supplied with the necessary authorizations. Regardless of the logistic and bureaucratic complications, some work has already been done on his personal archives. Among other things, this work has thrown some light on the philosophical, and indeed, as we shall see, ontological thinking of Sraffa.

below). Divergences between the Cambridge group were restricted to some of the implications of the topics discussed here, e.g. the interpretation of Sraffa's (1960) system.

First step: Piero Sraffa

Sraffa's manuscripts enable the archaeological reconstruction of his thought. They include his reflections over many years. In some cases, it is possible to track an idea or argument since its initial stages and analyse its process of progressive refinement. Thus, one can trace the evolution of his thought, not only on economic matters but also, and importantly, on some philosophical issues of relevance to economics. Most of these reflections remained unpublished, which makes one wonder to what extent his philosophical positions, for instance, played a role in his approach to, and the substance of, his economics.

The following reconstruction of part of Sraffa's thought is based mainly (but not exclusively) on his unpublished manuscripts. It focuses on what seems relevant to interpret his reservations towards neoclassical economics. Thus, I first explore the evidence in his manuscripts regarding what he believes to be the role, if any, of metaphysics in science, and in economics in particular. Second, I concentrate on the reflections that most directly triggered his critiques of neoclassical economics. Not every passage in Sraffa's manuscripts that could possibly be in some way related to these points is reviewed here, of course. I am confident, however, that the essence of his thought (as reflected in his manuscripts) is adequately conveyed by the selected passages. The growing literature on the interpretation of his thought as depicted in the manuscripts has helped me in identifying the key vectors of his thought as well as in interpreting his writings and validating my claims.

Despite the intellectual dominance of logical positivism in most of the 20th century, Sraffa's manuscripts include several instances of the word metaphysics, sometimes in a pejorative and sometimes in a sympathetic tone. Sraffa defines it as follows,

“By metaphysics here I mean, I suppose, the emotions that are associated with our terminology and frames (*schemi mentali*) – that is, what is absolutely necessary to make the theory living, capable of assimilation and at all intelligible.”

From Sraffa's definition of the subject, it follows that metaphysics plays (or should play) a role in science, including economics. The metaphysics of a theory serves to determine its utility and, on top of that, to understand the theory itself (Martins, 2013, p. 457). More generally one should always be aware that each method or theory carries with it its own metaphysics, which defines its legitimate scope of use⁹³.

The above reveals Sraffa's metaphysical awareness. The more interesting question now is, I believe, how this metaphysical awareness influenced his thought and work: in particular, how his metaphysical thought influenced his rejection of neoclassical theory. Or, alternatively, how his work and thought moulded his metaphysical beliefs.

The first major contribution to economics by Sraffa was a critique of the Marshallian supply and demand framework (1926). Sraffa's point was that in general one cannot take a partial equilibrium approach to study value, as industries are so *interconnected* that ultimately supply and demand curves are not independent (Martins, 2013).

Notwithstanding this, up until the late 1920s, Sraffa remained basically happy with the supply and demand framework. Subsequently, though, a change in his thought – in economic as well as in philosophical matters – started to take shape (see J. B. Davis, 2012; Marcuzzo & Rosselli, 2011; Rosselli & Trabucchi, 2019), as Sraffa (D3/12/7/161) himself recognizes. Eventually, he wholly rejected the marginalist theory of value and what he terms 'marginal method'. From his unpublished manuscripts, it is possible to conclude that methodological considerations grounded this rejection (see Rosselli & Trabucchi, 2018). Part of those considerations constitute a direct challenge to the proposition that the economic world consists of closed systems, and it is precisely on those that what follows focuses on.

First, underlying the concept of 'marginal quantities' – a conceptual building block of the marginal method – is some notion of 'change'. As Sraffa observes, "[w]ithout change either in the scale of an industry or in the 'proportions of the factors of

⁹³ As Sraffa puts it: "our metaphysics is in fact embodied in our technique; the danger lies in this, that when we have succeeded in thoroughly mastering a technique, we are very liable to be mastered by her" (Sraffa, D3/12/4/15).

productions' there can be neither marginal product nor marginal cost" (Sraffa, 1960, p. v). The notion of 'change' is better captured if contrasted with the notion of 'difference', as Sraffa himself does:

"The general confusion in all theories of value [...] must be explained by the failure to distinguish between two entirely distinct types of questions and the universal attempt of solving them both by one single theory. The two questions are: 1) what determines the [*difference* in the? (sic)] values at which various commodities are exchanged in a given market on a given instant? 2) what determines the *changes* in the values of commodities *at different times?* (e.g. of *one* commodity)."

Sraffa (D3/ 12/ 7/115, emphasis in the original)

As of now, the result of a change is something that does not exist just yet; it is not part of the current situation, as Sraffa would put it. Thus, in relying on the notion of change, neoclassical economics cannot avoid but to *base their explanations on prediction statements*. Sraffa finds in Wicksteed an example of this way of theorising:

"Wicksteed considers that the path to be followed when one of the quantities is changed is prescribed a priori like the rails prescribe the path of a tramcar"

Sraffa (D3/12/49)

The same is to say, Wicksteed seems to believe that knowledge of the initial situation suffices to know the positions associated with changes, as long as the forces triggering those changes are also known. But in so proceeding, Wicksteed, and marginal theory in general, are committed to determinism, a position Sraffa rejects:

"This is nothing less than a declaration of faith in universal determinism, for nothing less can support the belief in the actual existence of a prescribed path which must inevitably be followed, whether by the consumer or by the producer, such as is described by the demand – and supply – curves: for no observation, however minute, of the existing situation (in our case, of the existing methods of production) can bring out the path along which they must move in any given circumstances."

Sraffa (D3/12/46/52)

What leads Sraffa to reject determinism? In his manuscripts, it is possible to find at least two reasons behind that rejection. The first is that

“in mechanics if the experiment is repeated in similar circumstances (say, on the elasticity of a metal) the same results will be obtained. But with supply and demand, even if the external circumstances were the same, the result would be different because man learns from experience, or at any rate is changed by it, forms and transforms habits, etc.”

Sraffa (D3/12/42/10).

Another reason for the rejection of determinism is found in the following passage:

“Besides, for {supply and demand} curves, external {circumstances} include the behaviour of our individuals: for the action of an individual is not independent of others, and individual demand (or supply) curves cannot be aggregated into a collective curve (as is well-known for supply curves, {and} as Pigou as acknowledged at least for some cases of demand).”

Sraffa (D3/12/42/11)⁹⁴

Sraffa thus seems to reject determinism on the grounds that human agents themselves change over time, that is, their habits change, and experience can modify their internal structure and thus their choices. Moreover, human agents are not isolated atoms as external circumstances are relevant in determining their behaviour. All this leads Sraffa to suspect that the regularities expressed by supply and demand curves (or functions) hardly happen spontaneously. In more modern language, Sraffa argues that deterministic closures in the social world do not generally exist. And note that Sraffa’s rejection of determinism is not an acceptance of some form of stochastic determinism – he rejects the necessary conditions for a closed system, be it deterministic or stochastic, to arise – it is rather a commitment to an open systems ontology.

To summarize the point, Sraffa’s rejection of the legitimacy of the notion of change rests on the fact that, by necessity, the operationalization of that notion in economic explanation requires the assumption that reality, at least that part of reality that

⁹⁴ ‘{}’ in the original manuscript.

constitutes the object of explanation, is deterministic. And this assumption, Sraffa claims, is at odds with the way the world is.

Another criticism of the use of the marginalist method addresses the principle of continuity. Sraffa indicates that the principle of continuity in neoclassical theory is applied both to time and to matter. As applied to time, the principle of continuity consists in the consideration of intervals of time as small as possible. As applied to matter, the principle of continuity emerges from the consideration of infinitesimal variations in the use of a given factor of production or quantity demanded. The use of differential calculus in neoclassical theory directly brings the continuity assumptions.

In an unpublished note, Sraffa lists the principle of continuity as one of the problems in Marshall's analysis:

“The fundamental fallacies in Marshall are:

The principle of substitution

The principle of continuity, in its two applications:

1) To time ‘long and short periods’

2) To matter, ‘consideration of the margins as infinitesimals’

The metaphysics of utility-sacrifice

His one great contribution is the intelligent application of Leibnitz and Newton analysis to economics (...)”

Sraffa (D3/12/11/11, emphasis in the original)

It goes without saying that those fallacies in Marshall are present in the neoclassical theory in general, and so Sraffa's critique of Marshall's use of the principle of continuity extends to neoclassical theory in general, including the traditional neoclassical theory of capital.

But, after all, what is the problem with the principle of continuity? In a nutshell, Sraffa's objections to the principle of continuity are derived from considerations on the nature of time and the geometrical meaning of continuity. Firstly, time is not an arbitrary concept that one can make as small as necessary to serve one's intents. Instead, it corresponds to the lapse between cause and effect:

“‘Period of time’ is simply the interval (or relation) between the cause and its effect. This is a definite, not arbitrary unit/length. The cause takes place at one point in time, the effect explodes all at once at a different point of time.”

Sraffa (D3/12/11/40)

The idea behind Marshall’s use of infinitesimals is that economic reality is intricate, with phenomena being the effect of multiple, possibly interacting, causes. This, of course, raises challenges to the economist in her attempt to study reality (Martins, 2013). Marshall’s solution was to study one particular equilibrium at a time, focusing solely on the direct effects of a cause, leaving behind the indirect effects. Correspondingly, his analysis focuses on the short-run – when ‘causes’ have already produced their direct effects, while the indirect effects would, of course, influence the state of affairs, but only in the long run (see Martins, 2013, pp. 447–448). This way, the short-run corresponds to the lapse of time as small as necessary so that only direct effects can possibly occur. But since time is defined as the lapse between cause and (all its) effect(s), time is neither arbitrary nor continuous. Marshall’s separation between direct and indirect effects relies on an incorrect notion of time. Thus, the continuity of time in Marshall theory (and thus, in neoclassical theory), embodied in the mathematical apparatus of differential calculus, is unwarranted.

In order to dismantle the principle of continuity as applied to matter, Sraffa scrutinizes the meaning of continuity in terms of geometric categories like points, lines and planes. As his reasoning is technically challenging, drawing on fields beyond economics or philosophy, this argument is not explored here in detail (see Martins, 2013). In a nutshell, however, Sraffa’s point is that “we cannot analyse only two variables, as if the others were constant since even if we assume other variables to remain constant, there is still a different proportion between the variable that changed and the others that did not. So we must take all relevant variables into account” (Martins, 2013, p. 451). As Sraffa writes:

“But if the number of the means of production is more than two, most of the resulting positions, however close to the original, actual, one, will be impossible. For, in correspondence, with a rise (or fall) however small of r , the most profitable position will in general involve a change (some up and some down) in many or

all the quantities of means of production. And there is no ground for supposing that the change in ‘others’ is negligible compared with the change in the chosen one. Thus, as a result of assuming perfect continuity, we get in general as a result discontinuity or rather: even though all quantities may move continuously (i.e. go through all the intermediate positions) it will never be possible to change one, however small the increment, while keeping the others constant.

Sraffa (D3/12/42/70-71)

In short, “even if we admit continuity, for the sake of argument, the existence of more than two variables, and the interconnections between them, brings in discontinuity” (Martins, 2013, p. 453). While the principle of continuity as applied to time is logically flawed, when it is applied to matter it is unrealistic, in as much as reality is interconnected:

“Where marginism goes astray is in (falsely) assuming [...] that it has general applicability, whereas in fact it only applies exceptionally (in cases where partial change is feasible, there is independence, the whole is not affected)’

Sraffa (D3/12/42/9)

Sraffa’s critique of the use of the principle of continuity as applied to matter amounts to a critique of the atomism it presupposes, while the problem with the principle of continuity as applied to time is that it relies on a logically corrupted notion of time.

