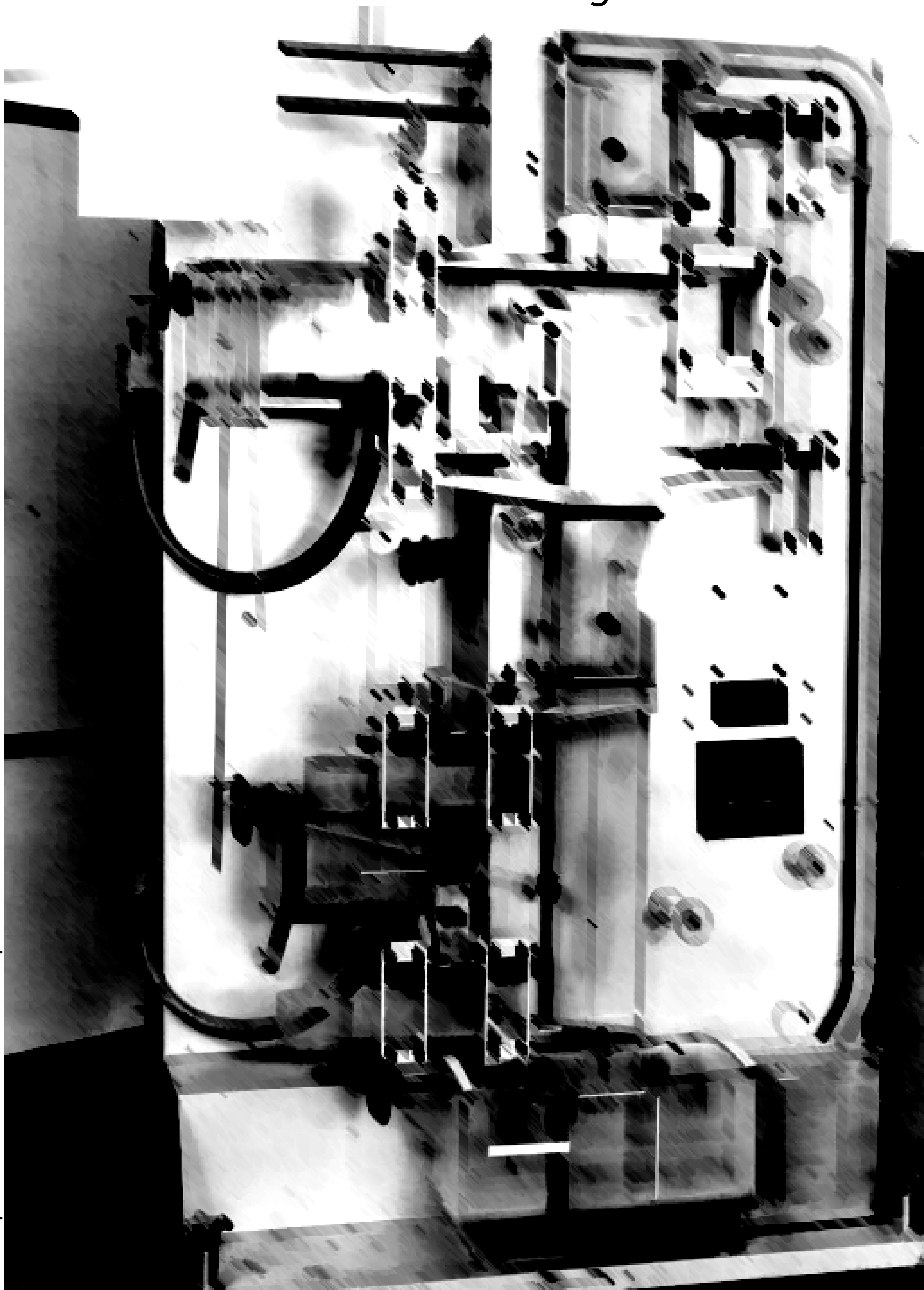
A defence of the economics profession after the crisis: a discussion on the use of models in economics.

Melissa Vergara Fernández



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A DEFENCE OF THE ECONOMICS PROFESSION AFTER THE CRISIS: A DISCUSSION ON THE USE OF MODELS IN ECONOMICS.

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One of my motives for writing *Against Method* was to free people from the tyranny of philosophical obfuscators and abstract concepts such as "truth", "reality" or "objectivity", which narrow people's vision and ways of being in the world.

—PAUL FEYERABEND

We should not be misled into thinking that the most realistic model will serve all purposes best.

—NANCY CARTWRIGHT

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wiping my tears when I would shed them. Thank him for giving me the rest that I could only have when he was lying by my side. Thank him for his love.

INTRODUCTION

The initial idea behind the writing of this thesis started as a rather different question than the one being here finally addressed. I was first interested in trying to understand what were the actual causes of the recent economic crisis in order to explore whether there was some failure in the economic models used, particularly in macroeconomic policy, that could have contributed to the crisis. Given that the causes of the crisis so far analysed are of different nature (Davies, 2010), I was interested in those that could be considered methodological. Then, the idea was to make a thorough examination of at least some of these failures to determine possible solutions.

The reason why I didn't continue with this idea is twofold: Insofar as I was not acquainted with the actual models used for macroeconomic policy, I had to rely on secondary literature that would at least point me to the models used and to the possible failures of these models. The required level of mathematics and experience in economic modelling suggested that I would only be able to analyse one or two models at most, inhibiting me from making an accurate appraisal of the use of economic models in a more general way. Thus, a plausible inference about what could have been the failures in the models, if there were such, did not seem possible. The project was perhaps too ambitious. More importantly, although most of the literature that I used as a first approach to get an understanding of the crisis coincided in that there had been an imminent failure of the economic discipline with regard to economic models, they failed to make a precise trace of how exactly the models could be considered to have failed. In other words, although it was clear that the way the models were used were regarded to certain extent responsible for the crisis, it was very difficult to see how exactly it was that they had failed and why this alleged failure translated into a possible cause of the crisis.

In consequence, my interest shifted towards the criticisms being made. I was curious that in my attempt as a philosopher of economics to understand how some models in economics had failed, I could not find a convincing discussion about how this was so. I could find several discussions in which it was argued that economic models had failed, but hardly one in which it was explained why such failure was considered such and how this affected the performance of the models. For instance, John Cassidy (2009) argues in his book how certain ideals, which he calls 'utopian economics', have prevailed and shaped the current economic thinking. These ideals, which are about the merits of free markets and free competition, obscured the inherent dangers and the possibility of market failures, like externalities. Thus, highly idealised models were used, which in a similar fashion ignored the possibility of such market failures. Such ideals, with the highly idealised models contributed to the crisis. Although it could be argued that Cassidy is not an academic economist, his view is interesting as one that is in touch with both economic theory and the economy. Furthermore, there are academic economists who hold a similar view to Cassidy. For instance, Daron Acemoglu (2009), although he ends up defending a view of 'creative destruction' and favouring the financial innovations that played a role in the crisis for their capacity to reduce the cost of capital, has a similar view when he argues that economists held three beliefs that turned out to be inaccurate: having conquered the business cycle, that free-markets require no regulations; and that large, established companies can be trusted to monitor their own behaviour. Similarly, others like Stiglitz (2009, 2010, 2011), which will be discussed together with Colander, et al. (2009) in more detail in Chapter I[1], have argued about how this 'conservative ideology' shaped unrealistic models of perfect markets and fostered lax regulation. Colander, et al., (2009), make special emphasis on the inappropriateness of the assumptions of the models.

Thus, although many of the commentators coincide in that the economic models failed because of some inappropriateness of the assumptions in the models that contributed to the crisis, they do not establish clearly how this represents a failure. As I will discuss later, some of the assumptions discussed are regarded as problematic given their unrealisticness, but the critics fail to explain why the unrealisticness of these assumptions is a problem.

Considering that there is no clarity in the arguments and that therefore it is very difficult to establish possible solutions for the alleged failures of the models, stated briefly, in this thesis I engage in a thorough discussion of the elements that I think would have to be considered first, before an accurate appraisal of the use of models in economics can be made. These elements have been addressed in discussions in the philosophy of science with regard to the role that models play in the scientific endeavour and the use that they can be given. Although these discussions need not necessarily be the views shared by practising economists, it is important to keep such

[1] I will concentrate on Stiglitz (2011), as it is the one that addresses the methodological issues in particular.

elements at the fore of the discussion, especially when the performance of the models in a science are being criticised, as it is the case of economic models in the light of the recent financial and economic crisis. Although the discussion here presented is not meant to be prescriptive about how models should be used, it is important to consider what the philosophical discussion about how models are used, in order understand the actual possibilities that models provide in the scientific understanding. This, in turn, contributes to having a more accurate view about what models can do in economics, and the ways in which they can be used. Some of the elements that I will discuss that I consider are important to have a more accurate view about what models can do, are issues about: i) the purposes with which models are built in the first place; ii) how scientific models are used to represent and how these purposes play a prominent role; iii) the use of idealisations in scientific models; and iv) the different types of epistemic purposes that models can have.

Hence, in this thesis I argue that once the elements that I discuss are taken into account, the criticisms as posed are dwindled. At least, they would need to be qualified and stated with regard to specific models; considering their purpose and the inferences that can be made from them, instead of disqualifying the use of some assumptions as a generalised claim for any type of model, as the critics in some cases do. Thus, it should be emphasised that my claim does not mean that the use of models in economics is devoid of flaws. This defence of the economics profession is solely with regard to the criticisms that have been made, in the way in which they have been made. Thus, is important to take the criticisms seriously, for, although considering the elements that I suggest would impair the criticisms as posed, this not necessarily implies that they cannot be pointing to some weak elements in the use of models in economics. Furthermore, aside from diminishing the strength of the criticisms, the discussion in this thesis contributes to a broader understanding of how models can be used in economics, not as a prescriptive way, but rather as a *possible* way, considering especially the power—and limitations—of models in the scientific enterprise.

In chapter I, I try to clarify the criticisms as much as possible. In particular, as a result of an exploration of several criticisms by different authors, I argue that two different types of argument are used and conflated in order to disqualify some assumptions used in economic modelling, by arguing that the crisis could not be anticipated because flawed models were used. To illustrate my point, I focus on the criticisms made by Stiglitz (2011) and Colander, et al. (2009) in relation to two assumptions in particular: the representative agent and the rational expectations. One argument is that the models could not anticipate the crisis because they were not built to do such thing. The second argument is that the models could not anticipate the crisis because the two aforementioned assumptions were 'the wrong' assumptions. My claim is that if one agrees that the models were not built to consider situations like possible economic downturns, it is superfluous to blame the models for using the 'wrong' assumptions. Furthermore, not being built to consider these situations does not imply that they are not good for any other purpose for which they could have been built. The critics argue that the use of these wrong assumptions was due to the microfoundations of macroeconomics. I revise how this project came about and argue that there were specific purposes in this project that supported the use of the criticised assumptions. If there ought to be a criticism for the use of these assumptions, the purposes for which they were used need to be considered.

In chapter II I present a discussion about the syntactic and the semantic view of theories, and later one of the most recent variations of the semantic view, defended, among others, by Ronald Giere (2004, 2006). The reason why I discuss these two views about how theories have been regarded in the philosophy of science to be structured is twofold: first, because the emphasis of the critics on the realisticness of the assumptions to judge their appropriateness, can be associated with what McCloskey (1983) has called a 'modernist attitude'. Thus, I discuss briefly McCloskey and how her criticism can be associated with the emphasis on realisticness of the critics. Second, because the contrast between the syntactic view and the semantic view is important to appreciate more clearly the features of the view defended by Giere and others. Here I discuss why realisticness is not necessary to represent some aspect of the world and how the concept of similarity, together with the purposes of such representation, can be a good account of how scientific models can be used to represent. I also discuss how this representation can be achieved by the use of idealisations and false assumptions, in a related view defended by Mäki (2009, 2011).

In the last chapter, in order to make more concrete my discussion of representation in Chapter II, I discuss two models as examples. First, using a previous discussion of Hoover (2010) about a model of how nominal rigidities can cause aggregate fluctuations by Blanchard & Fischer ([1989]

1993), I try to show the role of idealisations in models. Hoover argues that the assumption of the representative agent fails as an idealisation. I argue that this depends, among other points that I discuss, on the view that one has about what idealisations are; on the phenomenon under investigation; and on the epistemic purposes that a model can have. Furthermore, Hoover argues that the use of the assumption of the representative agent together with the assumption of perfect competition, translates into the use of contradictory assumptions, which jeopardise the scientific usefulness of the model. Although I admit that this can be problematic, there are other models in the sciences that stand in the same situation, and yet are not negligible. Finally, with the use of the other example, a model by Kydland & Prescott (1982) of real business cycles, I argue that with the use of the assumptions that the critics consider despicable, a model can still be considered valuable from a scientific point of view.