To sum up, then, Sraffa developed a metaphysical awareness and believed that methods must match the nature of the object. Overall, Sraffa criticises the marginalist method on the basis of, among other things, how reality is: indeterminate, constituted by non-atomistic agents, with an economic realm made up of by wholes, where phenomena have multiple causes. From this, it follows, once again, that Sraffa does not hold that reality is made up of closed systems. Sraffa’s rejection of neoclassical theory, and thus of the traditional neoclassical theory of capital, is ultimately ontological.⁹⁵

⁹⁵ An interesting question at this point, though irrelevant for my argument, is whether his main substantive piece, *Production of Commodities by Means of Commodities* (1960), does justice to his thought as reflected in his manuscripts. Contrary to neoclassical theory, including its formulation in the neoclassical theory of capital, Sraffa (1960) is not interested in changes – marginal magnitudes – and their use in explanations in terms of cause and effect relations. Instead, Sraffa’s strategy is to explain differences (between the values of different commodities) existing at any given point in time. The explanation of differences can not only do away with the problematic notion of ‘change’, but also with that of causality, and with the methodological challenge of incorporating time in economic theory. Thus, Sraffa’s economics focuses on

Second step: Joan Robinson

Sraffa's role in establishing the issues at stake in the capital controversies, as well as his intellectual leadership in Cambridge, are well known. As we have seen, Sraffa argues that neoclassical theory, as formulated in the traditional neoclassical theory of capital (and also in general equilibrium theory) is not appropriate to study the economy. Sraffa's position is based upon considerations about how the world really is: non-deterministic and constituted by wholes. My contention is that, to some degree, these considerations about how the world is are in some way shared by other Cambridge scholars, notably Robinson. This is not to suggest, of course, that the ontological considerations of these other scholars are always explicit or well-articulated. In any case, they must have played a role in determining their opposition to the neoclassical theory of capital. In what follows, I highlight three intertwined aspects of the Cambridge arguments, put forward by Robinson, that directly clash with the ontology of closed systems: uncertainty, the nature of equilibrium and time, and the embeddedness of the economic.

There is perhaps no better place to start than the paper that ignited the debate, Robinson (1953). The main point there, recall, concerns the circularity involved in the measurement of aggregate capital in neoclassical theory. But there is more to this than a simple technical difficulty in finding out how to count and value the machines, tools and

an 'instantaneous photograph' of the economy and asks what are the conditions of possibility for the repetition of that state (see Martins, 2013, pp. 9–11; see also Sraffa (D3/12/13/1:3). It pays to quote at length a portion of his manuscripts that would eventually lead to his book:

"This paper [the forthcoming book] deals with an extremely elementary problem; so elementary indeed that its solution is generally taken for granted. The problem is that of ascertaining the conditions of equilibrium of a system of prices & the rate of profits, independently of the study of the forces which may bring about such a state of equilibrium."

Sraffa (D3/12/15:2)

This way, Sraffa's theory, as put forward in 1960, is devoid of any causal statement. As he focuses on explaining differences, instead of accounting for changes, time is removed from the theory and thus no causation is involved. As Amartya Sen (2003, p. 1248) observes, Sraffa's equations must not be confused with statements of causality: there is a distinction between determining in a mathematical context and determining in a causal sense; mathematical determination is not necessarily causal determination. Sraffa's equations determine the values of existent commodities, without explaining the causes for those values. In other words, Sraffa's theory of value is *ex-post*: it establishes the necessary conditions for a given system of values, and thus for the reproduction of such a system. His theory is not, therefore, a 'predictive machine' for the values of commodities: it does not deal with time and it does not deal with change. In this light, Sraffa's (1960) does justice to his unpublished reflections, but of course, there is a huge debate on the proper interpretation of his book.

raw materials there are at a certain point in time in any given economy. The difficulties involved in measuring the stock of capital, Robinson claims, are connected to a deeper methodological problem. In her own words,

“In a position of equilibrium all three evaluations [of the stock of capital] yield equivalent results; there is a quantity which can be translated from one number to another by changing the unit. This is the definition of equilibrium. It entails that there have been no events over the relevant period of past time which have disturbed the relation between the various valuations of a given stock of goods and that the human beings in the situation are expecting the future to be just like the past - entirely devoid of such disturbing events. Then the rate of profit ruling to-day is the rate which was expected to rule to-day when the decision to invest in any capital good now extant was made, and the expected future receipts, capitalised at the current rate of profit, are equal to the cost of the capital goods which are expected to produce them.”

(Robinson, 1953, p. 83)

In this passage, Robinson is highlighting that the notion of equilibrium underlying the neoclassical theory of capital, which is, of course, related to the conception of capital deployed, is only valid in a state where the *expectations of agents turn out to be right*. For, “[w]hen an unexpected event occurs, the three ways of evaluating the stock of goods part company and no amount of juggling with units will bring them together again” (p. 84). Robinson observes that “there is a gap in time between investing money capital and receiving money profits and in that gap, events may occur which alter the value of money.” (Robinson, 1953, p. 84). Note that in this latter passage, Robinson seems to be referring to the actual world, rather than to some theory. The question, then, is whether theories that assume that somehow agents’ expectations are correct are insightful. She also notes:

“To abstract from uncertainty means to postulate that no such events occur, so that the ex ante expectations which govern the actions of the man of deeds are never out of gear with the ex post experience which governs the pronouncements of the man of words, and to say that equilibrium obtains is to say that no such events have occurred for some time, or are thought liable to occur in the future”

(Robinson, 1953, p. 83)

Robinson thus criticises the typical exercise of neoclassical economics of deploying the notion of equilibrium. Her contention is that such an approach is unable to account for an uncertain world. Given Keynes' influence on Robinson, it seems reasonable to suppose that she refers to fundamental uncertainty (see Gram & Walsh 1983, pp. 520–522, 548).

The impossibility of adequately addressing a world of uncertainty by means of an economic equilibrium was always a concern to Robinson, who returned to this issue:

“As soon as the uncertainty of expectations that guide economic behaviour is admitted, equilibrium drops out of the argument and history takes its place.”

(1974, p. 1)

That is, “the economy will not tend towards equilibrium because of the incorrect expectations of the individuals and of the uncertainty with which these expectations are held” (Garegnani, 1989, p. 350). Thus, comparisons between different equilibria (including steady states) can never be taken to be a study of changes, say of a sequence of equilibria in a process of accumulation. Change involves time, as Sraffa taught, hence uncertainty, which challenges the notion that the economy would reach a new equilibrium. “[T]he very process of moving has an effect upon the destination of the movement, so that there is no such thing as a position of long-run equilibrium which exists independently of the course which the economy is following at a particular date” (1953, p. 590)⁹⁶. Thus,

“The problem of the “measurement of capital” is a minor element in the criticism of the neo-classical doctrines. The major point is that what they pretend to offer as an alternative or rival to the post-Keynesian theory of accumulation is nothing but an error in methodology – a confusion between comparisons of imagined equilibrium positions and a process of accumulation going through history”

(Robinson, 1974, p. 11)

⁹⁶ Or, as Robinson puts it elsewhere: “The real source of trouble is the confusion between comparisons of equilibrium positions and the history of a process of accumulation. We might suppose that we can take a number of still photographs of economies each in stationary equilibrium; [...] This is an allowable thought experiment. But it is not allowable to flip the stills through a projector to obtain a moving picture of a process of accumulation” (1980, pp. 48, 53).

As Cohen and Harcourt note, “[t]he title of her 1975 paper, ‘The Unimportance of Reswitching’ (Robinson, 1975a), reflected her [Robinson’s] belief that, while reswitching and capital-reversing were problematic for neoclassical capital theory, her methodological critique was far more important” (2003, p. 204).

Another point to emphasise in Robinson’s critique of neoclassical theory, a point that is, of course, related to the above, concerns the idea that institutions are not negligible. For economic phenomena unfold in an institution conditioned environment. The idea that institutions are relevant appears in the writings of other economists of the Cambridge side. In Harcourt’s (1972) description of the debate, he observes that Cambridge economists

“see capitalist institutions – private property, an entrepreneurial class, a wage-earning class – as giving rise to conflicts between the classes. It is argued that the distribution between the classes of the net product (which is itself viewed as the surplus of commodities over those used up in its production) cannot be understood independently of the institutional nature of capitalism.”

(p. 2)

Robinson makes this point explicitly when she confesses that her views coincide with those of Thorstein Veblen, who, she observes, “made [her] point, much better than [she] did” (Robinson, 1980, p. 60) by articulating his own conception of capital:

“The continuum in which the abiding entity of capital resides is a continuity of ownership, not a physical fact. The continuity, in fact, is of an immaterial nature, a matter of legal rights, of contract, of purchase and sale. Just why this patent state of the case is overlooked, as it somewhat elaborately is, is not really seen”

quoted in Robinson (1980, p. 60)

Thus, Robinson praises Keynes (and the Classics – understood here as the economists before the marginalist revolution) because they “represent the structure and behaviour of the economy in which they were living”. And she adds that neoclassical economists “rarely say anything at all about the kind of economy to which the argument is to be applied” (Robinson, 1980, p. 58).

In terms of the worldview held by Cambridge economists, this emphasis on institutions is crucial. For it suggests that the economy is not in a vacuum, and so it can be influenced by external forces. As a consequence, relations between economic variables are contingent on the institutional framework in which they are observed. Thus, there seems to be no reason for the existence of universal regularities in economics. Regularities, if they arise at all, must be restricted to the particular institutional framework that gave rise to them. This, together with the points about uncertainty explored before, definitely call into question the appropriateness of the neoclassical commitment to closed systems.

In short, although Robinson ignited the debate by making public the issues involved with the measurement of capital and the associated problems of reswitching and capital reverse deepening, her methodological critique was far more important, as she came to realize herself. That methodological critique concerned the inability of neoclassical theory to address a world characterized by uncertainty. Furthermore, she was also concerned with the meaning of capital and the role that existing institutions have in shaping accumulation. Her methodological critique consisted, thus, in an attack against the unrealism of neoclassical theory, but especially against the ontological commitment of neoclassical theory to a world of closed systems.

Summary

Both the study of the ontological commitments of the neoclassical approach and the reconstruction of the Cambridge position suggest that there was an implicit and under-elaborated ontological question underlying the controversies. The traditional neoclassical theory of capital was committed to an ontology of closed systems, which is in turn associated to an atomistic and isolationist view of the economic world. In turn, Cambridge economists held that the economic world was open, and they did not think that neoclassical theory and, in particular, the traditional neoclassical theory of capital could produce explanations about an open world. Thus, despite the technical emphasis of the debate, the true source of disagreement was an ontological question. In sum, this chapter has concluded that

P5.2 The Cambridge capital controversies can be reinterpreted as a clash ignited by the belief that neoclassical theory was unable to address reality as it really is.

5.4 Concluding Remarks

The history of economics problem this chapter has attempted to render intelligible is the application of the traditional neoclassical theory of capital in real business cycles theory. This is puzzling because some years before the dawn of the real business cycles paradigm, in the context of the Cambridge capital controversies, that theory of capital was shown to be technically flawed, and this came to be accepted by neoclassical economists. In other words, this chapter has been concerned with the following proposition:

P5.1 The capital theory debate had little, if any, impact in the mainstream circles after the 70s, including in real business cycles theory, even though, during that debate, the neoclassical side recognized the flaws of its capital theory.

On top of that, there was an alternative neoclassical framework for capital which, to the eyes of neoclassical economists engaged in the capital controversy, was immune to the Cambridge critique. All this clearly suggests that, for some reason, the debate failed to have any impact beyond the years of discussion, despite the engagement of the most prominent figures in the discipline.

Accounting for this problem has meant reinterpreting the debate in the light of recent advances in the philosophy of economics. What is the account of the Cambridge capital controversies that emerges from the analysis made above? Considering the textual evidence gathered, the interpretation of the debate I propose is the following. The debate was fought essentially at the technical level (perhaps to facilitate communication, but arguably also because Cambridge economists were not immediately aware of the depth of their opposition to the mainstream), where the measurement of capital and the associated phenomena of reswitching and capital reverse deepening are central. The ultimate root of the Cambridge economists' resistance towards neoclassical theory is to be found at the ontological level, however. Implicitly at least, Cambridge economists believed that neoclassical theory was unable to address economic phenomena that occur

in an uncertain and internally related world. This explains the implicit opposition to the deductive nature of economic explanation characteristic of neoclassical economics, as noted by Cohen (1984). In short, as I suggest,

P5.2 The Cambridge capital controversies can be reinterpreted as a clash ignited by the belief that neoclassical theory was unable to address reality as it really is.

Since Cambridge were largely unaware of the true nature of their rejection of neoclassical theory, they hardly could challenge neoclassical theory differently. Moreover, since the technical arguments were using the same methodological language as neoclassical economics⁹⁷, it is unsurprising that neoclassical economists focused on those dimensions of the Cambridge critique.