CHAPTER I: 'The dismal science' and its microfoundations.

1. The failure of the economic discipline

The financial crisis that began in 2007 has provoked a massive bombardment of criticisms directed at the economics discipline. Not only journalists and the general public have invoked once again the label of "the dismal science" once stated by the historian Thomas Carlyle referring to the economic discipline, but also economists themselves have raised their voices against the dominant paradigm in financial economics and macroeconomics (Colander, 2010; Colander, et al., 2009; Hodgson, 2009, 2011; Kirman, 2010; Krugman, 2009; Lawson, 2009; Roubini & Mihm, 2010; Stiglitz, 2009, 2010, 2011; The Economist, 2009). Although some arguments have been more sophisticated than others, in general most of the criticisms amount to two different—though related—accusations: on the one hand, the critics argue that the economic models used by central banks did not possess the general characteristics necessary to make correct predictions about the economy, yielding them incapable of anticipating the risk, the credit crunch and the subsequent crisis that unfolded. In other words, the critics argue that the models ignored the possibility of the occurrence of the events that unfolded the crisis because they focussed on ideal situations like perfect markets. Therefore they were inappropriate because they were built for the wrong purposes. On the other hand, they argue that the models used were flawed because of some of the assumptions on which the models were based, yielding them completely incapable of providing good predictions about the economy. For these two reasons, the critics have argued that the crisis demonstrated the pitfalls in the models used in mainstream macroeconomics. In consequence, the economic discipline has been said to fail dramatically.

The debate that was usually held between schools of thought and the current criticisms, particularly from the detractors of the dominant paradigm towards the latter, have become popular because of the crisis. For instance, a public petition "to revitalise economics after the crash" has been signed not only by heterodox economists like Geoff Hodgson—he is the first signatory—but also by other 2327 people since September 2009 (Hodgson, 2009). Journalists, like John Cassidy, have also written about the crisis and the events that lead to it. He argues, in general terms, along the lines of the first argument mentioned above, namely that the crisis occurred because the idea that markets never fail was entrenched very deeply among those who identified themselves with the dominant paradigm. Therefore, their ideas and models were not prepared to observe that markets indeed fail and how they do so despite the evidence that suggests the contrary (Cassidy, 2009). In a similar fashion, though an economist himself and a Nobel Laureate, Paul Krugman wrote in his regular column in the New York Times, asking how economists had been able to get it so wrong:

> "Few economists saw our current crisis coming, but this predictive failure was the least of the field's problems. More important was the profession's blindness to the very possibility of catastrophic failures in a market economy. During the golden years, financial economists came to believe that markets were inherently stable—indeed, that stocks and other assets were always priced just right. There was nothing in the prevailing models suggesting the possibility of the kind of collapse that happened last year" (Krugman, 2009).

Despite the widespread criticism from both economists and non-economists pointing to the same elements that failed—and in some cases how to fix them—a clear understanding of what the failures were and how to fix them is still not conclusive. In great many cases the two different arguments mentioned, namely that models were built ignoring crucial elements and that the assumptions with which they were built yielded flawed models, have not been kept separate, making the understanding of the alleged failures more difficult, let alone how the discipline should deal with them. Conflating the two types of arguments creates misunderstandings about the nature of the alleged failures. Assuming that the criticisms have been made with the purpose of improving the insights and understandings that economics as a science is able to give us, it is important to be very precise about what it is that has failed first, in order to later provide recommendations as to how such failure can be worked out.

As it was mentioned above, the two types of arguments that have been provided by critics are first, that the models currently part of the dominant paradigm, which were used by central banks, did not possess the general characteristics necessary to make correct predictions about the economy because they only contemplated situations in which all goes well. Let us call this the 'appropriate for the wrong purposes' argument. Second, that the models were flawed because the assumptions on which they are based are wrong. Let us call this the 'wrong assumptions' argument. Despite their resemblance, these are two distinct—though related—types of argument. The 'appropriate for the wrong purposes' argument is about the inappropriateness of the models for anticipating the crisis because that was not their purpose. They only contemplated ideal situations (e.g. perfect competition, perfect information). The second one is about the flawed assumptions used in order to build the models. In other words, the first one is about the purposes of the models at hand; that they were inappropriate to anticipate crises because they were not built for that purpose in the first place. Attached to this claim is the discussion about the aims or goals of the science. At an abstract level, a discussion about whether economics—or science, in general—should aim to uncover truth. At a more concrete level, a discussion about whether economics should provide explanations or predictions about economic phenomena, or both. And at an even more concrete level, a discussion about what are the types of phenomena that economics (or in this case macroeconomics) should be dealing with: identifying when the economy is out of equilibrium; working out how to reduce unemployment, fostering growth or stabilising the economy, etc. In this case, the criticism was explicit about the two most concrete levels: on the one hand, that (macro) economics should aim to predict, and on the other, that it should be focussing on issues like crises and preventing them. Regarding the second type of argument, it is about the models themselves and the assumptions with which they are built. In great many cases the claim is related to the realisticness of the assumptions and that empirical evidence suggests that the assumptions are contrary to what happens in the real world. For example, the assumption of the representative agent is argued to be contrary to the fact that there is not such thing as an average individual and rather that individuals are heterogeneous and have heterogeneous tastes and preferences.

My claim that these two types of arguments have not been kept separate lies in the fact that both types of arguments have led to the conclusion that economic models failed, and in consequence that the discipline also failed. Although it is not difficult to understand why these two arguments have been easily conflated, for they both deal with the incapacity of the models to anticipate the crisis, the reason for such failure is different in each of the arguments. The first one is that the models failed to anticipate the crisis because they were not meant to do such thing in the first place. They were aimed for a different purpose. The second is that there are intrinsic failures in the models because the assumptions with which they are built are unrealistic or false. A thorough disentanglement of the two arguments in the criticisms is, in my opinion, vital for the correct understanding of what is actually being criticised. The danger of confusing these two arguments, and therefore the lack of clarity, is that the criticisms are very likely to remain unheard, as they have already in many cases in the past. In the following two sections I will illustrate the conflation with two examples: Stiglitz (2011) and Colander, et al. (2009).

Two different types of argument: Stiglitz

One of the fierce critics of the discipline because of the current events is Joseph Stiglitz, Nobel Laureate in Economics in 2001—jointly with George Akerlof and Michael Spence—for their analysis of markets with asymmetric information. In his paper "Rethinking Macroeconomics: what went wrong and how to fix it" (2011) Stiglitz argues, in broad terms, that the underlying economic framework on which the 'standard model'[2] is based is flawed: it is based on grossly simplifying assumptions that yield its models incapable of explaining phenomena like the crisis. The models, given the simplified assumptions, centred on 'special cases' where market inefficiencies do not arise, or on matters that are of second- or third-order importance, instead of matters of first-order importance like the fragility of financial markets. In consequence, he argues, because "[p]rediction is the test of a scientific theory; when subject to the most important test—where we really cared about the answer—the models failed, and failed miserably" (Stiglitz, 2011, p. 166). In addition, some of the insights that were ignored with such simplifying assumptions, like issues of agency, which have been placed in sound theoretic footing, have been known to economists for three quarters of a century and nevertheless neglected. The reason for which such simplifying assumptions have been made is that a particular kind of microeconomics,

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[2] Stiglitz does not give an explanation of what he means by "standard macroeconomic models", which is already problematic. I assume he is referring to the models whose use predominates in academic economics and in central banks–the mainstream. Even though I will not address the issue of the specific target of the criticisms, later in this work I do stress the need to focus on models or particular families of models, for the type of phenomena they deal with, instead of nominal labels like "saltwater" or "freshwater" economics.

one in which markets are efficient and individuals are rational was chosen when the project of providing microfoundations for macroeconomics emerged. Furthermore, the models, if not entirely culpable for the way in which the crisis was handled, they did provide succour to the ideas that dominated modern monetary policy, namely the belief that markets are efficient; that stabilising prices is necessary and almost sufficient to ensure stability and growth; and that one cannot tell a bubble until after it breaks, among others.

Put more simply, Stiglitz argues that the attempt to provide macroeconomics with a particular kind of microfoundations—one that imposed assumptions like competitive equilibrium and rational expectations—led macroeconomics to focus mainly on the special cases where market inefficiencies do not arise. In consequence, because prediction is the test for a scientific theory and these models failed so miserably in predicting the crisis, the standard model failed. Therefore, economics failed as a science. It is clear that the two different arguments mentioned above are being put forward here: the first is that research in modern macroeconomics was led away from what Stiglitz considers that are the central questions. That is, the attempt to provide microfoundations—of the simplifying kind—to macroeconomics, provoked a shift in the questions that macroeconomics should be asking. The second argument is that the models failed because they were based on too simplifying assumptions and therefore failed to predict. The first argument has to do with the discipline, with the research interests that dominated it. It does not say anything about how good or bad the models were. The second argument is epistemic: about the construction of the models and their 'scientific performance'. The problem is that they are being conflated, concluding that the economic discipline failed because the models used the wrong assumptions. Separating the two types of argument brings to the fore the fact that the second argument is only a consequence of the first one. For with the first one it is already being said that the models were not adequate to explain or predict a phenomenon like the financial crisis. If the models were found to be inadequate because they were not built for such purpose, it does follow that the assumptions were inappropriate, but solely because the models were not built for such purpose in the first place. Not because the models did not perform well or used assumptions that should not be used because they are unrealistic.

In order to illustrate my point more precisely, it is perhaps better to use more concrete examples from the text. It should be noted first, however, that Stiglitz seems to be aware that asking a different question—or using a model for a different purpose—and making wrong assumptions in the models, are two separate types of accusations that entail different types of failures. In the introduction of the paper, Stiglitz makes a distinction between the problem of the questions that the discipline should be addressing on the one hand, and on the other the inherent failures of the models because of the assumptions on which they are based. "In the discussion below, I outline the sense in which modern macroeconomics failed. There were failures in methodology, in asking the right questions, in making the right assumptions" (Stiglitz, 2011, p. 166). Furthermore, he makes explicit his idea that having selected second- or third-order importance questions—not the *right* questions—is a separate issue:

> "As I have suggested, part of the problem of modern macroeconomics is that it focussed on the wrong questions. It attempted to explain better the small and relatively unimportant fluctuations that occur 'normally', ignoring the large fluctuations that have episodically afflicted countries all over the world". (Stiglitz, 2011, p. 169).

Nevertheless, Stiglitz fails to stick to the distinction, conflating the two arguments. Therefore, he argues that the models were not adequate for the large fluctuations that have episodically afflicted all kinds of countries and at the same time argues that the models failed because they were based in poor assumptions. For example, in his section *The failure of 'economic science'* Stiglitz rejects Bernanke's claim that the crisis was more a failure of economic engineering or management, than of the science. First, Stiglitz defines 'economic science' as "the central macroeconomic models that have played key roles in the formulation of economic policy and thinking in recent years" (Stiglitz, 2011, p. 167). That is, for him 'economic science' is basically models. And then, proceeds by arguing: "But the standard macroeconomic models neither incorporated them [incentive structures prevalent in the financial sector] nor provided an explanation for why such incentive structures should become prevalent—and these failures are failures of economic science." (Stiglitz, 2011, p. 167). Why should the models be blamed for not possessing certain kinds of assumptions or for not explaining a particular phenomenon if that is not what they were built for? At most, what can be argued here is that the discipline failed in guiding research interests in a particular direction. But this has nothing to do with how sound the models are or what their performance is. As I will argue later, this can only be determined according to the particular purpose for which the model was built. And as Stiglitz has made clear, the standard model focussed on other issues, that he argues are of second- or third-order importance. But this is a completely different issue. Stiglitz does not even consider that the models could have performed well in addressing the issues that they were meant to address.

In his discussion about the *foundational approaches,* where Stiglitz talks about the representative agent assumption and the Dynamic Stochastic General Equilibrium (DSGE) models, the argument presented is similar to the example above. First, Stiglitz argues that in the ambition to provide solid microfoundations for macroeconomics, the representative agent with rational expectations assumption was adopted. "To achieve the ambition of putting macroeconomics on more solid microfoundations, many turned to what is known as the representative agent model with rational expectations and 'perfect markets' (Stiglitz, 2011, p. 168). He admits that attempting to derive macroeconomic behaviour from individual behaviour is uncontroversial. The problem for him lies in the type of microfoundations that were used. "It was important to derive macro behaviour from the *right* microfoundations—those that were consistent with actual behaviour, taking into account information asymmetries and market imperfections" (Stiglitz, 2011, p. 168). In consequence, Stiglitz claims that the model leaves out most of what needs to be explained:

> "[t]he major deficiency in the representative agent model is that there is no room for the central issues…The inability of the representative agent model to incorporate meaningful information asymmetries and financial constraints makes the model of particularly limited use in understanding shifts arising from credit fluctuations—such as the current recession" (Stiglitz, 2011, p. 168).

Up to this point, Stiglitz is making an acceptable claim: for him the models explain too little. Or at least, not much of what he considers most important. However, this does not mean that the models are wrong, as he later suggests: "As such, this model leaves out much, if not most, of what is to be explained; if that model were correct, the phenomena—the major recessions, depressions and crises that we seek to understand—would not and could not have occurred" (Stiglitz, 2011, p. 168).

Stiglitz argues in a similar way about the DSGE models. For him, these models are "to a large extent, elaborations and refinements on an approach that is basically flawed" (Stiglitz, 2011, p.

168). He doesn't tell us why the models or the approach in which they are based are flawed, which would be to point to why they do not work according to the purposes that they have been set to serve. Instead, Stiglitz although recognising that it is important to incorporate dynamics and uncertainty and to take into account general equilibrium responses, argues that, "in each of these arenas, the basic DSGE models often focussed on the wrong issues, deflecting attention from the right ones" (Stiglitz, 2011, p. 169). Again, this only shows that the DSGE models were used to focus on issues that for Stiglitz are not relevant, not that the models failed, or that need to be fixed. In order to establish the alleged failures of such models, they would have to be assessed according to the extent to which they were able to say something about the questions these models address. Some of these questions are the inefficiencies that can arise due to price distortions caused by inflation or whether wage rigidities or search costs can explain unemployment. Whether these are important or unimportant issues, or matters that macroeconomics should or should not be addressing is completely separate from whether the models performed well or not at the tasks they were meant to fulfil.

Colander, et al (2009).

Colander, et al. (2009) argue in a similar fashion as Stiglitz about the failure of the economics profession. That is, they conflate two different arguments in their criticism of the economics profession. In their paper they argue that the economics profession is responsible for the crisis in two respects: first, because the profession failed to anticipate the crisis, and second, because the profession played a role in fostering the crisis. In relation to the latter, Colander, et al. (2009) argue that researchers like financial engineers were aware of the limitations of their models but failed to warn those who were using the models about these limitations. According to Colander, et al. (2009) economists, as all scientists, have an ethical responsibility to communicate the limitations of the models they build and to warn about their potential misuse. In this case, economics researchers failed to communicate the limitations of the models to politicians and other parties using the models for public policy. In relation to the profession failing to anticipate the crisis, Colander, et al. (2009) argue that most economists focussed their attention on models that ignore fundamental elements:

> "Over the past three decades, most economists have developed and come to rely on models that disregard key factors—including the heterogeneity of decision rules, revisions of forecasting strategies and changes in the social context—that drive outcomes in asset and other markets. It is obvious, even to the casual observer, that these models fail to account for the actual evolution of the real-world economy. Moreover, the current academic agenda has largely crowded out research on the inherent causes of financial crises." (Colander, et al., 2009, p. 250).

That is, very similarly to Stiglitz (2009), Colander, et al. (2009) argue that most economists have focussed on models that are very limited in scope—that do not include 'key factors'—and that this overreliance has crowded out research in other varieties of models. Up to this point, Colander, et al. (2009) seem to be arguing solely on the grounds of the 'appropriate for the wrong purposes' argument, about the failure of the profession to focus on more pertinent issues. To this they add that those who built the models failed to provide warnings according to the limitations and the potential misuse of the models. In this respect, they are different from Stiglitz (2011), as they seem to recognise that purposes play a prominent role in the use of models. However, they do point out that the failure to anticipate and model the crisis has deep methodological roots. For them, the definition of economics provided by Robbins, where the concern of the discipline lies in the allocation of scarce resources, is shortsighted and misleading, for it reduces economics to the study of optimal decisions in well-specified choice problems. Furthermore, although the connection with the previous line of argument is not clear at all, they criticise the assumptions of rational expectations and the representative agent as flawed methodology:

> "For instance, only a sufficiently rich model of connections between firms, households, and a dispersed banking sector is likely to allow us to get a grasp on 'systemic risk', domino effects in the financial sector, and their repercussions on consumption and investment. The dominance of the extreme form of representative-agent reductionism has prevented economists from even attempting to model these important phenomena. It is this flawed methodology that is the ultimate reason for the lack of applicability of the standard macro framework to current events" (Colander, et al., 2009, p. 258).

For them, the flaw with the representative agent is that it is an extreme form of conceptual reductionism that is not common in the natural sciences. Typical reductionism is about reducing complex phenomena to the interaction of their parts, as a means to understand emergent phenomena at a higher hierarchical level (Colander, et al., 2009). By contrast, the reductionism implied in the representative agent is that the properties of the lower levels in the hierarchy are applied analogously at the higher levels. Hence, with the representative agent, the properties of an optimising individual are attributed to the entire economy. I will come back to this point later.

Regarding the rational expectations, Colander, et al. (2009) argue that findings in psychology, behavioural and experimental economics suggest a completely different behaviour on the part of individuals from the way in which the rational expectations models depict individual behaviour. In consequence, they argue that it is problematic to insist in a view of human beings that is irreconcilable with the evidence available. "What we are arguing is that as a modelling requirement, internal consistency must be complemented with *external consistency:* Economic modelling cannot be inconsistent with insights about real-world human behaviour obtained from other branches of science" (Colander, et al., 2009, p. 257; emphasis in the original).

Although Colander, et al. (2009) provide some deeper insights as to what their objections are as compared to Stiglitz (2011), there are still two elements that are worth emphasising. First, they still recognise as a failure that the research efforts of the discipline were directed to other endeavours that do not seem so valuable for them. As I pointed out earlier, acknowledging this is to acknowledge that the models were not built to give us insights about the crisis in any case, and therefore there is no reason to believe that they have flaws, at least with regard to crises. Second, although they are somewhat more specific than Stiglitz (2011) about their claims, they still fail to show how the reasons that they give for calling the mentioned assumptions flawed are problematic. In other words, they argue that the extreme form of reductionism, as opposed to the most typical type accepted in the natural sciences is problematic, but they do not tell us why this is the case. In the naturalist versus antinaturalist debate, although not solved in its entirety, it has been become clear that naturalism[3] is no longer a popular view (Martin & McIntyre, 1994). Therefore, it is not evident why in this case not adhering to the conceptual reduction of the

[3] When I use the term naturalism I am referring exclusively to a view of methodological monism, which is that the social sciences should follow the same methods used in the natural sciences.

natural sciences would be problematic. If they find it problematic, they would have to explain why it is so and how such reduction affects getting accurate economic insights. Similarly, they tell us that as a modelling requirement, internal consistency must be complemented with external consistency, meaning that models should be as realistic as possible, but they do not tell us why. Such a requirement is not so straightforward when one thinks of examples as James Watson's tin and cardboard model of DNA[4]. Of course, the cardboard in the model is not consistent externally—DNA is not made of cardboard—yet, it was sufficient for Watson to discover how adenine-thymine and cytosine-guanine pair together to form the double helix in the DNA.

An analogy

At the risk of being repetitive, I will illustrate with a trivial but clear analogy how Stiglitz (2011) and Colander, et al. (2009) are relating the economic crisis with the failure of the 'standard model'. When I first moved in to the Netherlands, one of the first things I was advised to do was to get a bicycle. I searched both in the new and the second hand market. After several days of seeing new and old bikes, I decided to get a brand new 'oma fiets' made in Holland: the classical Dutch bike for grannies. In the second hand market, the cheapest bike I could find was 80€ and it was rusted literally everywhere. I bought for 200€ this new granny bike that was very simple: no gears, no suspension and rather heavy. After all, made for grannies who live in the flatlands and cycle around in the city. That was the use I intended to give to it, anyway. Imagine that during my holidays I decide to take the bike back home, where there are mountains of all sorts of heights. Happy with my new bike at home, I decide to go with my friends to do downhill biking with it. Of course, I have a terrible accident. Downhill bicycles have a very different geometry than an 'oma fiets'; their durability and stability are their most important design features, given that they are intended mainly for high-speed descent in steep trails. Downhill bikes need to be made of aluminium or steel; have a rear and a front suspension in the fork; and among many other specifications, need a special kind of disk brakes, as opposed to the drum—coaster—brakes of my 'oma fiets'. The bike returns to the Netherlands in pieces, with the frame broken and the

[4] For a narration and explanation—by Watson himself!—of how he discovered how the bases pair to form a double helix in the DNA, see http://www.dnalc.org/view/15492-Discovering-the-double-helix-structure-of-DNA-James-Watson-video-with-3D-animation-and-narration.html.

rims bent: a complete wreck. Now imagine that I tell this story to Prof. Stiglitz and he decides to go to see the guy at the Mega Bike, where I bought my bike. Professor Stiglitz tells him that his entire stock of bicycles needs to be repaired not only because the bike is in pieces but also because I have a broken arm and bruises everywhere. Prof. Stiglitz admits that the 'Oma fiets' was not intended to do downhill cycling, that it was made to cycle around in the city. Yet, he argues that the bike I bought was of bad quality because it was very simple: it did not have gears or suspension or thicker tyres or disk breaks. To be sure, despite Prof. Stiglitz good intentions to help me, he will not get a refund and much less have the Mega Bike refurbish their bike stock.

By now, I hope to have made clear that if the economics discipline indeed selected certain questions at the expense of others, it is a problem that only relates to the institutional framework of the discipline and to the incentives that researchers have to guide their efforts in one direction or another. If this issue is indeed a problem, then a suitable treatment has to be given to it.

Colander (2010)[5] for instance, has argued for a change in the institutional structure within the economics discipline in order to overcome too much time spent on 'dotting i's and crossing t's on the DSGE model'. "Our argument was that the large majority had little incentive to study the problems [in the financial sector] because their perceived incentives were not to study a wide variety of models" (Colander, 2010, p. 420). In consequence, Colander makes three suggestions that could contribute to changing the incentives structure in the economics discipline: first, he argues for granting agencies to require publications of models to have warning labels in which possible limiting assumptions and the plausibility to use such models for policy to be openly expressed. Second, Colander suggests having a wider range of peers in the reviewing process. He argues that too much emphasis has been put into a single model particularly because peer reviewers are all part of the same intereducated small group of economists. When the pool of peer reviewers is expanded, not only with other type of economists but also peers from other disciplines, more creative research on a wider range of models is encouraged. Finally, Colander

[5] This paper is a rejoinder of the paper Colander, et al. (2009). Colander (2010) argues that in their previous paper their accusation of the economics discipline was misinterpreted as a failure of the models, while they only meant a failure in the sense that the discipline focussed too much on models like DSGE, neglecting other branches of research that could have provided some insights about the crisis. In my opinion Colander, et al. (2009) argue for much more than a simple failure of the profession as Colander (2010) argues. Some of the points of Colander, et al. (2009) will be discussed later.

suggests that training should be increased in interpreting models, rather than simply producing models, which is one of the skills taught at economics graduate programmes. He considers interpreting models and producing models two different skills, of which only the latter is important for many. Therefore, the skill of interpreting models should be emphasised, with knowledge from the field on which models should be applied, among other sources of information that can be useful to interpret models in a more cautious way.

Assessing models for their assumptions

Having to this point established that there are two different arguments in the criticisms made by economists about the economics discipline because of the current crisis, in what follows I will concentrate on the 'wrong assumptions' argument. As I already pointed out, the first argument, about the purposes that the discipline as a whole pursues, is a question that is more related to the practices and incentives that exist within the discipline. The way in which Colander (2010) addresses this issue is just a possible way out of the problem that both Stiglitz (2011) and Colander, et al. (2009) recognise as the crowding out of some research interests. By contrast, I will concentrate on the 'wrong assumptions' argument: that the models failed because of the assumptions on which they are based are unrealistic or do not correspond to evidence obtained from other branches of science. If the criticisms are to be taken as a means to contribute to the improvement of the discipline, in the sense that they can contribute to having more accurate models for the different types of phenomena that there could be concern for, the criticisms need to be taken seriously. This means that it is important to understand what it is precisely that they are referring to when they say that the models are flawed when they use certain assumptions. Furthermore, the fact that the argument has not been stated adequately does not mean that in some other sense, the critics could be right about their accusations. A second reason why it is important to understand what the critics are trying to say is that some of these have not been made for the first time as a consequence of the crisis. In great many cases it is already decades that opposition against certain assumptions in macroeconomic modelling is voiced. Kirman (1992) is one of the examples of this long-lasting criticism. Therefore, in the rest of this chapter, I will focus on trying to understand more clearly what these criticisms are about and why they have not been entirely heard.