How and why does this different interpretation of the debate explain my puzzle? Given the implicit ontological position of the neoclassical side, the arguments formulated by the Cambridge side, despite valid, were spurious and therefore insufficient to pose real problems to neoclassical theory. Spurious because, rather than targeting the underlying ontological question, they turned on some technical problems of the theory that could be, so thought neoclassical economists, overcome by adding some extra (perhaps mathematical) assumptions, or by invoking the empirical dimension of the problem. This way, after accepting the results of the first phase of the debate, neoclassical economists pondered the possibility of the empirical unimportance of those results. At this point, the debate was already being fought in terms of the general equilibrium theory, but the spectre of a defeated traditional neoclassical theory of capital was fading away, leading, ultimately, to the current indifference towards the debate. Given this, it is unsurprising that at the dawn of the real business cycles theory paradigm the traditional neoclassical theory of capital was among the possibilities for dealing with capital and ended up as the chosen framework. Thus:

P5.3 Since the essence of the Cambridge opposition was ontological but the debate never explicitly addressed ontology, the Cambridge criticisms

⁹⁷ Recall that those technical arguments constitute an immanent critique of the traditional neoclassical theory of capital and so are deductive in nature.

were not deadly, which accounts for the survival of the traditional neoclassical theory of capital.

Roy Weintraub once remarked that “[economists] look to the historian and wonder how the historian decides what is important, and how we go about deciding what will do into a future history book” (1999, p. 140). For historians of economics of a neoclassical persuasion, the Cambridge controversies in the theory of capital constitute nothing but an interlude in the refinement of the neoclassical approach, with no significant impact on the discipline. What this thesis argues is that that lack of measurable impact is a puzzle in the history of ideas. The reconstruction of the debate triggered by that puzzle emphasises the underlying role that ontology played in that debate, in shaping its course, and in determining its fate. In doing so, it partially accounts for some theoretical positions in modern mainstream (macro)economics. It is my impression that this work contributes to providing reasons for including a chapter on the Cambridge controversies in future books on the history of economics.

The greatest thing in this world is not so much where we stand as in what direction we are moving

(attributed to) Johann Wolfgang von Goethe

6 Economics and the Theory of Capital: Some Concluding Reflections

Whilst the last chapter can be said to illustrate Margaret MacMillan's remark that learning from history "is like looking in a rear-view mirror: if you only look back, you will land in the ditch, but it helps to know where you have come from and who else is on the road"⁹⁸ the present chapter exemplifies Dostoevsky's phrase that "there is no subject so old that something new cannot be said about it"⁹⁹.

The Cambridge capital controversies furnish crucial material to understand the current state of real business cycles theory. In particular, the interpretation of the controversies that chapter 5 articulates explains the options made in the 1980s as to how capital, or production more generally, would figure in the new generation of models.

This chapter elaborates on some other implications of the debate for modern economics, focussing on three topics. First, it comments on why the Cambridge economists are much less coherent than their orthodox, neoclassical opponents and connects this to a contemporary challenge in the study of, and in defining heterodox economics. Second, some insights forged in the debate are rehashed to provide a critical reading of a recent project engaged with improving current mainstream macroeconomics, the *Rebuilding Macroeconomic Theory Project*. Finally, some of the criticisms made by Cambridge economists are retrieved with a view to articulating, albeit in a fragmentary fashion, a wider framework to think about capital.

⁹⁸ Quoted from *The Uses and Abuses of History* (2008).

⁹⁹ Quoted from *A Writer's Diary (1873-1881)*.

6.1 The Cambridge Controversies in the Context of Heterodox Economics

The Cambridge controversies in the theory of capital constitute a seminal moment for a heterodox stream known as post-Keynesian economics. The debate is “the point where post-Keynesians part ways with the mainstream and emerge as a separate body.” Yet “[t]he Post Keynesian economics group took shape in the U.S. during the early to mid-1970s” (Mata, 2004, p. 257, p. 250). That is, though inspired by some Cambridge economists and spurred by the Cambridge capital controversies, post-Keynesian economics is mainly American, a view that is more or less consensual (see Cohen, 1984, 1985; Dow, 1980; Lee, 2009; M. Lavoie, 1992).

The Cambridge capital controversies are also connected to the emergence of a very particular group of economists, the Sraffians or neo-Ricardians¹⁰⁰. As the labels suggest, the intellectual origin of this group of economists is the reading Sraffa provided of David Ricardo, which inspired not only his critique of mainstream economic theory but also his *Production of commodities by means of commodities* (1960). The Sraffian approach is thus a by-product of the debate. Beyond Sraffa himself, other important economists often linked to this group are, for example, Garegnani, Eatwell, Bharadwaj, or Milgate¹⁰¹.

Perhaps somewhat surprisingly, one of the most contentious issues in the analysis of the post-Keynesian tradition turns precisely on the relationship between post-Keynesianism and neo-Ricardianism. Some regard neo-Ricardianism as a strand within post-Keynesianism, or at least consider that their coherence is achievable (see Harcourt, 1981). Harcourt and Hamouda (1989), for instance, claim that neo-Ricardianism constitutes one of the various branches of post-Keynesian economics, each branch focusing on different issues and deploying distinct levels of abstraction. Lavoie (2013) similarly claims that separating post-Keynesians and Sraffians rests on a mistaken

¹⁰⁰ Here I avoid discussing the appropriateness of the label. That has been done countless times, and despite the fact that those belonging to this tradition dispute the descriptive precision of the term, the fact remains that the label has kept its relevance (see Eatwell, 1974; Milgate et al., 1991; Pratten, 1996).

¹⁰¹ Despite the centrality of Sraffa and his work in the emergence of this relatively autonomous stream of thought, some authors have pointed out there exists an array of approaches (‘the Sraffian schools’) within what is conventionally referred to as neo-Ricardianism (see Roncaglia, 1991).

interpretation of their contributions. Others, in turn, disagree: enduring and considerable disagreements between both groups are invoked to substantiate the claim that they constitute separate traditions (see e.g. Dow, 2001; Dunn, 2000). Those disagreements are often found at the methodological level, namely on the relevance of equilibrium analysis in economics. Thus, Alessandro Roncaglia (1995, p. 120) suggests that the key point to settle this issue is related to the interpretation of the notion of ‘long-period centres of gravitation’. In particular, it is not clear whether the long-period equilibrium approach – favoured by neo-Ricardians – is compatible with the (post-Keynesian) idea of path dependency and appropriate to study a world pervaded by uncertainty.

As it happens, this was precisely the question that ignited the Cambridge mini-controversy I referred to in the last chapter. The criticisms of neoclassical theory made by Cambridge economists that became more popular and deserved direct neoclassical replies relate to phenomena of reswitching and capital reverse deepening. But recall that Robinson, in her important 1953 paper, also attacked neoclassical theory on methodological grounds. Her critique hinged upon the use of equilibrium constructs to study a world that is uncertain. This was clarified in 1975 when she published ‘*The unimportance of reswitching*’. Similar concerns, as we have seen, pervade Sraffa’s unpublished manuscripts, which substantiate his rejection of neoclassical theory and eventually led to his book ‘*Production of commodities by means for commodities*’.

It is with regard to the interpretation of Sraffa (1960) that underlying disputes among Cambridge economists come to the surface. In fact, a more complicated story of the nature of the Cambridge position in the capital controversies arises when one examines the contributions of Sraffians like Garegnani, Eatwell, Milgate and others. These authors tend to argue that Sraffa’s (1960) system describes a long-period equilibrium around which the actual values for the variables are gravitating. Consider, for instance, this methodological pronouncement by Eatwell:

“In defining the object of analysis and identifying the forces which determine it, the assumption is made, implicitly, that the forces of which the theory is constituted are the more dominant, systematic and persistent. Transitory and arbitrary phenomena are abstracted from intentionally: as are those forces which are related to specific circumstances as opposed to the general case. The dominant

forces are expressed in algebraic form, as functions and constants, and constitute the data of the theory. The model may then (if it has been specified correctly) be solved to determine the magnitude of the object. It is known that, except by a fluke, the magnitude determined as a solution will not be exactly that observed in reality. It cannot be, since a variety of transitory forces, known and unknown, have been excluded. None the less, since the theory is constructed on the basis of dominant and persistent forces, the magnitude determined by the analysis is the centre of gravity of the actual magnitude of the object.”

(Eatwell, 1983, pp. 94–95)

Robinson rejected this approach, as demonstrated in the last chapter (see also Cohen & Harcourt, 2003, pp. 204–205); which triggered a controversy between her and Sraffa’s followers that went far beyond the capital controversies (see M. Lavoie, 2013, pp. 43–49).

As Pratten (1996) points out, the approach taken by the neo-Ricardians is at odds with their explicit account of the nature of the world. Neo-Ricardians believe that deductive theory serves to account for the core – the theory of value and the long-period equilibrium – whilst they acknowledge the complexities and the open nature of world: “while recognising the reality of open systems, neo-Ricardianism, in its method, remains tied to closed systems; its results are formulated in terms of outcomes, events, states of affairs, and their correlations or ‘functional relations’, and remains clearly deductivist in structure” (Pratten, 1996, p. 32). Neo-Ricardians, in short, seem unaware of the incompatibility of their explicitly stated world view with the methods they employ.

I suspect that, at some level at least, Robinson understood the inconsistency between her views and the neo-Ricardian approach (as practised by Sraffa’s followers), which led to the mini-controversy regarding the role of equilibrium. The difficulty in defining a post-Keynesian tradition arguably mirrors the difficulty in establishing a comprehensive, coherent position of the Cambridge side during the controversies.

The last chapter argued that there was an ontological position underlying the Cambridge objection to neoclassical capital theory, and to neoclassical economics more generally. Now, Lawson’s conception of heterodox economics is that it “is first and foremost a rejection of modern mainstream economics [where] the latter consists in the

insistence that forms of mathematical–deductive method should everywhere be utilised [and so] heterodox economics, in the first instance, is just a rejection of this emphasis” (Lawson, 2006, p. 492). The reason for the rejection of that emphasis on mathematical–deductive methods, Lawson argues, is an implicit ‘orientation to ontology’ (pp. 497–498). In this sense, the Cambridge side in the Cambridge capital controversies is clearly heterodox.

However, the classification of the neo-Ricardian as a heterodox tradition remains problematic. For, on the one hand, neo-Ricardians clearly oppose neoclassical economics, on explicit or implicit ontological grounds; but, on the other, they fail to free themselves from deductivism. That is, though they identify themselves as heterodox economists – also because they propose different policies, though that is immaterial at this juncture – they end up deploying mathematical–deductive methods.

Lawson’s (2013) ‘*What is this ‘school’ called neoclassical economics?*’ helps us to justify where to place the neo-Ricardians. In this article, Lawson is interested in developing a sustainable conception of neoclassical economics, a term loosely defined and often incoherently applied¹⁰². His strategy involves retrieving Veblen’s original usage of the term, from which Lawson infers that neoclassical economists are

“those who are aware (at some level) that social reality is of a causal-processual nature as elaborated above, who prioritise the goal of being realistic, and yet who fail themselves fully to recognise or to accept the limited scope for any overly-taxonomic approach including, in particular, one that makes significant use of methods of mathematical deductive modelling.”

(2013, p. 970)

This definition has two ironical implications. First, some of those who self-identify as heterodox are actually neoclassical in Lawson’s sense. Second, neoclassical economists are ontologically part of heterodoxy, though they are methodologically inconsistent. Whatever the merits of this provocative conception, it does capture a tension within the group self-styled as heterodox economics. Economists or traditions that oppose the mainstream on ontological grounds – that hold a worldview incorporating open systems

¹⁰² Until now, I have used the terms ‘neoclassical’ and ‘mainstream’ interchangeably. The distinction highlighted in Lawson (2013) is immaterial for the argument proposed in the previous chapters.

– often do use methods that suppose a world of closed systems. The neo-Ricardians clearly do.

6.2 The Cambridge Controversies and the ‘Rebuilding Macroeconomic Theory Project’

The 2007/2008 financial crisis and the subsequent years of slow recovery are often claimed to pose a challenge to economics. For, as I noted earlier, not only was economic theory unable to predict the crisis, it also seemed unable to prescribe a clear solution for it. This, of course, is the general impression of the public but also that of important and influential economists (see Colander et al., 2009).