The assumptions of the representative agent and the rational expectations

The assumptions of the representative agent and the rational expectations are problematic according to the critics. Stiglitz (2011), on the one hand, argues that the model based on the representative agent with rational expectations in perfect equilibrium leaves out most of what needs to be explained. On the other hand, Colander, et al. (2009) argue that there is an inconsistency between the assumptions and the empirical evidence provided by other branches of science. Furthermore, they criticise how the models themselves are constructed using these two assumptions:

> "many economic models are built upon the twin assumptions of 'rational expectations' and a representative agent: That is, *all* market participants are homogenised into a single agent with rational expectations, and these are defined to be fully consistent with the structure of *the economist's* own model. Since the economist's model is, of course, treated as true (which is odd given that even economists are divided in their views about the correct model of the economy) the implication is that the representative individual, hence everyone in the economy, behaves as if he had *a complete understanding of the economics mechanisms governing the world*." (p. 256, emphasis in the original).

In a similar way, Kirman (2010, p. 509) argues that there is no theoretical justification for modelling the economy with these assumptions:

> "Thus, the really basic issue is that we continue, in much macroeconomic analysis, to dismiss the aggregation problem and to treat economic aggregates as though they correspond to rational economic individuals although this is theoretically unjustified. It is this simple observation that makes the structure of the models, however sophisticated, which macroeconomists build, unacceptable."

As I mentioned before, it is not entirely clear why these assumptions are problematic in the first place. At least there are a few things that would have to be agreed upon first, or argued as to why we should endorse them, before the assumptions could become problematic on grounds of being unrealistic or not following naturalism. One of them, for instance, would be that we would have to agree that the aim of science is truth[6]. Only then, and only depending on the view of truth, unrealistic assumptions would pose a problem[7]. Thus, not being realistic—not being consistent with findings in other branches of science—or not adhering to the naturalistic methodology, pose no problems to the assumptions if one considers, for instance, the usefulness of science.

Both Stiglitz (2011) and Colander, et al. (2009) argue that these assumptions began to be made as a consequence of attempting to provide microfoundations for macroeconomics. In addition, other critics point to the 1970s as a period in which macroeconomics changed considerably from the way in which it was practised before. In particular, in the 1970s, the consensus that existed, which established that Keynesian macroeconomics explained the economy before it achieved full capacity and classical theory as the one describing it once the equilibrium had been achieved, was broken. From then on, as I will report later, to many economists Keynesian economics was considered something of the past. The result was that to many, having macroeconomics founded on microeconomic principles became of paramount importance. Therefore, it is important to understand the role that the assumptions played in the attempt to provide microfoundations for macroeconomics. In some sense, it seems as though the whole project of micro founding macroeconomics is something the critics consider despicable. For example, Stiglitz (2011) claims that "[t]he quest begun some 40 years ago to put macroeconomics on more solid microfoundations has, in short, proved quixotic" (p. 168). Nevertheless, they both suggest that providing microfoundations to macroeconomics is not the real problem. Stiglitz, for instance, argues that it is the type of microeconomics that were chosen to give foundations to the macroeconomics.

> "The standard paradigm takes as its methodological foundation that macroeconomic behaviour has to be derivable form underlying microeconomic foundations. That proposition *seems* on the face of it uncontroversial. Yet, in carrying out that research

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[6] Even if it were agreed that the aim of science is truth, a whole new discussion about what is meant by truth would emerge, for some would favour, for instance, a correspondence theory of truth, which states that "a belief is true if there *exists* an appropriate entity—a fact—to which it corresponds" (Glanzberg, 2009), while others could favour a Habermasian consensus theory of truth, in which truth is considered as an ideal situation, as a commitment to anticipated consensus, which impels scientists to abandon falsified positions and schemes that do not lead to consensus and that entails accountability of the statements made. (Hesse, 1978)

[7] And this is still something subject to debate. As I will discuss later, Mäki defends a view in which false models can serve a purpose of truth.

> agenda, further assumptions were imposed—entailing assuming *particular* microeconomic foundations (competitive equilibrium, rational expectations, etc.)" (Stiglitz, 2011, p. 168; emphasis in the original).

Likewise, Colander, et al. (2009) put forward that the concept of microfoundations for macroeconomics has to be restated in order to develop models that allow us to deduce macro events from actual microeconomic regularities. "For instance, only a sufficiently rich model of connections between firms, households, and a dispersed banking sector is likely to allow us to get a grasp on 'systemic risk', domino effects in the financial sector, and their repercussions on consumption an investment" (p. 258). That is, they seem to agree that the attempt to give macroeconomics solid microfoundations is not problematic. What they seem to be criticising is the *way* in which this attempt was accomplished. In a similar fashion, Paul Krugman has been quoted as arguing that much of the past 30 years of macroeconomics was "spectacularly useless at best, and positively harmful at worst" (The Economist, 2009).

In consequence, in order to clarify the criticisms, to understand in what sense the use of these assumptions is problematic for macroeconomic models, it is important to go back to the microfoundations project, as a way to understand how is it that these assumptions were used in the first place, when the attempt to provide more solid microfoundations to macroeconomics started. In the next section I give an account of the microfoundations project.

2. Microfoundations

It is to some extent clear that in order to get a deeper grasp of the criticism that has been made to macroeconomics it is necessary to revise the origins of what provoked the criticisms in the first place. Therefore, in this section I describe how the idea of the microfoundations came about in macroeconomics and discuss some of the elements that have been object of the criticism, namely the assumptions of the representative agent and the rational expectations.

Keynes against the classics

The attempt to provide macroeconomics with microfoundations was, in simple terms, the attempt to make economic theory internally consistent, by explaining macroeconomic phenomena as stated by John Maynard Keynes in terms of individual rational behaviour. It began as a consequence of the new distinction that Keynes made in his criticism of the Classics. Keynes is considered to be the founding father of macroeconomics. But not because he first used the term or invented it; the term 'political economy', as the discipline was called as early as the seventeenth century, means the management of the state, coming from the word 'economics' that is derived from the Greek word that means management of the household. The reason for which Keynes is considered the founding father of macroeconomics is because he clarified the distinction that we use today between macro and microeconomics (Hoover, 2004). Before Keynes' main contribution, his *General Theory of Employment, Interest and Money* (1936), economics was dominated by classical[8] thought. The distinction made in the classical thought obeys to the classical dichotomy, which is the contention that there is a separation of the real from the nominal sector; or that real variables are independent from the nominal ones. Accordingly, the distinction made then was between theory of value and distribution on the one hand, which is about the determination of relative prices and allocation of resources (the real sector); and on the other, the theory of money (the nominal sector), of which the quantity theory was the dominant macroeconomic theory. Keynes, in turn, argued that such distinction was not real:

> "The division of Economics between the Theory of Value and Distribution on the one hand and the Theory of Money on the other hand is, I think, a false distinction. The right dichotomy is, I suggest, between the Theory of the Individual Industry of Firm…on the one hand and the Theory of Output and Employment *as a whole* on the other hand" (Keynes, 1936, pp. 292-3)

[8] Even though there was a clear distinction between classical and neoclassical, the differences between these two schools are more explicit in the microeconomics than in the macroeconomics. Furthermore, Keynes referred to the 'classical' school by including Smith, Ricardo and Mill, but also the followers of Ricardo like Marshall and Pigou. For these two reasons I refer to Keynes being preceded by the classical model. This is also the use of Snowdon, B., & Vane, H. (2005).

Although he did not contribute a straightforward model of the economy, Keynes did establish a framework in which the pressing problem of the time, the Great Depression, was addressed. During this period, unemployment in the United States increased from 3.2% in 1929 to 25.2% in 1933. Likewise, real GDP[9] fell from $103.1 billions to $73.7 in the same years (Snowdon & Vane, 2005, p. 78). Upon this situation, Keynes controverted the classical idea that all markets, including the labour market, clear in equilibrium, for he raised the question of existing involuntary unemployment during equilibrium. In more specific terms, Keynes' framework results were that in equilibrium there could be an excess supply of labour—involuntary unemployment. The problem with this result is that it was inconsistent with classical assumptions: in the classical model the competitive market is ruled by Walras' Law, which states that in a multimarket system with *n* markets, where the value of purchases equals the value of sales in all markets and *n -1* markets are in equilibrium, it follows that the n[th] market is also in equilibrium. Therefore, when the goods and financial markets are in equilibrium, the labour market cannot be in disequilibrium—unemployment is not plausible. This result, it should be emphasised, follows from the assumption that agents in the economy—firms and households—are aiming to maximise their profits and utility. In consequence, the results that Keynes obtained were incompatible with the microeconomic fundamental assumption of individual maximising rational behaviour.

The classical model departs from the notion of a competitive labour market where the equilibrium level of employment is reached at the market-clearing real wage. Together with fixed capital and total factor productivity, the equilibrium level of employment determines the aggregate production function. In other words, in the classical model the level of output is determined by the position of the aggregate production function once the equilibrium level of employment is determined in the labour market. Say's Law and the theory of interest rate determination guarantee that the equilibrium reached with aggregate demand is actually consistent with the full employment output. In sum, the Classics assumed that the labour market is in equilibrium and therefore attributed very little importance to the determinants of aggregate demand or the stabilisation policies that could have effect on them in order to promote full

[9] Measured in prices of 1929.

employment. By contrast, Keynes' theory is supported in the principle of effective demand, which states that in a closed economy with spare capacity, the level of output—and employment—is determined by effective demand, independently of the level of supply (Snowdon & Vane, 2005).

General Equilibrium as a tool

From the moment of the publication of *The General Theory*, the attempt to reconcile the macroeconomics of Keynes in terms of the classical microeconomic postulates of rational behaviour began to take place. For such an endeavour, the general equilibrium theory seemed like the most intuitive way to proceed. The general equilibrium theory dates back to the 1870s and specifically to Léon Walras and the Lausanne School. It was Walras who first suggested that, under certain conditions, equilibrium in every single market of the economy can be attained as a result of the maximising behaviour of economic agents—therefore the name of *general* equilibrium.

By contrast to this idea of general equilibrium, it was Marshall who later on, in the attempt to link the concept of marginal utility with the classical outcomes of Smith, Ricardo and Mill, attempted the use of partial equilibrium analysis and popularised the use of diagrams like the cross of supply and demand. In consequence, microeconomics was developed in two different—though related—types of analysis: one, the partial equilibrium analysis, which is used in most of what is called microeconomics—as opposed to macroeconomics—where a specific segment of the economy is studied, with the implicit assumption that the overall effect on the economy as a whole can be disregarded. For example, the study of the effect of a tax in a particular industry. On the other hand, the other type of analysis, general equilibrium, assumes that there can only be a general equilibrium, implying that one industry cannot be in disequilibrium while the others are in equilibrium. In consequence, this type of analysis contemplates the effects that a change in a segment of the economy has in the overall economy.

> "Microeconomic analysis that treats individuals as optimising within a wider context that is fixed, so that there is no feedback from the choice of the individual to the economy as a whole, is called *partial equilibrium*. Partial equilibrium is frequently justified on the

assumption that the individual is small relative to the whole, so that feedbacks are negligible. Microeconomics that considers the complete set of interactions that makes up an economy is called *general equilibrium*. It is still a type of *micro*economics, even though there is a sense in which it treats the economy as a whole, since the individual agent remains the driver and there is no place for aggregates" (Hoover, 2010, p. 330, emphasis in the original).

Although providing a precise characterisation of the general equilibrium theory is a difficult task for there have been different constructions of which perhaps the Arrow-Debreu-Mantel is the most popular, Weintraub (1985) provides a characterisation that allows for multiple interpretations and hence its usefulness to illustrate in more precise terms what the framework is about. According to Weintraub (1985) the general equilibrium framework is assembled around the following hard-core propositions[10]:

HC1. There exist economic agents.
HC2. Agents have preferences over outcomes.
HC3. Agents independently optimise subject to constraints.
HC4. Choices are made in interrelated markets.
HC5. Agents have full relevant knowledge.
HC6. Observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium states.
(Janssen, 1993, p. 43; quoted from Weintraub, 1985).

As it can be observed, propositions HC1-HC5 are related to the assumption of rational behaviour of individuals, and proposition HC6 is the concept of equilibrium that suggests that each plan of every individual is the outcome of maximising behaviour and that all individual plans are mutually consistent. In consequence, the general equilibrium framework can be said to be based on two theoretical principles, namely the notions of individual rational behaviour and equilibrium, which, at the moment, seemed appropriate for the purpose of reconciling Keynesian macroeconomics with classical individual behaviour. As it was mentioned earlier, the idea of the microfoundations was to put Keynes' macroeconomic findings—incompatible with the classical insights as he stated them—in terms of the Classics, derivable from individual rational behaviour.

[10] Weintraub describes them as core-propositions because he uses a Lakatosian framework in which theories are composed of a hard-core and a protective belt. I will use his notation but in no respect I am arguing for a Lakatosian framework.

John Hicks, one of the first to attempt to interpret Keynes, wrote: "I believe I had the good fortune to come upon a method of [economic] analysis which is applicable to a variety of economic problems [:]…the method…is perhaps, most illuminating when it is applied to the most complex problems (such as those of trade fluctuations)" (quoted in Weintraub, 1977, p. 3). Likewise, Oskar Lange, who continued with Patinkin the work of Hicks, in 1944 wrote:

> "According to traditional economic doctrine, unemployment is entirely due to rigidity of factor prices…Lord Keynes maintains that, under certain conditions, changes in money wage rates have no effect upon employment but influence only the level of product prices…The diversity of opinion can be disentangled only by considering the problem within the framework of the general theory of economic equilibrium" (quoted in Weintraub, 1977, p. 3).

In this respect, it can be said that in the attempt to provide macroeconomics with microeconomic foundations, the method that was considered more appropriate for such a task was that of the general equilibrium. By the 1930s, when the debate of Keynes and the classics began, general equilibrium theory was in a good position. Cassel and Wald had improved and developed in a consistent manner the framework that Walras had put forward in 1874. The way in which Walras proved that equilibrium could exist was somehow unsatisfactory, for his proof relied basically on that his system had as many variables as equations and therefore it could be assumed that a solution existed. Hence it was Cassel and later Wald—precisely in 1936, the same year of the publication of *The General Theory*—that "reported the first true solution of the general equilibrium problem" (Weintraub, 1979, p. 20).

However, later in the 1940s Samuelson, who was concerned with the concept of dynamics, unveiled new complications with the theory. In particular, he argued that economists had never been properly concerned with formulating dynamic theories in spite of relying on comparative statics in order to study how changes in certain parameters affected the equilibrium outcome. In other words, Samuelson emphasised the strong relationship between the concepts of existence and stability by highlighting the role of the dynamics, for it cannot be ascertained that a 'new' equilibrium will be reached—and thus compared to the 'old' one—after a change in a parameter if it is unknown the "path" that will be taken. In consequence, the use of the general equilibrium approach as a method to provide microfoundations to the macroeconomics became relegated, and rather, the attempt to make general equilibrium theory a more solid theory, with proofs of existence, stability, and uniqueness, became a new research endeavour on its own. Nonetheless, the piecemeal advances that were made in this direction, nurtured into the research of the microfoundations.

Three stages in 'microfounding' the macroeconomics

According to Backhouse (1995), the attempt to provide solid microfoundations to macroeconomics, which he calls the evolution of macroeconomics, can be divided into three stages. The first one is when the formal modelling of the economy took place. As it was mentioned earlier, right after Keynes published *The General Theory* in 1936, among several other attempts, Hicks with his *Mr. Keynes and the 'classics'; a suggested interpretation* (1937) provided a simpler and concrete version of Keynes' model. Later on, it was Hicks again who first attempted to reach Keynes' results using the Walrasian general equilibrium system in his *Value and Capital* (1939), "…We shall [with this method] thus be able to see just why it is that Mr. Keynes reaches different results from earlier economists on crucial matters of social policy" (quoted in Weintraub, 1977, p. 3). These models, although appealed to some individual behaviour to provide aggregate explanations, they were stated only in terms of aggregate variables. Furthermore, the individual behaviour did not emerge out of a rational individual who maximised utility, but were simply assumed. For instance in the consumption function the marginal propensity to consume is based on the notion of what Keynes referred as the "fundamental psychological law" that, when someone's income increases, her consumption also increases but only by a portion of the total amount. This notion however, is not the result of a maximising behaviour. Instead, it was simply assumed by Keynes, because he considered it a psychological law. And regarding the model being stated in terms of aggregate variables, for example, the two equations of the basic IS/LM model—Hick's interpretation of Keynes—are:

$$Y = C(Y\text{-}T) + I(r) + G \qquad IS$$

$$M/P=L(r, Y) \qquad LM$$

where Y represents national income, C consumption, T taxation, I investment, G government spending, P price level and r the interest rate. And at the same time, the consumption function is stated as $C = a + cY$, where a is the autonomous consumption and c is the marginal propensity to consume ($\partial C/\partial Y$). The psychological assumption here is that it is assumed that the consumption of individuals is composed of two parts: autonomous consumption, which refers to consumption that is fixed or determined, and another part that depends on output. The problem with such an assumption is not that it is based upon individual behaviour, but rather that it is not justified on rational behaviour. That is, such an assumption is not derived from the maximising behaviour of an agent who intends to maximise utility but is rather assumed *ad hoc.* Lucas referred to 'free parameters' as those parameters in the model that were not the outcome of maximising rational behaviour. In a similar fashion, Leontief (1937) "criticised Keynes for what he called 'implicit theorising': of assuming theoretical relationships without specifying them sufficiently precisely for it to be possible to say exactly what they implied" (Backhouse, Interpreting Macroeconomics: Explorations in the history of macroecononic thought, 1995, p. 120).

The second stage involved the gradual removal of the 'free parameters'. New models were built to explain the outcome of the macroeconomic models as the result of maximising behaviour. In other words, a maximising behaviour *justification* was found in the models built. But this, naturally, did not have any effect for the macroeconomic models, for they were considered simplifications of the optimising models. Despite the effort to get rid of the free parameters, models as important as Friedman's criticism of the Philips curve (1968) still contained a free parameter, namely the assumption that individuals form expectations of inflation according to actual inflation with a lag. The speed with which individuals adjust their expectations was not justified under rational behaviour, a reason for which the attempt to apply rational behaviour consistently was not yet accomplished.

The third and final stage corresponds to the 'rational expectations revolution', which started in the early 1970s with Robert Lucas Jr., who put forward the notion that optimising rational behaviour should be applied to all aspects of the macroeconomic models. In this period, supply and demand were derived from optimising models, but also were expectations and market equilibrium conditions. That means that the necessary elements to put the economy in motion, namely an equilibrium, preferences, production technology, and expectations, were all derived from individual maximising behaviour. According to Backhouse (1995), the systematic use of optimising behaviour characteristic of the New Classical School, a school of thought that emerged with Lucas, was being applied systematically in a way 'that was not true of earlier work in macroeconomics'. Therefore, what started by Hicks in 1939 and was followed by Lange, Patinkin and Friedman with his permanent income hypothesis, in which he attacked the Keynesian consumption function and therefore the fiscal policy multipliers, fundamental for the Keynesian theory, paved the road for the establishment of the systematic use of microfoundations that today play a central role in macroeconomics (Mankiw, The Macroeconomist as Scientist and Engineer, 2006).

Rational expectations: individuals as mediators

So far, I have given a brief description of the events that conduced economists to attempt to provide microfoundations to macroeconomics. Briefly, it can be said that it was due to a theoretical inconsistency between the macroeconomics as proposed by Keynes and his interpreters on the one hand, and the classical theory on the other. Undoubtedly Keynes raised important questions, particularly with the great depression, that classical theory could not answer comfortably. Therefore, investigation about how was it that Keynesian macroeconomics could reach so different results, was needed.

Aside from the need to put an end to this theoretical inconsistency, there are at least two further reasons for which attempts were made to produce macroeconomic results as the outcome of individual behaviour (Janssen, 1993): first, because it serves as a reference point from which understanding must emerge. That is, other levels of interaction, above the individual are not so easily distinguishable. The second reason, which I consider more important, is because of the character of economics and macroeconomics in particular. This is, macroeconomics is concerned with addressing issues of the economy as a whole such as output, employment, inflation and interest rates; aggregates that are strongly related with public policy. And even though public policy is aimed to have effect on the economy as a whole, the mechanism through which it operates is one in which the individual plays the role of a mediator. The mechanism can be considered to work in two steps: firstly by influencing individuals or their behaviour in order to secondly achieve a particular outcome at the system level. Accordingly, it is important to have an accurate understanding of how policies affect individual behaviour and how this in turn generates a large-scale outcome.

This argument is very much related to what Lucas (1976) sustained in his famous 'Lucas Critique' where he argued against the faculty of macroeconometric models, which are based solely on aggregate relations, to guide policy:

> "Given that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision-maker, it follows that any change in policy will systematically alter the structure of econometric models." (p. 43).

Lucas argued that economic policy could not be based on macroeconometric models that only use aggregate relations as their input because these relations do not necessarily hold after the introduction of an economic policy. This means, in other words, neglecting the role of mediators that individuals have when a new policy is implemented, for the policy affects their behaviour and this in turn has an overall effect in the economy. Therefore, for Lucas, models should take into account how rational economic agents adapt to these changes in economic policy. That is, instead of modelling relationships between aggregates that will perhaps not hold after a certain policy is implemented, the behaviour of individuals should be modelled.

Arguments have been provided against this idea that stable relations are only at the individual level and not at the aggregate level, for it is said that stable aggregate relations can hold because 'on average' they cancel out, while at the individual level relations can be rather erratic. However, individual responses like tax evasion to tax policies seem to suggest that people indeed adapt their behaviour in response to particular policies that undo the overall effect intended by the policy. Therefore, according to Lucas, models should be built with the assumption of rational expectations, which is the attempt to take the level of analysis to the individual, by acknowledging that individuals understand changes in policy and that they incorporate the expectations of these changes to be made in the future, into the choices that they make today. It is, in other words, the attempt to trace the first step of the mechanism that operates when a public policy is enforced in a dynamic framework. Viewed in another way, expectations are part of the 'law of motion' of the economy, for the expectations or beliefs of consumers, firms and investors about the future, determine how the economy is shaped. Therefore, a dynamic economic system is "propelled" by an expectations feedback system. Although it was not Lucas (1976) who first talked about rational expectations—the first one was John Muth in 1961—it was his critique that attracted initial attention towards the seriousness of the implications of such an assumption in macroeconomic modelling.

The assumption of rational expectations, according to Snowdon & Vane (2005), has two possible interpretations: on the one hand, a weak version, in which individuals make use of all the information available of the factors they consider that determine a variable, in order to form expectations about that variable. On the other hand, a strong version, the one provided by Muth, is that the expectations of economic agents will coincide with the objective mathematical conditional expectation of each variable in the model. That means that the expectations that individual agents hold about each particular variable of the economy are exactly those of the model. In the General Equilibrium framework provided by Weintraub stated above, the weak version of the rational expectations can be considered to be stated in the propositions HC1-HC3, where it says that agents optimise subject to constrains—or optimise subject to all the information they have available—while the strong version—that agents have the information from the model—can be interpreted from proposition HC5, which states that agents have full relevant knowledge.

So far, I have established that behind the microfoundations there were two interrelated purposes: first, a theoretical one, which was to reconcile the results obtained by Keynes in terms of the Classical framework, which was based on individual rational behaviour. The fact that there were two separate bodies of theory that yielded different results was considered unsatisfactory with regard to the 'scientific status' of the discipline. Second, given the nature of macroeconomics, to put the individual in the role of mediator, as a way to understand how macroeconomic policy affects individuals and how these in turn react to policies, producing a particular outcome. In other words, to understand the possible feedback effect that a macroeconomic policy can have on the economy when individual agents are mediators. General Equilibrium theory seemed appropriate to accomplish these two purposes: given its general nature, it was meant to keep track of all the markets in the economy. Furthermore, it was compatible with both the weak and strong versions of the rational expectations, the assumption that indeed individuals posses all the information available, in order to understand how they react to a particular policy.

However, using the general equilibrium described above, together with the rational expectations assumption, has very strong implications that under many points of view are hard to sustain. In this section I will address two of these implications, which have been what the criticisms related to the crisis have been about. The first one is the aggregation problem and the second is the consequent use of the representative agent.