It was in this background that some initiatives sharing the diagnosis that economics was in a bad shape and that some sort of change was needed took roots. For better or for worse, perhaps the most successful, or impactful, initiative is the creation of the *Institute for New Economic Thinking*¹⁰³. Here, however, I would like to focus on a more recent and targeted initiative, the *Rebuilding Macroeconomic Theory Project* promoted by the *Oxford Review of Economic Policy* and headed by David Vines and Samuel Wills (2018). The aim of this project is to inquire how the standard macroeconomic model – the new-Keynesian dynamic stochastic general equilibrium model¹⁰⁴, the skeleton of which, so to speak, is the real business cycles model – should be altered in the face of the challenges posed by the 2007/2008 crisis and its consequences. Vines and Wills note that

“the Great Moderation collapsed into another crisis: the global financial crisis, or GFC. When this happened, the macroeconomic experts – who were by now in charge – appeared to lack both competence and humility. As a result of the GFC we are no longer clear what macroeconomic theory should look like, or what to

¹⁰³ The *Institute for New Economic Thinking* is a heavily funded organization – one of its co-founders, and certainly one of its main sources of funds, is George Soros – “devoted to developing and sharing the ideas that can repair our broken economy and create a more equal, prosperous, and just society” (taken from the website <https://www.ineteconomics.org/about/our-purpose>, in November 2019).

¹⁰⁴ The label ‘new-Keynesian’ is used to denote the fact that these are sticky-price models. The idea of price rigidity an essential element in Hicks’s (1937) interpretation of Keynes’s general theory.

teach the next generation of students. We are still looking for the kind of constructive response to this crisis that Keynes produced in the 1930s.”

(2018, p. 2)

They continue,

“During the Great Moderation, the New Keynesian Dynamic Stochastic General Equilibrium (DSGE) model had become the ‘benchmark model’: the one taught to students at the start of the first-year graduate macro course. Many of us – although not all – were proud of what had been achieved. But the benchmark model has let us down; it explained neither why the GFC happened, nor what to do about it. What new ideas are needed? What needs to be thrown away? What might a new benchmark model look like? Will there be a ‘paradigm shift’? And how should the new model to be used in our teaching – let us call it the ‘new core model’ – relate to the evidence-based professional work that macroeconomists do when giving policy advice?”

(2018, p. 2)

In the pursuit of that ‘new core model’, a few, very influential economists were invited for a series of meetings and conferences, which culminated in the publication of a special edition of the *Oxford Review of Economic Policy*¹⁰⁵. Very briefly, the main conclusions of the project are that the new-Keynesian dynamic stochastic general equilibrium model can and should be improved, through four main changes: the inclusion of financial frictions, the relaxing of rational expectations assumptions, the inclusion of heterogeneous agents, and the use of more appropriate micro-foundations (Vines & Wills, 2018, p. 4).

Assessing the possible merits and shortfalls of this project, including the substantive proposals for a renewed macroeconomics, would be a subject for an entire thesis. So here I just focus on a simpler task. Bearing in mind insights from the Cambridge capital controversies, I point out some difficulties with the idea that the new-Keynesian dynamic stochastic general equilibrium model needs ‘more appropriate micro-foundations’. I do this without questioning the implicit assumptions of mainstream

¹⁰⁵ The economists in question are Olivier Blanchard, Simon Wren-Lewis, Joseph Stiglitz, Randall Wright, Ricardo Reis, Paul Krugman, Wendy Carlin, David Soskice, Fabio Ghironi, A. G. Haldane, A. E. Turrell, Jesper Lindé, David Hendry, John Muellbauer, Warwick McKibbin, and Andrew Stoeckel.

economists regarding economic explanation. This exercise, then, speaks the language of those who are committed to the basic tenets of the mainstream approach.

The question of appropriate micro-foundations is directly related to some issues addressed during the controversies and is presently regarded as the most relevant aspect for the improvement of the standard model (Vines & Wills, 2018, p. 23). Yet different authors have different understandings as to what exactly is the right direction. Let me give some examples. Paul Krugman (2018) and Olivier Blanchard (2018), for instance, claim that the relation between inflation, wages, and unemployment as predicted by the model do not quite match the data. One possible way of addressing this, they suggest, involves dealing, among other things, with the model's price determination mechanisms (Blanchard, 2018, pp. 47, 49–50; Krugman, 2018, p. 163). For instance, it seems insufficient to model price stickiness by merely assuming some sort of price-adjustment cost. Doing it that way is simply assuming price stickiness, which, as seems to be suggested (Blanchard, 2018, p. 51), should instead be derived: should have micro-foundations.

Randall Wright (2018), on the other hand, claims that the right way to improve the standard model is by endogenizing some key economic categories: money, the banking system, and institutions in general (Wright, 2018, pp. 108–111). Instead of simply assuming the existence of money, of a banking system and other institutions and features – price stickiness, for example – and deriving the ways in which they possibly affect the results of the model, economists should model those institutions and features as outcomes of the endogenous interaction between agents within an economic environment possibly containing exogenous rigidities, such as imperfect information, limited commitment, and so forth (Wright, 2018, p. 114).

Other contributors to this project argue for an entirely different approach to micro-foundations. Instead of treating micro-foundations as the explicit derivation of structural equations for the macroeconomy from the optimization of the utility function of some representative agent, they suggest the use of simulation methods and agent-based models (Haldane & Turrell, 2018). These kinds of models should be “thought of as algorithms for aggregating the behaviours of individual actors” (Haldane & Turrell, 2018, p. 243). These individual actors are typically heterogeneous and behave according to a set of rules.

Haldane and Turrell (2018, p. 243) argue that this approach can complement the currently existing ones, but is especially useful “where interactions between agents really matter, where heuristics dominate, where the heterogeneity of agents is important, where policies have agent-level implications, where granular data are plentiful, and where analytical methods fail” (Haldane & Turrell, 2018, p. 237).

The examples just listed are fairly representative of the vision professed by the economists contributing to the project regarding the future of micro-foundations in the new-Keynesian model. Perhaps more telling than those suggestions is, however, what is neglected. A problem with new-Keynesian models – it follows from the previous chapters – concerns the way capital is treated. As pointed out before, conceiving of capital as a single factor of production (here, as a homogeneous quantity) is unwarranted as is the use of an aggregate production function. Technically, therefore, the relations derived from such a function, especially those in which the quantity of capital is (directly and indirectly) involved, cannot be said to be micro-founded¹⁰⁶. More appropriate micro-foundations would require changing the way capital, and therefore, the entire production sphere, is treated in new-Keynesian models.

To be sure, two economists contributing to the project, Blanchard and Stiglitz, display awareness (or at least intuition) that a problem with capital does exist in new-Keynesian models. Blanchard observes that one of the discussions to be had concerns the difficulties in dealing with aggregation problems (2018, p. 50). He later notes that “aggregation and heterogeneity lead to much more complex aggregate dynamics than a tight theoretical model can hope to capture” (2018, p. 53). Stiglitz, in turn, is much more explicit in connecting the potential problems of aggregation with the difficulties of dealing with capital:

“Long ago we learned the difficulties of constructing an aggregate production function. The ‘putty-putty’ model provides great simplification, but one should not claim that any analysis based on it is really ‘microfounded’. While earlier analyses provided a critique of the use of the standard model for equilibrium analysis, e.g. when there is production of commodities by means of commodities

¹⁰⁶ Reswitching and capital reverse deepening show precisely this: it is not possible to aggregate multiple techniques of production into a single, aggregate production function.

or when there are production processes involving capital goods of markedly different durability; the use is even more questionable for analyses of dynamics: the dynamics of putty-clay models and vintage capital models, for instance, are markedly different from those of putty-putty models. It would thus be foolhardy to rely on the putty-clay model for any analysis of dynamics in the short to medium term when such vintage effects can be important.”¹⁰⁷

(Stiglitz, 2018 p. 88)

At the end of the day, however, both Blanchard and Stiglitz shy away from any commitment to a solution to this particular set of problems. Stiglitz seems to suggest a possible way forward, though. This is to consider (some) heterogeneity of capital goods, by differentiating them according to their age, or according to their character (circulating versus durable capital, for instance). Instead of a stock of homogeneous capital, whose mathematical expression is a scalar, the theory could perhaps introduce a set of heterogeneous capital goods, expressed as a vector.

The only meaningful way to introduce a heterogeneous conception of capital in the new-Keynesian dynamic stochastic general equilibrium model is to consider the endowment of each variety of capital goods as given for the determination of equilibrium – precisely as (disaggregated) neo-Walrasian models of general equilibrium do. The initial composition of the stock of capital is therefore arbitrary, and so only by chance will it be compatible with the equilibrium conditions defined by consumer preferences and the available methods of production. Since this initial composition is *given*, the pressures that preferences and technology create, through supply and demand, for the adjustment of that composition are inconsequential as far as the determination of equilibrium goes. One implication is that the assumption of a uniform rate of profits cannot be held¹⁰⁸. Now, considering that if the composition of the capital stock is not compatible with a uniform rate of profits that would immediately lead to changes in that composition, the equilibrium that is determined in a setting in which the composition of

¹⁰⁷ Putty-clay models take capital to be either durable or circulating. Circulating capital can be transformed into durable capital, but not the other way around. Putty-putty capital corresponds to the homogeneous capital that can take any form dictated by the equilibrium conditions of the model (fully malleable capital). Finally, vintage models differentiate capital goods with respect to their age.

¹⁰⁸ In other words, if the composition of the capital stock is not in sync with the goods and services demanded and the technology available, the existing arbitrage opportunities cannot be exploited and so there is no adjustment in the capital stock, and so no tendency for a uniform rate of profits.

capital is given is a very short-period equilibrium (see Petri, 2004, pp. 33–35). Since new-Keynesian models traditionally determine a long-period equilibrium, the use of a heterogeneous concept of capital changes the nature of equilibrium: the version of the model with heterogeneous capital would determine a (sequence of) very short-period equilibria.

This solution, however, brings further problems to the new-Keynesian dynamic general equilibrium model, problems that were first identified during, or in the aftermath of, the Cambridge controversies. Here I focus on two of them.

The first concerns the complications that would arise at the theory (as opposed to the technical) level from the conception of capital as a vector of heterogeneous goods¹⁰⁹. With heterogeneous capital goods, different methods of production would differ not only with respect to the capital intensity necessary for the production of a unit of output (as is the case with standard production functions) but also with respect to the *quality*, or *type*, of capital goods required. This means that the substitutability between (heterogeneous) capital goods is not perfect.

Thus, an increase in the wage rate, for instance, may not lead to an increase in the capital intensity in the economy, as predicted by the traditional neoclassical theory of capital. This is so because the traditional substitution effects can be obstructed in a theory that takes as given the endowment of each variety of capital goods. With this constraint, the composition of the stock of capital is not allowed to change in response to changes in relative prices. Accordingly, there may be a shortage of some capital goods while others may be in excess demand. This hinders firms' capacity to change their methods of production according to the typical substitution mechanisms, which in turn inhibits the existence of well-behaved employment functions and of demand functions for the factors of production (see Garegnani, 1990, pp. 57–58). This could, in turn, preclude the implementation of equilibrium, which conflicts with the methodological motto according to which the study of economic dynamics can be done through the study of a sequence of equilibria. A change in the notion of capital demands, it seems, further changes in the basic theoretical and methodological structure of the new-Keynesian model¹¹⁰.

¹⁰⁹ The introduction of a disaggregated, heterogeneous notion of capital would obviously lead to a more complex representation of technology than in the case of an aggregate production function.

¹¹⁰ One could be tempted to see here a further (insufficient) reason for using the traditional neoclassical

The second problem I want to highlight is the following: regardless of the complications a different conception of capital involves, would it be possible to do away with a conception of capital as a single factor of production, given the dynamic general equilibrium approach these economists, both implicitly and explicitly, desire to preserve? Among the elements of this approach is the method of studying economic dynamics through the analysis of sequences of equilibria (i.e., the economy is treated as if it is always in equilibrium) and the conception that those equilibria correspond to a solution of a system of equations representing the equality of supply and demand in all markets.