The assumption of the representative agent

The aggregation problem arises with the attempt to use a framework of general equilibrium. General equilibrium, as I already discussed, is based on the idea of evaluating the equilibrium that emerges out of the interaction of every agent in the economy. This means that in principle the choice of every agent, given their preferences, the production technology of every firm and the constraints that each of them faces, has to be taken into account. In other words, it refers to the idea of how to establish a relationship between individual and aggregate behaviour. However, it is not as simple as adding up the preferences of the individuals in order to obtain an aggregate utility function. In order to aggregate the preferences of individuals, that is, to add one by one the preferences of every individual as a way to get a final aggregate preference function, very stringent assumptions about these preferences need to be made. Preferences, for instance, would have to be identical, meaning that every single agent would have to have the same preferences, say, to prefer chocolate to vanilla, and homothetic, meaning that every agent would have to spend the same proportion of their income on a particular good. And, "without perfect aggregation, there is no fixed relationship between the functional forms that might govern aggregates and those that describe the behaviour of individuals" (Hoover, 2010, p. 332). In addition, Arrow (1951) with his impossibility theorem in the domain of collective choice demonstrated that preferences cannot be aggregated unless conditions such as non-dictatorship[11] are violated. In consequence, as a way to escape this problem, the use of a representative agent has been adopted. This means that

[11] Other conditions are Pareto efficiency, independence of relevant alternatives, or unrestricted domain. For a discussion see Mueller (2003).

aggregate data is used as if it were the result of one individual's decisions. "[a] single agent or limited number of agents (typical of types or categories of agents), who follow microeconomic rules, stands for the whole economy" (Hoover, 2010, p. 332). The problem with the representative agent strategy is that under none of the two possible interpretations that it can take, it seems to deliver a reasonable concept, acceptable for the role that it is aimed to play (Hoover, 2010).

The first interpretation corresponds to one in which the utility function approximates the average preferences of individuals. This is the problem of aggregation that was discussed above and therefore can be discarded. The other interpretation corresponds to that of a social planner problem. That is, the maximisation problem to be solved is one that a social planner faces as a single individual that commands the economy. It is not a feasible interpretation because the planner would then be faced with the need to actually distribute endowments. Furthermore, there is little reason to believe that this sort of interpretation captures the actual way the market economy works. However, this is the interpretation that has been used, in which the optimisation problem is one in which "aggregate data is [treated] as if it were the result of one individual's decision" (Kirman, The economic crisis is a crisis for economic theory, 2010, p. 508). In other words, the aggregation problem is solved when an agent is assumed to maximise utility from macroeconomic aggregates subject to national income. "Yet, there are no individual agents and no individual commodities in the model. Rhetoric aside, the model mimics the mathematics of microeconomics without employing its substance" (Hoover, 2010, p. 334).

In consequence, it is not difficult to appreciate why the microfoundations project has been severely criticised. As Hoover (2010, p. 334) argues, "it is easy to lampoon the representative agent strategy: it is analogous to providing a reduction of the gas law to mechanics by modelling a single molecule scaled up to room size". It is in this line of argument that critics of modern macroeconomics have attacked the discipline for its responsibility in the crisis. Nevertheless, again, one would have to defend the view that the aim of science is truth, and that truth is, for example, a matter of correspondence[12]. That means that beliefs are only considered to be true if

[12] There are perhaps more views of truth that would indeed compromise the use of the false assumption. However, I will not even attempt to discuss them briefly for the debate is rather complex. What I am trying to

they correspond to a fact or an entity that exists. In this case, it would be appropriate to reject assumptions like the representative agent—for a representative agent does not *exist*—or the rational expectations for it is clear that individual with perfect foresight do not exist either. But, defending a view like this one would have to be argued for—something the critics do not do—and enough reasons of how taking such a stance, makes us get closer to such truth. Another way in which the criticisms could have been taken for granted was if history had showed us that only when we have an accurate description of facts, we are able to gain understanding and to make predictions about phenomena. This, however, has not been the case either. In great many cases, take Watson mentioned above, or classical mechanics, that in a theory of motion, usually the size of objects is considered negligible. Again, if examples like this, in which there is not a perfect description of how we observe facts, could not be considered successful, the criticisms as they have been posed could be taken for granted. That is, however, not the case. Classical mechanics or the way in which DNA is paired to form a double helix are indisputable successes in the multiple applications that these insights have.

Purposes matter

An element that I think it's important to highlight is that the insights that we have got from classical mechanics or how DNA is paired are successful with respect to a particular purpose. In the case of a theory of planetary motion governed by Newton's laws, the size of the planets is considered negligible because other elements like their mass or their position are what matter. In the case of the microfoundations, I have shown that there were particular purposes that were pursued. That is, there were some purposes behind such assumptions, namely to reconcile Keynesian macroeconomics with Classical thought and to put the individual in the middle of the process of enacting a public policy. Whether the microfoundations project was successful or not—with regard to the purposes I have discussed—is both beyond the scope of this thesis and a matter of debate. But what I want to emphasise is that these purposes need to be taken into account in order to judge whether the assumptions that were used were inappropriate or not. The assumptions have not been assessed in the context of such purposes but rather in the context of

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emphasise is that we would have all to agree upon a particular view of science before the criticisms could be taken for granted.

the crisis. Again, using the analogy of the bicycle, the 'oma fiets' is being assessed with the standards of the downhill bicycle and not with the standards of an 'oma fiets', or a regular city bike. If Watson had been investigating about the process of transcription, that is, the way in which the genetic code is read from the DNA and copied in a sequence of RNA, perhaps the tin and cardboard model would have been entirely useless. However, since he was working with the structure of the molecules of the nucleobases, the cardboard was enough and proved to be successful. In his cardboard model Watson assumed that DNA was made of cardboard, ignored the ester bonds that keep the polymers together and dismissed the information carried in the DNA. This example, in my view, only shows that assumptions made in modelling can be made or assessed only by keeping into account the purposes of the model first. Therefore, in the following chapters I will discuss a view in which this consideration is taken into account, in order to provide a philosophical approach to the way in which theories are built and the role that models play in theories and in the scientific practice. Although the discussion is far from settled, for there are many different levels on which philosophers in the discussion do not seem to agree with, what is clear is that the way in which assumptions are used in models is far from straightforward.

I started the chapter with the claim that the 'appropriate for the wrong purposes' and the 'wrong assumptions' arguments needed to be kept separate and not conflated, as the critics have. Then, I suggested that the 'appropriate for the wrong purposes' argument is of an institutional kind that obeys to the incentives that exist within the profession to guide the research efforts in one direction or another. That is, that the critics have lambasted the discipline for having focussed on the wrong purposes. I decided not to discuss this issue, although I provided the view of Colander (2010) who has three suggestions as to how this alleged institutional failure of crowding out first-order research directions might be corrected. Instead, I addressed the 'wrong assumptions' argument, which states that economic models failed with regard to anticipating the crisis because the assumptions used were unrealistic or against evidence obtained from other branches of science. However, I am closing the chapter by arguing that purposes matter. Thus, it would seem as if I am not keeping the two arguments separate either: first saying that the discussion about purposes is a different matter and then saying that purposes matter for establishing when the use of some assumptions is justified. Just to clarify, what I have been trying to argue that needs to be kept separate is the argument about whether the purposes that have been pursued are the right ones. That is, the institutional aspect; the decision to pursue some research directions at the expense, perhaps, of others. As I already pointed out, if one acknowledges that a model was not made for a particular purpose—in this case to study how markets can fail—it is superfluous to say that the model's assumptions were wrong, because it is clear that they were wrong for a different purpose other than the one it was originally built for. Thus, by arguing that the two arguments mentioned need to be kept separate I am not undermining the role of purposes or arguing that they are a completely separate issue when assessing the use of assumptions in modelling. On the contrary, as I will discuss later in Chapter II how models are used to represent reality, the role of purposes in scientific modelling is paramount in the act of representing.

CHAPTER II:
How models are used to represent reality.

1. The critics once more

In the previous chapter I argued that in the criticisms that have been made to economics because of the economic crisis, two different arguments have been put forward. The first is about economics having failed to answer the questions that are considered important. This one, I left it aside because it involves resolving an issue that has more to do with institutional arrangements. It does not mean that these are unimportant or not worth exploring. As I mentioned before, Colander (2010) considers the crowding out of some research directions to be an outcome of how institutions have moulded the research interests within the discipline and suggests three particular changes that could be made in these arrangements in order to foster a broader range of research topics and methods. This is a question that deserves attention, but that unfortunately for matters of space and time, has to be left for another occasion. The second argument, the target of this thesis, is about the intrinsic failure of the economic models because of the assumptions on which they are based, in particular the rational expectations and the representative agent. For commentators such as Stiglitz (2011) the failure of the models using these assumptions lies on that they leave out too much. That is, the models are based on a very special kind of microeconomics that can only account for phenomena in 'normal' times. For commentators such as Colander, et al. (2009), the representative agent assumption corresponds to an extreme form of conceptual reductionism that is atypical compared to the concept of reductionism that is typically used in the natural sciences. That is, for Colander, et al. (2009) the concept used in economics is not the same as the one used in the natural sciences and for them this is a problem. Likewise, they argue that findings in other branches of science like behavioural and experimental economics and psychology, suggest that individuals form expectations in a way that is incompatible with the assumption of rational expectations. Evidence obtained in these other branches suggests that individuals respond to situations following heuristic decision rules, given their 'bounded rationality'. Furthermore, there is also evidence that suggests that individuals can also respond to particular situations following some kind of herd behaviour. On the contrary, the rational expectations assumption suggests that individuals make use of all the information available of the factors they consider that determine a variable, in order to form expectations about that variable—in the weak sense—or that the expectations of economic agents will coincide with the objective mathematical conditional expectation of each variable in the model—in the strong sense. A first response to these claims, as I already suggested, is that none of them are as straightforward as the critics seem to insinuate. As a response to Stiglitz, for instance, it could be said that having very simplifying assumptions does not seem to be problematic in the first place. Quite on the contrary, if we could ask for an ideal model, perhaps it would be a very simple model that could explain everything. That is, explain much by little. To be fair, Stiglitz claim is that too simplifying assumptions can only explain very limited cases. But, in that case it can be said that no model explains everything, or much. It would be desirable, but not because some model cannot accomplish such desire it can be considered a failure. Regarding Colander, et al. (2009), that a model in economics does not follow the naturalistic paradigm does not seem to be problematic. Hardly any philosopher of science today is committed to this. Therefore, it is not so straightforward that because an extreme concept is being used in economics—as opposed to the typical use that is given to it in the natural sciences—the modelling is flawed. In addition, provided the example of Watson and his tin and cardboard model of DNA, it is not clear why models should be realistic in every sense. This example suggests that depending on the purpose of the investigation, some elements in the models do not necessarily have to be true. For Watson's investigation, it was enough to use tin and cardboard, given that he was interested in the structure of molecules, which can be modelled using tin and cardboard. To be sure, neither Stiglitz (2011) nor Colander, et al. (2009) tell us why the representative agent and the rational expectations assumptions are flaws in accordance with some purposes that the models were aimed to satisfy. In fact, the critics are judging the use of some assumptions in general, without acknowledging the wide variety of purposes that these models have served. They are judging the assumptions solely related to the crisis, but as I already commented, the models were not necessarily meant to capture market failure and the possibility of crises; this was not their purpose. For instance, Mankiw (1990) divides the developments of macroeconomics from the 1970s up to the 1990s—when the paper was written—in three broad categories: one, in which research efforts were guided exclusively on expectations. More specifically, this category was about modelling expectations in 'a more satisfying way' than it was done before, and testing the implications of the use of such assumption in standard models (Mankiw, 1990). Some research developed in this domain was the monetary policy irrelevance, started by Sargent & Wallace (1975); the rules versus discretion literature that started with Kydland & Prescott (1977); and the empirical test of the permanent income theory of consumption, first started by Robert Hall (1978). The second category proposed by Mankiw (1990) is the research that attempted to explain macroeconomic phenomena using new classical models. This literature is mostly the one I referred to in the previous chapter, as the attempt to explain economic fluctuations with the assumptions that markets clear, instead of adopting the view that output fluctuations are the result of disequilibrium. In this area, models of imperfect information were explored, in which agents only were aware of the prices of the goods they produced but not of those they purchased; models or real business cycles, in which output fluctuations are explained by random shocks that affect aggregate supply (See Chapter III, Kydland & Prescott (1982)); and models of sectorial shifts, which also attempt to explain the business cycle emphasising the costly adjustment of labour among different sectors of the economy. Finally, the third category, New Keynesian macroeconomics, which takes economic fluctuations not as the efficient response of the economy to changes in preferences and technology—as the new classical approach—but rather as the result of market imperfections. The most common of these imperfections is the wage and price stickiness, which in a more developed type of models, resulted out of monopolistically competitive firms, that faced costs in price setting (see Chapter III, the model by Blanchard and Fischer for an example). So, according to these three categories, even those interested in economic fluctuations as market failure were concentrated in how market imperfections like monopolistic competition settings could have macroeconomic effects. These purposes were quite different from establishing how a systemic market failure could occur.

In this respect, coming back to the criticisms, the approach taken by the critics seems to be one in which it is clear and straightforward for them how models should be used to represent reality. That is, critics argue according to a preconceived view of the way in which models should be used, as if 'the right way' to construct and use models were either evident or pre-established according to a particular rule. For example, taking together the criticisms of Stiglitz (2011) and

Colander, et al. (2009), there is at least one respect in which the critics seem to adhere to a modelling rule, namely that the critics argue implicitly for realisticness in modelling. In the case of Stiglitz (2011), by claiming that the models make too strongly simplifying assumptions. Therefore, he is implicitly arguing that models should take more elements from reality, and model them accordingly. His work in economics, for instance, has focussed on modelling markets with asymmetric information as a way to show that markets by themselves are not efficient in all circumstances, arguing that there are some cases in which government intervention can yield Pareto–efficient outcomes. Likewise, as I already mentioned, Colander, et al. (2009) argue implicitly for realisticness by saying that models should not only be internally consistent but also externally consistent. That means that models should not only make sense within their own construction but should also make sense in terms of the way in which we observe reality. In this case, it is the incompatibility of findings in other branches of science about individual decision making and forming of expectations, with the assumption of rational expectations.

Above I have maintained that the criticisms as they have been posed are not entirely clear, and I have given two reasons for it. One of these reasons is that the alleged flaws in the use of the assumptions are not evident. That is, what makes the criticisms become obscure is not that the critics argue for some sort of realisticness. If this were the case, they would have to show us why it is that a realistic approach to modelling is paramount for the success of science. The problem with their criticisms is rather that they argue as if there were a preconceived methodological precept that pointed out the right way to model and that the models they are referring to were not following such precept. As I will discuss later in this chapter, this *a priori* view of what makes good science is an aspect that can be associated with Positivism, a view no longer held in the philosophy of science. It was abandoned, among other things, for its inaccurate portrayal of how science is made. Although I am not claiming that this is a view held by the critics themselves, the fact that they argue about the use of assumptions in economic modelling without considering other postures, resembles an *a priori* view of what good science is, that can be associated with Positivism. A brief discussion of the latter will be presented in the next section, as a criticism that McCloskey (1983) made about the economics profession.

Modernist attitude

Already a long time ago, D. McCloskey (1983) criticised the economics profession for having an attitude that she denounced as modernist, in which an *a priori* view of a correct science was one of the features she criticised. According to her, economics has two different attitudes towards discourse: one, the official rhetoric, being the one that economists and methodologists preach to follow but don't actually practise. The other, the unofficial rhetoric, being the one that is actually practised but unexamined. According to her, the official methodology, the one that is preached as official, is modernist, which means that it is "an amalgam of logical positivism, behaviourism, operationalism, and the hypothetico-deductive model of science" (McCloskey, 1983, p. 484). In addition, she enumerates a few precepts as characteristic of modernism, of which, for this case, I consider four of particular relevance (McCloskey, 1983, p. 484)[13]:

i) Prediction (and control) is the goal of science.
ii) Only the observable implications (or predictions) of a theory matter to its truth.
iii) Observability entails objective, reproducible experiments.
iv) If (and only if) an experimental implication of a theory proves false is the theory proved false.

These four are of particular relevance because they appear to be close to what the critics seem to have in the back of their minds when criticising the assumptions, without really making it explicit or arguing for it. For the first precept, prediction as the goal of science, it is clear that what motivated the criticisms in the first place was the fact that the dominant models did not predict the crisis. As I already discussed, Stiglitz (2011) considers prediction to be the test of any scientific theory and Colander, et al. (2009) argue that the discipline should be blamed for not anticipating the crisis, and for fostering it to some extent. Similarly, Paul Krugman, Nobel Laureate of Economics wrote in the New York Times: "Few economics saw our current crisis coming, but this predictive failure was the least of the field's problems" (Krugman, 2009). Had this idea of prediction not been behind the criticisms, the doubt whether economics was responsible or not would not have emerged in the first place. Actually, Hodgson (2011) enquires about this goal of prediction, questioning whether it should be the foremost goal of economic theory. He, however, argues in a rather opposite direction: Hodgson argues that it was perhaps

[13] Although I consider these four of particular relevance to the case, I will only elaborate on the first two.

this foremost goal of prediction what has some responsibility for the crisis, for exaggerating the capacity of the models used to predict future outcomes. Although I will not go into the details of his argument, I think it is important to emphasise that it is perhaps not only the economists behind the models, but of the economics discipline in general that seems to maintain this attitude of granting utmost importance to prediction. In that case, the modernism that McCloskey (1983) was referring to almost three decades ago, still seems to be embedded in the economic discipline.

As for the second precept, that only the observable implications (or predictions) of a theory matter to its truth, is a way to emphasise the necessity to 'corroborate' theories—or for this matter models—empirically, being such the ultimate test. In this respect, two aspects of the criticism come to the fore: first, that the models are trying to be dismissed because the test of reality—the most important one, as Stiglitz (2011) argues—proved them to be flawed. Secondly, that empirical findings in other branches of science are being taken as a test for the assumptions, like in the case of rational expectations. And it should be emphasised here that it is not about proving that the rational expectations are a 'good' assumption to make according to reality, the point is that there is no reason to dismiss such an assumption except for the fact that it does not resemble reality.

Furthermore, McCloskey (1983), very similarly to the point that I mentioned above, namely that the critics argue with an implicit methodology that they do not refer to, criticises the prescription of a particular, determinate methodology, arguing that it is arrogant and pretentious regardless of the label one uses. That is, the problem is not solely about a modernist methodology being imposed, or prescribed, or preached; it is about *any* methodology that attempts to prescribe a particular method for a science to be followed:

> "The objections to modernist method so far, however, are lesser ones. The greater objection is simply that modernism *is* a method. It sets up laws of argument drawn from an ideal science or the underlying history of science or the essence of knowledge. The claim is that the philosopher of science can tell what makes for good, useful, fruitful, progressive science. He knows this so confidently that he can limit arguments that worthy scientists make spontaneously, casting out some as unscientific, or at best placing them firmly in the 'context of discovery'. The philosopher undertakes to second-guess the

> scientific community. In economics the claim of methodological legislation is that the legislator is not merely expert in all branches of economic knowledge within sound of his proclamations but expert in all future economics, limiting the growth of economics now in order to make it fit a philosopher's idea of the ultimate good" (McCloskey, 1983, p. 490; emphasis in the original).

The last sentence in the above quote makes a point that is worth emphasising: any methodology that attempts to prescribe a particular method limits the growth and future of economics—or of any other science. I think this important because it highlights the uncertainty that we have regarding, especially, the laws—if there are such—that govern economic phenomena. In other words, we do not have complete certainty about the underlying causal relations that produce the outcomes that we observe. Our theories and models give us some insights about them and enough confidence to think that they will continue to hold. Nevertheless, we cannot be absolutely certain about it. To argue that a particular method ought to be followed for the sake of proper science, unavoidably implies complete knowledge about these underlying causal relations. And as I said before, we simply do not know everything about these causal relations. Or, rather, we don't know whether we know everything about these causal relationships. Therefore, to attempt to prescribe a particular method, deserting any other one, is to deny other ways that can give us insights about a particular phenomenon in the future. As I will discuss later, Paul Teller (2001) has a similar view as to why a model should not be something we determine *a priori*, but rather something that is determined once one decides to use a particular object as a model.

A final point to be made about the previous quote is the role that McCloskey attributes to the philosopher of science. In her quote, the philosopher of science is the one who prescribes the 'right' method to follow, the one who is above the practising scientist because he knows what is 'good, useful, fruitful, progressive science'. As I will illustrate in the following section, this philosopher of science portrayed by McCloskey is one whose ideas pertain to the logical positivists, a school of thought initiated by Moritz Schlick, with his Thursday evening discussion groups in the late 1920s, that later came to be known as the Vienna Circle. For the logical positivists, there were only two types of genuine knowledge: on the one hand, the *a priori*—analytic—knowledge, like logic and mathematics that was true in all possible worlds as long as it was correctly derived. On the other hand, the *a posteriori*—synthetic—knowledge, which was produced by empirical science. These two types of knowledge exhausted knowledge. Following Wittgenstein's ideas of the *Tractatus Logico-Philosophicus* (1922), metaphysical propositions, which were statements coming from religion, theology or anything other than logic, mathematics, or the empirical sciences, were considered 'meaningless'[14] (Hands, 2001). In the words of Hume, whose positivist, empirical epistemology was adopted by the logical positivists:

> "When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume—of divinity or school metaphysics, for instance—let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames, for it can contain nothing but sophistry and illusion" (Hume, [1748] 1955, p. 173, emphasis in the original).

Given the 'meaninglessness' of metaphysics, the logical positivists faced a dilemma. Their own propositions could not be regarded as pertaining to the domain of logic or mathematics or empirical science. Therefore, the role of philosophy, according to their own distinction, seemed to fall in the realm of the 'meaningless'. In consequence, the logical positivists redefined the role of the philosopher, to point what was 'meaningful' and what was 'meaningless' discourse (Hands, 2001). "In other words, the philosopher is reduced, or elevated, to the position of a park keeper whose business it is to see that no one commits an intellectual nuisance; the nuisance in question being that of lapsing into metaphysics" (Ayer, 1990, p. 5; quoted in Hands, 2001, p. 75). This view of philosophy continued to be held later by the logical empiricists becoming part of the Received View until its demise. The point, therefore, is that there was indeed a time—the time of the Received View—when there were philosophers prescribing rules in order to keep intellectual efforts within the realm of the meaningful. This attitude, however, has been long abandoned in the philosophy of science.

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[14] The young Wittgenstein, though never an active member of the Vienna Circle, was considered a logical positivist, in particular for his close relationship with B. Russell. Nevertheless, his use of the word 'meaningless', was not meant in the pejorative sense as intended by Hume, or the logical positivists, but rather to say that there is little point in discussing metaphysical propositions. Not because he disdained metaphysics, but the discussion about metaphysics he considered pointless. For example, his last statements in the Tractatus are: "6.522 There are, indeed, things that cannot be put into words. *They make themselves manifest.* They are what is mystical…7 What we cannot speak about we must consign to silence" (Wittgenstein, 2001, p. 89); emphasis in the original).

But that this attitude was abandoned does not mean, however, that a distinction has been established between what the meaningful is as opposed to the meaningless. Or, to put it more clearly, the role of the philosopher ceased to be 'a park keeper' not because it was finally established how it is that science conveys meaningful knowledge. Quite to the contrary, the debate about how it is that theories—or science—provide knowledge is still a matter of concern among philosophers and still very much unresolved. Therefore, what I am trying to point out is that the attitude of the critics, when they argue as if there were a preconceived idea of what good models are, is unwarranted. There is not such idea nowadays and philosophers of science have different views about how scientific theories and models represent reality. This implies that there are differing views about how we can learn from models and about what sort of knowledge can we gain from them.

As I commented above, with the Received View (RV), when philosophers were given the role to distinguish the meaningful knowledge from the meaningless, physics was used as the role model as to how theories should be structured in order to convey meaningful knowledge. Hence, theories were expressed in terms of first-order predicate logic. Nowadays, in general terms, the debate about the structure of theories, or how it is that theories are structured in order to provide us with insights about reality, has transformed in order to capture and resemble more the way in which science, and especially scientific *practice*, proceeds. This, as a consequence of the fact that theories are not rigid, determinate, isolated structures as portrayed by the RV, but rather malleable, incomplete and partial. Craver (2002) refers to them as "theories in the wild", meaning "as they are constructed, conveyed, learnt, remembered, presented, taught and tested by scientists" (Craver, 2002, p. 58). Hence, in the following section I will give a brief description of the way in which theories were conceived by the logical positivists and the philosophers of the Received View in general, as a way to contrast it with the debate that is currently held in the philosophy of science, in particular the way in which models are used to represent reality. In my view, there are at least two reasons why presenting this contrast is important. First, because, as I already mentioned, the attitude of the critics of economics today can be associated with the view of theories held during the Received View. It is not explicit, but again, they argue as if there was a clear view of how theories are constructed in order to represent reality. Second, because looking back at the debate around theories and models is important to understand the view that I want to highlight about how models are used to represent reality.

Once upon a time in the philosophy of science

The syntactic view of theories was the view of the structure of theories held by the logical positivists and was considered its 'epistemic heart' (Suppe, 2000). Philosophers no longer endorse the syntactic view of theories, also known as the Received View[15], for the way in which theories are represented is considered inaccurate; it is considered that it distorts many aspects that are relevant to science. However, it is the view that provided a logical pattern to the scientific argument and therefore, still considered indispensable for any account of the epistemology of science (Craver, 2002).