Garegnani (1990, 2000), in a kind of afterthought to the debate, argues that capital as a single quantity would emerge even in the general equilibrium theory taking capital as a set of heterogeneous factors of production – which would make the theory vulnerable to the criticisms developed during the Cambridge capital controversies. In a heterogeneous capital setting, the quantity of capital emerges in the savings-investment market, which implicitly relies on a decreasing demand function for investment. From the point of view of savers, it is indifferent which kind of capital good is acquired with their saved income. In fact, what savers demand is simply a return. As such, capital goods, from the viewpoint of savers, are perfect substitutes with respect to the rate of return. Now, if for some exogenous reason (say a change in preferences) savers decide to increase their savings, this means that, for the same interest rate, there are now more available funds for purchasing capital goods. At the same time, some resources previously employed in the production of consumer goods are now idle. Accordingly, the general equilibrium can only be reached if, following an increase in saving, investment also increases. These new investments should, given the full employment assumptions, absorb the production of the new capital goods demanded, absorb the idle resources, including capital goods, from the consumer goods sector, and provide for the future increase in consumption. Now, resources diverted from the production of consumer goods are not necessarily compatible with the requirements for the production of new capital goods, for instance. Thus, the necessary adjustments in the savings-investment market following an increase in savings, for instance, can only be meaningfully assumed if capital goods are perfect substitutes; the same is to say, if they are homogeneous. Otherwise, the savings-

theory of capital in real business cycles settings.

investment market would be subject to multiple equilibria as the conventional relations between investment and interest rate are not guaranteed.

Assuming full employment – in particular, assuming that the savings market reaches a full-employment equilibrium – commits the theory to a conception of capital as a value magnitude. At the end of the day, therefore, the theory cannot avoid a conception of capital as a single factor. As such, the critiques made by the Cambridge side of the controversies apply, even to a modified version of the new-Keynesian general equilibrium model.

The *Rebuilding Macroeconomic Theory Project* fails in not considering the lessons taught by the Cambridge capital controversies: it neglects one of the most important problems regarding the micro-foundations of the standard model. What the foregoing suggests is that preserving the main tenets of the model while introducing some degree of capital heterogeneity into the theory – the most obvious solution to the problem – may bring about new difficulties. The corollary, perhaps, is that we need to take a radically different route for dealing with capital. This is the topic I briefly touch upon in the next section.

6.3 Considerations on a Framework for Capital

If I were to sum up the most noteworthy criticisms of the neoclassical theory of capital made by Cambridge economists in one single paragraph, it would be as follows. The neoclassical theory of capital fails to address a world pervaded by uncertainty. Human beings do not mechanically react to impulses; their environment is relevant, as is their capacity to learn and to form expectations. Thus, the way human agents behave is not independent of the conditions of time and space, which of course includes the specific institutions in the context of which economic phenomena unfold. Because the world is uncertain, the future is unknown and so expectations are falsifiable. Hence, neoclassical theory and its notion of equilibrium are useless. A historical approach is required.

Though they offered a particularly incisive criticism of neoclassical theory in general and of the neoclassical theory of capital in particular, Cambridge economists did not live up to the challenge of establishing an alternative framework. To cite but one example, it is hard to see how the way in which Robinson deals with expectations in her

piece on the ‘Accumulation of Capital’ (1956) – which amounts to assuming that expectations are given and mirroring the current situation (p. 65) – is consistent with the idea that the world is uncertain and thus that the current situation is bound to change. The same could be said about the notion of “golden age” (p. 99). Although the Cambridge side were correct in many of their criticisms of the neoclassical theory, they lacked the conceptual framework to make better use of their insights.

The question which necessarily follows is how one should think about capital within a framework alternative to neoclassical thought and consistent with the explicit and implicit criticisms of the neoclassical theory of capital. In what follows, I venture to broadly articulate some elements of such a framework. Obviously, many routes could be followed in such an endeavour. What follows is influenced not only by the Cambridge controversies but also by a bunch of other texts, namely on social ontology and by some authors of the Austrian tradition¹¹¹.

In order to understand how to think about capital, and indeed about any other matter of interest, one needs to understand the nature of the world. Thus, I start by following Lawson’s advice that the work of science must rely on explicit ontological analysis.

Regarding the nature of the social world, the very first element to behold, one that follows from the Cambridge position in the debate, is the conception that social reality constitutes an *open system*. Open systems, recall, are systems in which spontaneous event regularities, particularly those of a causal sequence, are merely accidental and thus rare. That the social world is an open system is, first and foremost, an ex-post observation (Lawson 1997), consistent with the fundamental uncertainty characterising the social world. But openness is also implied and presupposed by other features of the social world. Take, for instance, the fact that human beings make real choices. This means that they could have always done otherwise. They could have chosen differently in the very same circumstances simply because they could have different expectations about the implications of choosing any of the options available to them, or because they interpreted differently their own circumstances¹¹².

¹¹¹ Among the texts that influenced me most are D. Lavoie (1991); Lawson (1997); Mises (1949); and Shackle (1991).

¹¹² This echoes Lachmann’s thought, as explored by Lewis and Runde (2007).

From an epistemological viewpoint, the assertion that the social world is open raises a particularly important question, especially in the face of the current practices of mainstream economics. If there are virtually no spontaneous regularities of causal sequence, what is there to be known about the social world? What shall social science and economics endeavour to explain? Is economic explanation even possible?

To answer those questions, more needs to be established about the nature and constitution of the social world. If human choice is real, then agents need to possess some knowledge of the circumstances of their actions. They need to know their ends, their expectations about the outcomes of their actions and what actions can be taken in the light of their particular ends. That is, human action is intentional, and intentionality presupposes that agents know something about what they are doing. Now, the object of this knowledge cannot be events and states of affairs, since the world is open. Rather, objects of knowledge must lie at a different level of reality, sufficiently stable to allow agents to (fallibly) know it. Lawson calls that level of reality the level of structures (see, e.g., Lawson, 1997, pp. 30-31), observing that structures “govern, but are irreducible to events, including human activities” (Lawson 1997, p.30). Furthermore, social reality constitutes a

“structured realm of emergent phenomena that, amongst other things, are processual (being constantly reproduced and transformed through the human practices on which they depend), highly internally related (meaning constituted through [...] their relations with each other – e.g., employer/employee or teacher/student relations), value-laden and meaningful, amongst much else”

(Lawson, 2012a, p. 4).

Structures are not directly experienced or observed, but they are nonetheless real in that they condition, facilitate, or causally contribute to the events or states of affairs human beings experience. If triggered, the causal powers of structures generate tendencies, potentialities which are in play even if they are not actually manifest. The extent to which they are depends on other causal powers that may be operating. This is why structures and events are often out of phase with one another.

So long as pure theory (as opposed to applied theory) goes, economics should, therefore, aim at

“identifying and illuminating the structures and mechanisms, powers and tendencies, that govern or facilitate the course of events. The scientific objective is to identify relatively enduring structures and to understand their characteristic ways of acting. Explanation ... entails providing an account of those structures, powers and tendencies that have contributed to the production of, or facilitated, some already identified phenomenon of interest. It is by reference to enduring powers, mechanisms and associated tendencies, that the phenomena of the world are explained.”

(Lawson, 1997, p. 22)

Whilst they are a condition of the possibility of human intentional agency, social structures are also the product of human action and understanding. It is because of this feature that they are termed ‘social’ (as opposed to natural). Their mechanisms, powers and tendencies are mediated by human agency.

Beyond social structures and events or states of affairs, there is an eminently subjective level of reality made up of agent’s experiences, perceptions, or interpretations. One implication is that the study of human agency calls for a consideration of the subjective nature of the act of choice. Not only human beings have different preferences, but they also hold subjective interpretations of the objective facts of their circumstances and formulate (subjective) expectations.

Social science, and economics in particular, must thus address expectations and how they are formed. The general scheme to integrate expectations into economic analysis, in a way that is consistent with the observation that the social world is uncertain and that social structure conditions and is conditioned by human action, is provided by authors of the Austrian tradition. Mises puts it as follows:

“Inheritance and environment direct a man’s actions. They suggest to him both the ends and the means. He lives not simply as man in abstracto; he lives as a son of his family, his people, and his age; as a member of a definite social group; as a practitioner of a certain vocation; as a follower of definite religious, metaphysical, philosophical ideas; as a partisan in many feuds and controversies. He does not himself create his ideas and standards of value; he borrows them from other people. His ideology is what his environment enjoins upon him”

(Mises, 1949, p. 46)

As a result, the expectations agents create are conditioned by the social environment – which may be rephrased as ‘social structure’ – which is then transformed by the actions of human beings¹¹³. As Lewis and Runde (2007) note, the conditioning exercised by the social environment, i.e., social structure, “is of paramount importance because in attempting to divine the significance of price signals, say, people are able to transcend a purely subjective (and therefore potentially arbitrary and idiosyncratic) viewpoint, and so avoid lapsing into solipsism, only by drawing on the traditional conceptual schemes they share with other members of their society” (p. 206).

The foregoing suggests that hermeneutical processes – processes of interpretation which are by necessity subjective, but nonetheless conditioned by pre-existing, objective, social structures – are crucial to conceptualize the agency-structure relation and hence central for economic explanation. But what does this mean for the proper way to think about capital?

From the point of view of much of economic theory to date, capital consists of produced means of production or, alternatively, a sum of money destined to feed a productive process. In any case, capital is something that can yield an income. But, if capital, as anything social, has to be accounted for along the lines of the agency-structure relationship sketched above, a more interesting question to pose is how anything becomes capital. When one asserts that something – be it an artefact or a sum of money - has the property or quality of being capital, one is referring to the economic function of that thing. And, regarding the economic function of things, Lachmann observes that

“The generic concept of capital without which economists cannot do their work has no measurable counterpart among material objects; it reflects the entrepreneurial appraisal of such objects. Beer barrels and blast furnaces, harbour installations and hotel-room furniture are capital not by virtue of their physical properties but by virtue of their economic functions. Something is capital because the market, the consensus of entrepreneurial minds, regards it as capable of yielding an income”

¹¹³ See Ferlito (2018, p. 21).

(Lachmann, 1956, p. xv)

Thus, the emergence of capital is a social process: it is the outcome of human action and interaction. In general, the assignment of economic function to things can be conceived as a process of social positioning. Processes of social positioning are easily identified when one is speaking about human beings. The social identity of human beings follows from their placement in different social positions, to which a complex of rights and obligations are associated. Processes of social positioning are not exclusive of human beings, though:

“Social reality comprises in some part a multitude of inanimate objects, mostly humanly constructed as artefacts, that obtain social identities through being socially positioned in various ways”

(Lawson, 2012b, p. 376).

Of course, it does not make sense about talk about rights and obligations attached to inanimate objects. After all, human agency is presupposed in at least some of the rights and obligations associated with the positioned object for those are supposed to be exercised by whoever happens to be slotted into the position, or by whoever stands in some relation to that position. What is implicated by the positioning of an artefact is a complex of rules that condition the way agents interact with that artefact. Instead of being bearers of rights and obligations, objects are bearers of system functions. As Lawson, writes:

“Notice first that when an *artefact* is positioned as, for example, a paperweight, traffic beacon, door or identity card, certain of its causal capacities become interpreted as its (positional) *functions*. The latter are interpreted as functions within and relative to the system in which it is positioned.

(Lawson, 2015e, p. 9)

Before being positioned, say, as capital – or assigned the economic function of capital – artefacts have been thought suitable for yielding an income. This expectation is the product of a hermeneutical process, which in turn is conditioned by social structure. Indeed,

“the economic role of something seldom depends on the attitudes of a single individual, but rather on appropriate relations between the attitudes of several agents ... This dependence is not merely a result of the intersubjective dependence of attitudes themselves. It is also due to the fact that the role of things is defined by the *articulation* of several agents’ attitudes.”

(Lourenço & Graça Moura, 2018, p. 11)

Rebuilding economics certainly necessitates a new framework for capital: a meta-theory of capital. That meta-theory should emphasise the open nature of the world, the centrality of human agency and the importance of the subjective aspects of human action. The scheme tentatively and fragmentarily set out above is a step towards that aim.

Capital theory is central for various explanatory projects within economics. Not incidentally, it is at the centre of almost every theoretical dispute within the field. Bliss, to cite one example, notes that “[w]hen economists reach agreement on the theory of capital they will shortly reach agreement on everything else” (1975, p. vii). I am not so sure. In any case, there is something to be learned from the Cambridge controversies and the current indifference to them. Accepting the teachings the controversies have to offer implies rebuilding much of economic theory, and not just its hugely dominant mainstream. Like the neo-Ricardians, many self-styled heterodox economists cannot escape the mainstream’s methodological rulebook

References

- Arrow, K. J., & Debreu, G. (1954). Existence of an Equilibrium for a Competitive Economy. *Econometrica*, 22(3), 265.
- Arrow, K. J., & Hahn, F. (1971). *General Competitive Analysis*. San Francisco: Holden-Day.
- Backhouse, R. (2014). MIT and the Other Cambridge. *History of Political Economy*, 46(Supplement 1), 252–271.
- Backhouse, RE. (2012). *Interpreting Macroeconomics: Explorations in the History of Macroeconomic Thought*. Routledge.
- Backhouse, Roger. (1994). *New Directions in Economic Methodology. Economics as Social Theory*. London; New York: Routledge.
- Backhouse, Roger. (2000). Progress in Heterodox Economics. *Journal of the History of Economic Thought*, 22(02), 149–155.
- Barro, R. J. (1976). Rational Expectations and the Role of Monetary Policy. *Journal of Monetary Economics*, 2(1), 1–32.
- Bhaskar, R. (1975). *A Realist Theory of Science*. London: Routledge.
- Birner, J. (2002). *The Cambridge Controversies in Capital Theory: A Study in the Logic of Theory Development*. London: Routledge.
- Blanchard, O. (2009). The State of Macro. *Annual Review of Economics*, 1(1), 209–228.
- Blanchard, O. (2018). On the Future of Macroeconomic Models. *Oxford Review of Economic Policy*, 34(1–2), 43–54.
- Blaug, M. (1992). *The Methodology of Economics: Or, How Economists Explain*. Cambridge University Press.
- Bliss, C. (1970). Comment on Garegnani. *The Review of Economic Studies*, 37(3), 437–438.
- Bliss, C. (1975a). *Capital Theory and the Distribution of Income*. Amsterdam; New York: North-Holland Pub. Co. ; American Elsevier.
- Bliss, C. (1975b). *Capital Theory and the Distribution of Income*. Elsevier.
- Böhm-Bawerk, E. (1891). *The Positive Theory of Capital*. London; New York: Macmillan.
- Brock, W., & Mirman, L. (1972). Optimal Growth Under Uncertainty. *Journal of*

- Economic Theory*, (4), 479–513.
- Bruno, M., Burmeister, E., & Sheshinski, E. (1966). The Nature and Implications of the Reswitching of Techniques. *The Quarterly Journal of Economics*, 80(4), 526–553.
- Caldwell, B. (1981). *Beyond Positivism* (3rd ed.). London: Routledge.
- Carnap, R. (1996). The Elimination of Metaphysics Through Logical Analysis of Language. In S. Sarkar (Ed.), *Logical Empiricism at Its Peak: Schlick, Carnap, and Neurath*. New York: Garland Publishing.
- Cartwright, N. (1988). A Case Study in Realism: Why Econometrics Is Committed to Capacities. In *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* (Vol. 1988, pp. 190–197). Philosophy of Science Association.
- Cass, D. (1966). Optimum Growth in an Aggregative Model of Capital Accumulation: A Turnpike Theorem. *Econometrica*, 34, 833–850.
- Champernowne, D. (1953). The Production Function and the Theory of Capital: A Comment. *The Review of Economic Studies*, 21(2), 112–135.
- Clark, J. B. (1888). Capital and Its Earnings. *Publications of the American Economic Association*, 3(2), 9–69.
- Clark, J. B. (1891). Distribution as Determined by a Law of Rent. *The Quarterly Journal of Economics*, 5(3), 289–318.
- Clark, J. B. (1894). The Genesis of Capital. *Publications of the American Economic Association*, 9(1), 64–68.
- Clark, J. B. (1899). *The Distribution of Wealth: A Theory of Wages, Interest and Profits*. New York: Macmillan.
- Cohen, A. (1984). The Methodological Resolution of the Cambridge Controversies. *Journal of Post Keynesian Economics*, 6(4), 614–629.
- Cohen, A. (1985). Issues in the Cambridge Controversies. *Journal of Post Keynesian Economics*, 7(4), 612–615.
- Cohen, A., & Harcourt, G. (2003). Retrospectives: Whatever Happened to the Cambridge Capital Theory Controversies? *Journal of Economic Perspectives*, 17(1), 199–214.
- Colander, D., Goldberg, M., Haas, A., Juselius, K., Kirman, A., Lux, T., & Sloth, B. (2009). The Financial Crisis and the Systemic Failure of the Economics Profession. *Critical Review*, 21(2–3), 249–267.

- Colander, D., Holt, R., & Rosser, B. (2004). The Changing Face of Mainstream Economics. *Review of Political Economy*, 16(4), 485–499.
- Davis, J. B. (2012). The Change in Sraffa's Philosophical Thinking. *Cambridge Journal of Economics*, 36(6), 1341–1356.
- Dawkins, C., Srinivasan, T. N., & Whalley, J. (2001). Calibration. In J. J. Heckman & E. Learner (Eds.), *Handbook of econometrics* (Vol. 5, pp. 3653–3703). Elsevier.
- DeVroey, M. (2012). Microfoundations: a Decisive Dividing Line Between Keynesian and New Classical Macroeconomics? In P. Duarte & G. Lima (Eds.), *Microfoundations Reconsidered – The Relationship of Micro and Macroeconomics in Historical Perspective* (pp. 168–189). Cheltenham: Edward Elgar Publishing.
- DeVroey, M., & Malgrange, P. (2011). The History of Macroeconomics from Keynes's General Theory to the Present. *IRES Discussion Paper Series*.
- Dow, S. (1980). Methodological Morality in the Cambridge Controversies. *Journal of Post Keynesian Economics*, 2(3), 368–380.
- Dow, S. (2001). Post Keynesian Methodology. In R. Holt & S. Pressman (Eds.), *A New Guide to Post-Keynesian Economics* (pp. 26–35).
- Duarte, P. (2012). Not Going Away? Microfoundations in the Making of a New Consensus in Macroeconomics. In P. Duarte & G. Lima (Eds.), *Microfoundations Reconsidered – The Relationship of Micro and Macroeconomics in Historical Perspective* (pp. 190–237). Cheltenham: Edward Elgar Publishing.
- Dunn, S. P. (2000). Wither Post Keynesianism? *Journal of Post Keynesian Economics*, 22(3), 343–364.
- Eatwell, J. (1974). Controversies in the Theory of Surplus Value: Old and New. *Science & Society*, 38(3), 281–303.
- Eatwell, J. (1983). Theories of Value, Output and Employment. In J. Eatwell & M. Milgate (Eds.), *Keynes's Economics and the Theory of Value and Distribution*. Oxford University Press.
- Eatwell, J. (1990). Walras's Theory of Capital. In J. Eatwell, M. Milgate, & P. Newman (Eds.), *The New Palgrave: Capital Theory* (pp. 247–256). London: Palgrave Macmillan UK.
- Faulkner, P., Lawson, C., & Runde, J. (2010). Theorising Technology. *Cambridge Journal of Economics*, 34(1), 1–16.

- Faulkner, P., & Runde, J. (2013). Technological Objects, Social Positions, and the Transformational Model of Social Activity. *MIS Quarterly*, 37(3), 803–818.
- Felipe, J., & Fisher, F. (2003). Aggregation in Production Functions: What Applied Economists should Know. *Metroeconomica*, 54(2–3), 208–262.
- Felipe, J., & McCombie, J. (2014). The Aggregate Production Function: ‘Not Even Wrong.’ *Review of Political Economy*, 26(1), 60–84.
- Ferguson, C. (1969). *The neoclassical theory of production and distribution*. Cambridge: Cambridge University Press.
- Ferguson, C., & Allen, R. (1970). Factor Prices, Commodity Prices, And Switches of Technique. *Economic Inquiry*, 8(2), 95–109.
- Ferlito, C. (2018). For a New Capital Theory: A Hermeneutical Approach. *StoriaLibera. Rivista Di Scienze Storiche e Sociali*, 4(7), 11–61.
- Fischer, S. (1977). Long-Term Contracts, Rational Expectations, and the Optimal Money Supply Rule. *Journal of Political Economy*, 85(1), 191–205.
- Fisher, I. (1906). *The Nature of Capital and Income*. New York: Macmillan.
- Fisher, I. (1907). *The Rate of Interest*. New York: Macmillan.
- Fisher, I. (1930). *The Theory of Interest*. New York: Macmillan.
- Fleetwood, S. (2001a). Causal laws, Functional Relations and Tendencies. *Review of Political Economy*, 13(2), 201–220.
- Fleetwood, S. (2001b). What Kind of Theory is Marx’s Labour Theory of Value? A Critical Realist Inquiry. *Capital & Class*, 25(1), 41–77.
- Fratini, S. M. (2019). On the Second Stage of the Cambridge capital Controversies. *Journal of Economic Surveys*, 33(4), 1073–1093.
- Friedman, M. (1953). The Methodology of Positive Economics. In *Essays in Positive Economics* (pp. 1–43). Chicago: Chicago University Press.
- Friedman, M. (1968). The Role of Monetary Policy. *The American Economic Review*, 58(1), 1–17.
- Fullbrook, E. (2009). *Ontology and Economics: Tony Lawson and his Critics*. Routledge advances in heterodox economics. London; New York: Routledge.
- Garegnani, P. (1970). Heterogeneous Capital, the Production Function and the Theory of Distribution. *The Review of Economic Studies*, 37(3), 407–436.
- Garegnani, P. (1989). Some Notes on Capital, Expectations and the Analysis of

- Changes. In *Joan Robinson and Modern Economic Theory* (pp. 344–367). London: Palgrave Macmillan UK.
- Garegnani, P. (1990). Quantity of Capital. In J. Eatwell, M. Milgate, & P. Newman (Eds.), *The New Palgrave: Capital Theory* (pp. 1–78). London: Macmillan.
- Garegnani, P. (2000). Savings, Investment and Capital in a System of General Intertemporal Equilibrium. In H. D. Kurz (Ed.), *Critical Essays on Piero Sraffa's Legacy in Economics* (pp. 392–445). Cambridge: Cambridge University Press.
- Garegnani, P. (2012). On the Present State of the Capital Controversy. *Cambridge Journal of Economics*, 36(6), 1417–1432.
- Goodfriend, M., & King, R. G. (1997). The New Neoclassical Synthesis and the Role of Monetary Policy. *NBER Macroeconomics Annual*, 12, 231–295.
- Gordon, R. J. (1990). What Is New-Keynesian Economics? *Journal of Economic Literature*, 28(3), 1115–1171.
- Graça Moura, M. (2002). Metatheory as the Key to Understanding: Schumpeter after Shionoya. *Cambridge Journal of Economics*, 26(6), 805–821.
- Graça Moura, M. (2003). Schumpeter on the Integration of Theory and History. *The European Journal of the History of Economic Thought*, 10(2), 279–301.
- Graça Moura, M. (2015). Schumpeter's Conceptions of Process and Order. *Cambridge Journal of Economics*, 39(4), 1129–1148.
- Graça Moura, M. (2017). Schumpeter and the Meanings of Rationality. *Journal of Evolutionary Economics*, 27(1), 115–138.
- Gram, H., & Walsh, V. (1983). Joan Robinson's Economics in Retrospect. *Journal of Economic Literature*, 21(2), 518–550.
- Greenwood, J. (1994). *Modern Business Cycle Analysis* (Working Paper No. 520). Rochester Center for Economic Research.
- Gregory, A., & Smith, G. (1990). Calibration as Estimation. *Econometric Reviews*, 9(1), 57–89.
- Gregory, A., & Smith, G. (1991). Calibration as Testing: Inference in Simulated Macroeconomic Models. *Journal of Business & Economic Statistics*, 9(3), 297–303.
- Gregory, A., & Smith, G. (1993). Statistical Aspects of Calibration in Macroeconomics. In G. Maddala, C. Rao, & H. Vinod (Eds.), *Handbook of Statistics* (pp. 703–719).

- Elsevier.
- Grüne-Yanoff, T., & Mäki, U. (2014). Introduction: Interdisciplinary Model Exchanges. *Studies in History and Philosophy of Science*, 48, 52–49.
- Hahn, F. (1975). Revival of Political Economy: The Wrong Issues and the Wrong Argument. *Economic Record*, 51(3), 360–364.
- Hahn, F. (1982). The Neo-Ricardians. *Cambridge Journal of Economics*, 6(4), 353-374.
- Haldane, A., & Turrell, A. (2018). An Interdisciplinary Model for Macroeconomics. *Oxford Review of Economic Policy*, 34(1–2), 219–251.
- Hall, R. (1976). *Notes on the Current State of Empirical Macroeconomics*. Hoover Institution, Stanford University.
- Hamouda, O., & Harcourt, G. (1988). Post Keynesianism: From Criticism To Coherence? *Bulletin of Economic Research*, 40(1), 1–33.
- Hansen, L., & Heckman, J. (1996). The Empirical Foundations of Calibration. *Journal of Economic Perspectives*, 10(1), 87–104.
- Harcourt, G. (1969). Some Cambridge Controversies in the Theory of Capital. *Journal of Economic Literature*, 7(2), 369–405.
- Harcourt, G. (1972). *Some Cambridge Controversies in the Theory of Capital*. London; New York; Melbourne: Cambridge University Press.
- Harcourt, G., & Hamouda, O. (1989). Post-Keynesianism: from Criticism to Coherence? In *New Directions in Post-Keynesian Economics* (pp. 1–34). Aldershot: Edward Elgar.
- Hayek, F. (1942). Scientism and the Study of Society. Part I. *Economica*, 9(35), 267–291.
- Hellwig, C. (2017). Monetary Business Cycles (Imperfect Information). In *The New Palgrave Dictionary of Economics* (pp. 1–7). London: Palgrave Macmillan UK.
- Hennings, K. H. (1990). Capital as a Factor of Production. In J. Eatwell, M. Milgate, & P. Newman (Eds.), *The New Palgrave: Capital theory* (pp. 108–122). New York: Macmillan.
- Hicks, J. (1937). Mr. Keynes and the “Classics”; A Suggested Interpretation. *Econometrica*, 5(2), 147–159.
- Hicks, J. (1939). *Value and Capital* (2nd ed.). London: Oxford University Press.
- Hicks, J. (1965). *Capital and Growth*. Oxford University Press.

- Hobson, J. (1891). The Law of the Three Rents. *The Quarterly Journal of Economics*, 5(3), 263.
- Hodge, D. (2007). Economics, Realism and Reality: a Comparison of Mäki and Lawson. *Cambridge Journal of Economics*, 2(32), 163–202.
- Hylton, P. (2004). Quine on Reference and Ontology. In R. Gibson (Ed.), *The Cambridge Companion to Quine* (pp. 115–150). New York: Cambridge University Press.
- Inwagen, P. (1998). The Nature of Metaphysics. In C. Macdonald & S. Laurence (Eds.), *Contemporary Readings in the Foundations of Metaphysics* (pp. 11–21). Blackwell.
- Isaacson, D. (2004). Quine and Logical Positivism. In R. Gibson (Ed.), *The Cambridge Companion to Quine* (pp. 214–269). New York: Cambridge University Press.
- Jorgenson, D. (1963). Capital Theory and Investment Behavior. *The American Economic Review*, 53(2), 247–259.
- Kim, K., & Pagan, A. (1995). The Econometric Analysis of Calibrated Macroeconomic Models. In H. Pesaran & M. Wickens (Eds.), *Handbook of Applied Econometrics in Macroeconomics* (pp. 356–390). London: Blackwell Press.
- Knight, F. (1933). Capitalistic Production, Time and the Rate of Return. In *Economic essays in honour of Gustav Cassel* (pp. 327–342). London: George Allen & Unwin.
- Knight, F. (1934). Capital, Time, and the Interest Rate. *Economica*, 1(2), 257–286.
- Knight, F. (1936a). The Quantity of Capital and the Rate of Interest: I. *Journal of Political Economy*, 44(4), 433–463.
- Knight, F. (1936b). The Quantity of Capital and the Rate of Interest: II. *Journal of Political Economy*, 44(5), 612–642.
- Knight, F. (1936c). The Quantity of Capital and the Rate of Interest: II. *Journal of Political Economy*, 44(5), 612–642.
- Koopmans, T. (1963). On the Concept of Optimal Economic Growth. In *The Econometric Approach to Development Planning*. Amsterdam: North Holland.
- Krugman, P. (2018). Good Enough for Government Work? Macroeconomics Since the Crisis. *Oxford Review of Economic Policy*, 34(1–2), 156–168.
- Kuhn, T. (1932). *The Structure of Scientific Revolutions* (2nd ed.). Chicago University Press.

- Kydland, F. (2005). Autobiography. In T. Frängsmyr (Ed.), *Les Prix Nobel: The Nobel Prizes 2004*.
- Kydland, F., & Prescott, E. (1982). Time to Build and Aggregate Fluctuations. *Econometrica*, 50(6), 1345–1370.
- Kydland, F., & Prescott, E. (1991). The Econometrics of the General Equilibrium Approach to Business Cycles. *The Scandinavian Journal of Economics*, 93(2), 161. <https://doi.org/10.2307/3440324>
- Lachmann, L. (1956). *Capital and its Structure*. Kansas city: Sheed Andrews and McMell, inc.
- Lakatos, I. (1976). Falsification and the Methodology of Scientific Research Programmes. In S. Harding (Ed.), *Can Theories be Refuted?* (Vol. 81, pp. 205–259). Springer Netherlands.
- Latsis, J. (2007). Quine and the Ontological Turn in Economics. In Clive Lawson, J. Latsis, & N. Martins (Eds.), *Contributions to Social Ontology* (pp. 15–127). Routledge.
- Lavoie, D. (1991). *Economics and Hermeneutics*. (D. Lavoie, Ed.). Routledge.
- Lavoie, M. (2013). Sraffians, other Post-Keynesians, and the Controversy over Centres of Gravitation. In *Sraffa and the Reconstruction of Economic Theory: Volume Three* (pp. 34–54). Palgrave Macmillan UK.
- Lawson, T. (1994). A Realist Theory for Economics. In Roger Backhouse (Ed.), *New Directions in Economic Methodology* (pp. 257–286). Routledge.
- Lawson, T. (1997). *Economics and Reality*. London; New York: Routledge.
- Lawson, T. (2003). *Reorienting Economics*. London: Routledge.
- Lawson, T. (2006). The Nature of Heterodox Economics. *Cambridge Journal of Economics*, 30(4), 483–505.
- Lawson, T. (2012a). Mathematical Modelling and Ideology in the Economics Academy: Competing Explanations of the Failings of the Modern Discipline? *Economic Thought*, 1(1), 3–22.
- Lawson, T. (2012b). Ontology and the Study of Social Reality: Emergence, Organisation, Community, power, Social Relations, Corporations, Artefacts and Money. *Cambridge Journal of Economics*, 36(2), 345–385.
- Lawson, T. (2013). What Is this “School” Called Neoclassical Economics? *Cambridge*

- Journal of Economics*, 37(5), 947–983.
- Lawson, T. (2015a). A Conception of Social Ontology. In S. Pratten (Ed.), *Social Ontology and Modern Economics* (pp. 19–52). Routledge.
- Lawson, T. (2015b). *Essays on: The Nature and State of Modern Economics*. Routledge.
- Lawson, T. (2015c). The Nature of Gender. In S. Pratten (Ed.), *Social Ontology and Modern Economics* (pp. 485–506). Routledge.
- Lawson, T. (2015d). The Nature of the Firm and Peculiarities of the Corporation. *Cambridge Journal of Economics*, 39(1), 1–32.
- Lawson, T. (2015e). The Nature of the Firm and Peculiarities of the Corporation. *Cambridge Journal of Economics*, 39(1), 1–32.
- Lawson, T. (2015f). What is an Institution? In S. Pratten (Ed.), *Social Ontology and Modern Economics* (pp. 553–577). Routledge.
- Lawson, T. (2016). Social Positioning and the Nature of Money. *Cambridge Journal of Economics*, 40(4), 961–996.
- Lawson, T. (2018). The Constitution and Nature of Money. *Cambridge Journal of Economics*, 42(3), 851–873.
- Lazzarini, A. (2015). Some Unsettled Issues in a Second Phase of the Cambridge-Cambridge Controversy. *Review of Radical Political Economics*, 47(2), 256–273.
- Lee, F. (2009). *A History of Heterodox Economics: Challenging the Mainstream in the Twentieth Century*. Routledge.
- Levhari, D. (1965). A Nonsubstitution Theorem and Switching of Techniques. *The Quarterly Journal of Economics*, 79(1), 98.
- Lewis, P., & Runde, J. (2007). Subjectivism, Social Structure and the Possibility of Socio-Economic Order: The Case of Ludwig Lachmann. *Journal of Economic Behavior and Organization*, 62(2), 167–186.
- Long, J. B., & Plosser, C. I. (1983). Real Business Cycles. *Journal of Political Economy*, 91(1), 39–69.
- Lourenço, D., & Graça Moura, M. (2018). The Economic Problem of a Community: Ontological Reflections Inspired by the Socialist Calculation Debate. *Cambridge Journal of Economics*, 42(1), 1–17.
- Lucas, R. (1972). Expectations and the Neutrality of Money. *Journal of Economic Theory*, 4(2), 103–124.

- Lucas, R. (1976). Econometric Policy Evaluation: A Critique. In *Carnegie-Rochester Conference Series on Public Policy* (Vol. 1, pp. 19–46). Elsevier.
- Lucas, R. (2001). *Professional Memoir*. Retrieved from <http://coin.wne.uw.edu.pl/rkruszewski/memoir.pdf>
- Lucas, R., & Prescott, E. (1971). Investment Under Uncertainty. *Econometrica*, 39(5), 659–681.
- Lucas, R., & Prescott, E. (1974). Equilibrium Search and Unemployment. *Journal of Economic Theory*, 7(2), 188–209.
- Lucas, R., & Sargent, T. (1979). After Keynesian Macroeconomics. *Federal Reserve Bank of Minneapolis Quarterly Review*, 3(2), 1–16.
- Lucas, R., & Sargent, T. (1981). *Rational Expectations and Econometric Practice*. Minneapolis: University of Minnesota Press.
- Mäki, U., & Caldwell, B. (1992). The Market as an Isolated Causal Process: A Metaphysical Ground for Realism. In B. Caldwell & S. Boehm (Eds.), *Austrian Economics: Tensions and New Directions* (pp. 35–65). Dordrecht: Springer.
- Mäki, U. (1990a). Mengerian Economics in Realist Perspective. In B. Caldwell (Ed.), *In Carl Menger and His Legacy in Economics* (pp. 289–310). History of Political Economy.
- Mäki, U. (1990b). Scientific Realism and Austrian Explanation. *Review of Political Economy*, 2(3), 310–344.
- Mäki, U. (1996). Scientific Realism and Some Peculiarities of Economics. In R. Cohen, R. Hilpinen, & R. Qiu (Eds.), *Realism and anti-realism in the philosophy of science* (pp. 427–447). Springer.
- Mäki, U. (1998a). Aspects of Realism About Economics. *Theoria: An International Journal for Theory, History and Foundations of Science*, 13(2), 301–319.
- Mäki, U. (1998b). Realism. In J. Davis, U. Mäki, & W. Hands (Eds.), *The Handbook of Economic Methodology* (pp. 404–409). Edward Elgar Publishing.
- Mäki, U. (2001a). Economic Ontology: What? Why? How? In Uskali Mäki (Ed.), *The Economic World View: Studies in the Ontology of Economics* (pp. 3–14). Cambridge University Press.
- Mäki, U. (2001b). The Way the World Works (www): Towards an Ontology of Theory Choice. In U Mäki (Ed.), *The Economic World View: Studies in the Ontology of*

- Economics* (pp. 369–389).
- Mäki, U. (2002). Some Nonreasons for Nonrealism about Economics. In U Mäki (Ed.), *Fact and Fiction in Economics: Models, Realism and Social Construction* (pp. 90–106). Cambridge University Press.
- Mäki, U. (2005). Models are Experiments, Experiments are Models. *Journal of Economic Methodology*, 12(2), 303–315.
- Mäki, U. (2009a). Economics Imperialism: Concept and Constraints. *Philosophy of the Social Sciences*, 39(3), 351–380.
- Mäki, U. (2009b). Missing the World. Models as Isolations and Credible Surrogate Systems. *Erkenntnis*, 70(1), 29–43.
- Mäki, U. (2009c). Unrealistic Assumptions and Unnecessary Confusions: Rereading and Rewriting F53 as a Realist Statement. In Uskali Mäki (Ed.), *The Methodology of Positive Economics Reflections on the Milton Friedman Legacy* (pp. 90–116). Cambridge University Press.
- Mäki, U. (2011). Scientific Realism as a Challenge to Economics (and Vice Versa). *Journal of Economic Methodology*, 18(01), 1–12.
- Mäki, U. (2013). Scientific Imperialism: Difficulties in Definition, Identification, and Assessment. *International Studies in the Philosophy of Science*, 27(3), 325–339.
- Mäki, U. (2016). Philosophy of Interdisciplinarity. What? Why? How? *European Journal for Philosophy of Science*, 6(3), 327–342.
- Malinvaud, E. (1953). Capital Accumulation and Efficient Allocation of Resources. *Econometrica*, 21(2), 233–268.
- Mankiw, N., & Reis, R. (2018). Friedman’s Presidential Address in the Evolution of Macroeconomic Thought. *Journal of Economic Perspectives*, 32(1), 81–96.
- Marcuzzo, M., & Rosselli, A. (2011). Sraffa and his Arguments Sgainst “Marginism.” *Cambridge Journal of Economics*, 35(1), 219–231.
- Martins, N. (2013). Sraffa, Marshall and the Principle of Continuity. *Cambridge Journal of Economics*, 37(2), 443–462.
- Martins, N. (2014). *The Cambridge Revival of Political Economy*. New York: Routledge.
- Mata, T. (2004). Constructing Identity: The Post Keynesians and the Capital Controversies. *Journal of the History of Economic Thought*, 26(2), 241–259.

- McKenzie, L. (1954). On Equilibrium in Graham's Model of World Trade and Other Competitive Systems. *Econometrica*, 22(2), 147.
- Michael, M. (2008). Implicit Ontological Commitment. *Philosophical Studies*, 141(1), 43–61.
- Milgate, M., Eatwell, J., & Newman, P. (1991). Neo Ricardianism. In *The New Palgrave: A Dictionary of Economics*.
- Mises, L. v. (1949). *Human Action: A Treatise on Economics*. Martino Fine Books.
- Modigliani, F., & Miller, M. (1958). The Cost of Capital: Corporation Finance and the Theory of Investment. *American Economic Review*.
- Montes, L. (2003a). Smith and Newton: Some Methodological Issues Concerning General Economic Equilibrium Theory. *Cambridge Journal of Economics*, 27(5), 723–747.
- Montes, L. (2003b). Smith and Newton: Some Methodological Issues Concerning General Economic Equilibrium Theory. *Cambridge Journal of Economics*, 27, 723–747.
- Muth, J. (1961). Rational Expectations and the Theory of Price Movements. *Econometrica*, 29(3), 315–335.
- Pagan, A. (1994). Introduction Calibration and Econometric Research: an Overview. *Journal of Applied Econometrics*, 9(S), 1–10.
- Pasinetti, L. (1966). Changes in the Rate of Profit and Switches of Techniques. *The Quarterly Journal of Economics*, 80(4), 503.
- Pasinetti, L., Fisher, F., Felipe, J., McCombie, J., & Greenfield, R. (2003). Cambridge Capital Controversies. *The Journal of Economic Perspectives*, 17(4), 227–232.
- Pasinetti, L., & Scazzieri, R. (1990). Capital Theory: Paradoxes. In *The New Palgrave: Capital Theory* (pp. 136–147). London: Palgrave Macmillan UK.
- Peirce, C. S. (1955). *Philosophical Writings of Peirce*. (J. Buchler, Ed.). New York: Dover Publications.
- Petri, F. (1978). The Difference Between Long-Period and Short-Period General Equilibrium and the Capital Theory Controversy. *Australian Economic Papers*, 17(31), 246–260.
- Petri, F. (2004). *General Equilibrium, Capital and Macroeconomics: A Key to Recent Controversies in Equilibrium Theory*. Cheltenham, UK; Northampton, USA:

- Edward Elgar.
- Phelps, E., & Taylor, J. (1977). Stabilizing Powers of Monetary Policy under Rational Expectations. *Journal of Political Economy*, 85(1), 163–190.
- Plosser, C. (1989). Understanding Real Business Cycles. *Journal of Economic Perspectives*, 3(3), 51–77.
- Popper, K. (1959). *The Logic of Scientific Discovery*. London: Routledge.
- Pratten, S. (1996). The “Closure” Assumption as a First Step: Neo-Ricardian Economics and Post-Keynesianism. *Review of Social Economy*, 54(4), 423–443.
- Pratten, S. (2007). Ontological Theorising and the Assumptions Issue in Economics. In C. Lawson, J. Latsis, & N. Martins (Eds.), *Contributions to Social Ontology* (pp. 50–67). London: Routledge.
- Pratten, S. (2014). The Scope of Ontological Theorising. In S. Pratten (Ed.), *Social Ontology and Modern Economics* (pp. 68–94). Routledge.
- Prescott, E. (1988). Robert M. Solow’s Neoclassical Growth Model: An Influential Contribution to Economics. *The Scandinavian Journal of Economics*, 90(1), 7–12.
- Quah, D. (1995). Business Cycle Empirics: Calibration and Estimation. *The Economic Journal*, 105(433), 1594–1596.
- Quine, W. van O. (1948). On What There Is. *Review of Metaphysics*, 2(5), 21–36. Reprinted in Quine (1980).
- Quine, W. van O. (1951). Two Dogmas of Empiricism. *The Philosophical Review*, 60(1), 20–43. Reprinted in Quine (1980).
- Quine, W. van O. (1980). *From a Logical Point of View: 9 Logico-Philosophical Essays* (3rd ed.). Harvard University Press.
- Ramsey, F. P. (1928). A Mathematical Theory of Saving. *The Economic Journal*, 38(152), 543–559.
- Rayo, A. (2007). Ontological Commitment. *Philosophy Compass*, 2(3), 428–444.
- Rebelo, S. (2005). Real Business Cycle Models: Past, Present and Future. *Scandinavian Journal of Economics*, 107(2), 217–238.
- Robinson, J. (1953). The Production Function and the Theory of Capital. *Review of Economic Studies*, 21(2), 81–106.
- Robinson, J. (1956). *The Accumulation of Capital*. Palgrave Macmillan UK.
- Robinson, J. (1974). History versus Equilibrium. *Thames Papers in Political Economy*.

- Robinson, J. (1980). *What are the Questions? And Other Essays: Further Contributions to Modern Economics*. New York: M. E. Sharp.
- Romer, P. (2015). Mathiness in the Theory of Economic Growth. *American Economic Review*, 105(5), 89–93.
- Roncaglia, A. (1991). The Sraffian Schools. *Review of Political Economy*, 3(2), 187–219.
- Roncaglia, A. (1995). On the Compatibility between Keynes's and Sraffa's Viewpoints on Output Levels. In *Income and Employment in Theory and Practice* (pp. 111–125). Palgrave Macmillan UK.
- Rosselli, A., & Trabucchi, P. (2019). Sraffa, the 'Marginal' Method and Change. *Structural Change and Economic Dynamics*, 51, 334–340.
- Salmon, N. (1987). Existence. *Philosophical Perspectives*, 1, 49–108.
- Samuelson, P. (1962). Parable and Realism in Capital Theory: the Surrogate Production Function. *The Review of Economic Studies*, 29(3), 193–206.
- Samuelson, P. (1966). A Summing Up. *The Quarterly Journal of Economics*, 80(4), 568–583.
- Samuelson, P. (2000). Revisionist Findings on Sraffa. In H. Kurz (Ed.), *Critical Essays on Piero Sraffa's Legacy in Economics* (pp. 25–45). New York: Cambridge University Press.
- Sargent, T. (1976). A Classical Macroeconometric Model for the United States. *Journal of Political Economy*, 84(2), 207–237. <https://doi.org/10.1086/260429>
- Sargent, T., & Sims, C. (1977). Business Cycle Modeling Without Pretending to Have Too Much a Priori Economic Theory. In *New Methods in Business Cycle Research: Proceedings From a Conference* (pp. 45–109). Federal Reserve Bank of Minneapolis.
- Schumpeter, J. (1949). Science and Ideology. *American Economic Review*, (39), Reprinted in: Clemence RV (ed) (1989), pp 272–286.
- Schumpeter, J. (1954). *History of Economic Analysis*. New York: Oxford University Press.
- Sen, A. (1974). On Some Debates in Capital Theory. *Economica*, 41(163), 328–335.
- Sen, A. (2003). Sraffa, Wittgenstein, and Gramsci. *Journal of Economic Literature*, 41(4), 1240–1255.

- Shackle, G. (1991). *Epistemics and Economics: A Critique of Economic Doctrines*. Taylor and Francis Inc.
- Soames, S. (2014). *The Analytic Tradition in Philosophy, Volume 1: The Founding Giants*. Princeton University Press.
- Solow, R. (1955). The Production Function and the Theory of Capital. *The Review of Economic Studies*, 23(2), 101–108.
- Solow, R. (1956). A Contribution to the Theory of Economic Growth. *The Quarterly Journal of Economics*, 70(1), 65–94.
- Solow, R. (1957). Technical Change and the Aggregate Production Function. *The Review of Economics and Statistics*, 39(3), 312.
- Solow, R. (1963). *Capital Theory and the Rate of Return*. Amsterdam: North-Holland.
- Solow, R. (1988). Growth Theory and After. *The American Economic Review*, 78(3), 307–317.
- Solowremove, R. (1956). A Contribution to the Theory of Economic Growth. *The Quarterly Journal of Economics*.
- Soros, G. (2008). *The New Paradigm for Financial Markets: the Credit Crisis of 2008 and What it Means*. London: PublicAffairs.
- Sraffa, P. (1926). The Laws of Returns under Competitive Conditions. *The Economic Journal*, 36(144), 535–550.
- Sraffa, P. (1932). Dr. Hayek on Money and Capital. *The Economic Journal*, 42(165), 42–53.
- Sraffa, P. (1960). *Production of Commodities by Means of Commodities: Prelude to a Critique of Economic Theory*. Cambridge University Press.
- Sraffa, P. (1962). Production of Commodities: A Comment. *The Economic Journal*, 72(286), 477–479.
- Stadler, G. (1994). Real Business Cycles. *Journal of Economic Literature*, 32(4), 1750–1783.
- Stigler, G. (1941). *Production and Distribution Theories*. New York: The Macmillan Company.
- Stiglitz, J. (1974). The Cambridge-Cambridge Controversy in the Theory of Capital; A View from New Haven: A Review Article.
- Swan, T. (1956). Economic Growth and Capital Accumulation. *Economic Record*,

32(2), 334–361.

- Tyfield, D. (2007). Tracking Down the Transcendental Argument and the Synthetic A Priori: Chasing Fairies or Serious Ontological Business. In Clive Lawson, J. Latsis, & N. Martins (Eds.), *Contributions to Social Ontology* (pp. 142–159). Routledge.
- Vines, D., & Wills, S. (2018). The Rebuilding Macroeconomic Theory Project: An Analytical Assessment. *Oxford Review of Economic Policy*, 34(1–2), 1–42.
- Vromen, J. (2001). Ontological Commitments of Evolutionary Economics. In Uskali Mäki (Ed.), *The Economic World View: Studies in the Ontology of Economics* (pp. 189–224).
- Walras, L. (1874). *Elements of Pure Economics*. London; New York: Routledge.
- Weintraub, E. (1999). How Should We Write the History of Twentieth-Century Economics? *Oxford Review of Economic Policy*, 15(4), 139–152.
- Weintraub, R. E. (1989). Methodology Doesn't Matter, but the History of Thought Might. *The Scandinavian Journal of Economics*, 91(2), 477–493.
- Wickens, M. (2008). *Macroeconomic Theory A Dynamic General Equilibrium Approach*. Princeton University Press.
- Woodford, M. (2009). Convergence in Macroeconomics: Elements of the New Synthesis. *American Economic Journal: Macroeconomics*, 1(1), 267–279.
- Wright, R. (2018). On the Future of Macroeconomics: A New Monetarist Perspective. *Oxford Review of Economic Policy*, 34(1–2), 107–131.