According to the syntactic view, theories are sets of sentences. These are sentences containing observation terms and theoretical terms. Both the observation and theoretical terms compose the non-logical part of the language that is part of an abstract formalism or, more intuitively, the formal language in which the sentences are expressed, which is the first-order calculus. The observation terms are constructed from observable phenomena, and via correspondence rules, the former are linked to the theoretical terms, which form theoretical postulates or axioms. Correspondence rules are therefore a bridge that link the terms that are formed from observations made about phenomena with the axioms stated only in theoretical terms. And a theory is, more specifically, the conjunction of theoretical postulates and correspondence rules. Since sentences are syntactic objects, their identity is independent of interpretation, and it is deduction that relates sentences between themselves. Therefore, "by focusing on the sentences on which a theory is composed, one can investigate the deductive consequences of these sentences without semantic distraction" (Hausman, 1992, p. 71). This is the reason why this approach has received the name of syntactic, because what is emphasised here is the way in which sentences are arranged, without the need to recourse to the interpretation of these sentences.

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[15] This is not entirely precise. The syntactic view of theories is rather part of what came to be known as the Received View, referring only to the structure of theories held by the RV. The Received View is a term used by Patrick Suppes and refers to a whole view held by the logical positivists and the later logical empiricists. For convenience, however, I will refer to the RV interchangeably with syntactic view, unless otherwise stated.

This, however, does not mean that in this view scientific theories do not have semantics or do not have interpretations. As any language, scientific language also has semantics and pragmatics. In the syntactic view, the sentences made out of observational vocabulary are the ones given a semantic interpretation. These interpretations for which the sets of sentences are true of are considered models. For instance, the specification of the domain of the variables used or the assignment of functions to function symbols are interpretations. A model, in the logician's terminology that characterises the syntactic view, is a model if and only if it is an interpretation that yields the sentences true.

One of the advantages considered of the syntactic view was that the formal connections between different problems could be established without the need for interpretations. A good illustration of this point is the already mentioned Arrow's impossibility theorem, by which he proved that a social preference ordering cannot be derived from individual preference orderings unless at least one of the four conditions he established was not fulfilled. His proof, like all proofs, was a syntactic proof. That means that the results are obtained deductively from the order of the premises and the conclusion that follows instead of from their interpretation. Therefore, the four conditions that he established could not be satisfied at once, namely collective rationality, the weak Pareto principle, non-dictatorship, and independence of irrelevant alternatives, were given a different interpretation by Alfred Mackay, and the same conclusion applied. Mackay proposed the four conditions to be used to consider the problem of deriving an overall ranking of athletic performance in a multi-event competition from the ordering of achievements in individual events (Hausman, 1992). As it can be observed, both interpretations deal with the problem of aggregating individual preferences or achievements, in order to obtain an overall preference ordering or an overall ranking of athletic performance. The interpretation given to the conditions, is of course different: for example the first condition, collective rationality, in Arrow's case is interpreted as "it must provide some social preference ranking for any profile of individual preferences", while for Mackay's case it is interpreted as "it must provide some overall ranking for any profile of finishes in individual athletic events". In consequence, the syntactic view offered an economical way of understanding phenomena. Given that two distinct problems shared the same logical structure, the same type of analysis could be used for both.

> "[B]y separating syntax and semantics, one economises on logical effort, and one can see precisely the formal identity of the distinct problems of scoring athletic events and making social choices. By seeing theories as syntactic objects and by formalising them, one might, in the positivist's view, put logic to work, gain just such an economy of logical effort and recognise the formal connections between distinct problems" (Hausman, 1992, p. 72).

However, with the demise of the RV, also the syntactic view was abandoned and an alternative semantic view was proposed. As Suppe (2000) describes, on March 26, 1969 Carl Hempel, one of the main developers of the RV, declared at the Illinois Symposium of the Structure of Scientific Theories that he was abandoning the view and the reliance on the syntactic axiomatisation, instead of presenting the latest revision of the view. According to Suppe (2000) the syntactic view was not abandoned because the semantic view could offer a much better description of scientific practice and therefore offered a better replacement, but rather because of failures of the former that were not related to the semantic view. In other words, the syntactic view was abandoned not because the semantic view offered a better description of what theories were, but because the syntactic view itself had several limitations. In this respect, Craver (2002) argues that in spite of the RV not being typically defended as an accurate description of theories, but rather as a "regimented reconstruction of their shared inferential structure" (p. 58), there is still a descriptive difference between such reconstruction and theories in the wild.

Suppe (2000) in particular argues that the most critical failures of the Received View are: first, that theories are not linguistic entities and thus theories are not individuated correctly; second, that correspondence rules are a heterogeneous confusion of meaning relationships, experimental design, measurement and causal relationships, some of which are not properly parts of theories; and third, that symbolic logic is an inappropriate formalism, in the sense that concern is raised about how serviceable the formalism is to the endeavour of modelling and understanding science. Regarding this last concern, Suppe (2000) argues that the way in which the syntactic view emerged was, as I mentioned above, as a model of science that intended to accommodate all genuine examples of good theories and to differentiate them from the clear examples of bad theories. Since modern physics was their role model of what good theories represented, the syntactic view was modified in order to accommodate the incompatibilities that emerged as physics progressed and posed new challenges. This however, made positivism "become sidetracked from development of their substantive ideas by technical problems that were mere artefacts of modelling science via predicate-calculus axiomatisations" (Suppe, 2000, p. S104).

These predicate-calculus axiomatisations raise several concerns regarding theory construction. Craver (2002) argues, for instance, that first, multiple, partial, or incomplete theory formulations are neglected or homogenised. This, in other words, means that theories written in a natural language, or simply diagrammed, put in a graph, ostensively explicated, or "(increasingly) animated in the streaming images of webpages" (Craver, 2002, p. 58) lie beyond the realm of the theories that the RV can accommodate. In addition, theories can often be put into different formalisms, influencing the use that they are given and the way in which they represent a particular phenomenon. Thus, "regimenting theories into the ORV[16] structure obscures the diverse representational tactics used by scientists when they deploy, express, and teach their theories" (Craver, 2002, p. 59). Second, that nomological, covering explanations are emphasised over causal/mechanistic explanations. This means that explanations that resort to causal/mechanistic explanation are not accommodated by the view and only nomological explanations are. A very simple but typical example that highlights the importance of causal/mechanistic explanations is the one of the sun, the pole, and the shadow. In this example the elevation of the sun and the length of the pole explain the length of the shadow produced by the pole—because the sun and the pole cause the shadow—but not the other way around. So, instead of explaining by subsuming a particular phenomenon under a general law, a causal explanation is needed. The RV cannot accommodate this sort of explanations. Lastly, Craver (2002) argues that mathematical structures required for expressing theories in quantum mechanics, relativity, or population genetics are awkwardly accommodated by the view. .

In relation to the first criticism posed by Suppe (2000), it is not perhaps that theories are not linguistic entities but rather that they are more than only linguistic entities. That is, the logico-linguistic nature of theories in the RV is too restrictive in some respects. For instance, since theories are regarded as sets of sentences, a change in the language in which a theory is stated means that the theory is no longer the same theory but rather another one. So, if a theory is stated

[16] Craver (2002) refers to the RV as the Once Received View (ORV).

in English and also in Spanish, it is, according to the RV, two different theories instead of one in two different languages. And, as French (2008) comments, "whether Newtonian mechanics is presented in English or Portuguese, it is still Newtonian mechanics" (p. 271). To defend the RV it could be argued that although theories are expressed in sentences in a particular language, they ultimately express propositions. However, this defence does not solve the problem, for arguing that theories are propositions implies that their semantic character—a non-linguistic entity—and not its syntactic nature is what is relevant. Of course, the syntactic view favoured the emphasis given to the syntactic character. In a similar way, Craver (2002) argues that the analysis of meaning of the RV enforces a successional account of theory change. Thus, theory dynamics—the process of generating, evaluating, revising and replacing theories over time—are neglected or distorted. Theories are too finely individuated to be able to explain the piecemeal and extended process of theory dynamics. That is, the process of refining and articulating theories is obscured and instead a clash of competing alternative theories is suggested, without this being the typical way in which theory dynamics has been observed in science or its history.

The second failure proposed by Suppe (2000), which is related both to theory construction and theory dynamics, namely the nature of correspondence rules and the role they play, can be divided into two different problems: on the one hand, their very distinct nature makes the concept of correspondence rules very confusing. That is, according to the RV, both a term obtained following a certain experimental design and an observed causal relationship are considered correspondence rules. The confusion emerges in particular because some of these correspondence rules would not be considered part of a theory, as the RV would classify them—recall that according to the RV, theories are the conjunction of correspondence rules and theoretical postulates. For example, it is difficult to consider a particular experimental design as part of a theory in the same way that certain causal relationship can be. The RV would consider both as correspondence rules in the sense that they both link the observational terms with the theoretical postulates. On the other hand, a change in a correspondence rule, a new experimental design, for instance, from which a different observational term emerges, yields a different theory, according to the RV. As I mentioned above, correspondence rules are the ones that link observational terms, with theoretical ones, which form themselves theoretical postulates or axioms. This means that the correspondence rules are the bridge between statements produced out of observed phenomena—or the world—and axioms. Therefore, any observation from the real world that can be fit into a sentence derived from the theoretical postulates, has attached a correspondence rule. The conjunction of these theoretical postulates and the correspondence rules, form a scientific theory. But a change in a correspondence rule, say, from an experimental innovation, according to the RV, means that another theory has been established. This is of course, a very strange result, because a new way to experiment with a phenomenon does not necessarily imply that a new theory has emerged, but in the way the syntactic view has been defined, this necessarily implies a new, different theory.

Aside from the failures pointed out by Suppe (2000), which point strictly to the intrinsic limitations of the syntactic view, there are at least two others that highlight its limitation in relation to the semantic view. That is, compared to the semantic view, the syntactic view does have some limitations, in particular explicating the way in which scientific practice proceeds. The first limitation is that scientists use many different kinds of models in their research rather than theories *per se.* This means that in great many cases scientists use models as a means to find an explanation or to advance in scientific knowledge instead of working on a theory directly. Economics is a very good example of this way of proceeding: economists can be said to work with two types of models. On the one hand, they use econometric models in order to find possible causal relationships using data and some theoretical postulates. On the other hand, they use theoretical models, which are models constructed from theoretical postulates and some initial conditions. With these models they test the results that are obtained given a set of assumptions, to check whether these results obtained make more than deductive/logical sense. That is, the results they obtain deductively from the theoretical postulates are contrasted with empirical data. Furthermore, the models that they use sometimes make the use of idealisations and in some other cases the models are built independently of theories. This is the distinction that Cartwright (1983) makes between phenomenological and theoretical models. For her, the quantum statistical approach for lasers in quantum physics shows that a large part of the explanatory work is made by phenomenological terms, which means that the results are not derived from a theory, but rather that the process described by the theory is mimicked in order to obtain a particular result. This way of proceeding in scientific practice cannot be accommodated by the SV, for either it would have to be accepted that theories and models are two separate sources of scientific knowledge—which would in turn require an independent analysis of the role that models play in science and how they are related to each other—or to treat models as 'little theories', having the same characteristics, which is certainly not the case (French, 2008).

There is a further limitation of the RV, which, to my purposes is much more significant. Namely it is that the concept of representation, at least in the way in which models in modern science represent, is absent in this view, while it is not in the semantic view. Representation, according to the second of the definitions in the Oxford English Dictionary means "an image, model, or other depiction of something". There are at least two reasons for which representation is particularly characteristic of models in science. On the one hand, if we take that reality is extremely complex and through science we want to explain phenomena, we need to simplify that complexity. As human beings we don't have the intellectual capacity to dominate or understand the structure of nature, if such exists, as simple and universal natural laws. In consequence, we need to represent phenomena in a way in which we can understand it. Scientists build models that represent some target phenomenon and that at the same time they can manipulate, in order to trace the effects of such manipulation. On the other hand, representation is fundamental in delivering scientific results: different phenomena are represented through graphs, like supply and demand in economics; equations, like van der Pol's equation in the treatment of lasers; images, like in climate science, etc. The RV does not capture representation in any of these two ways. According to the RV, the vehicles of representation are sentences, given that it is correspondence rules that give theories their empirical content—and thus their explanatory and predictive power—and these, together with theoretical postulates form theories, which are sets of sentences. And models are, as I mentioned above, sets of sentences for which the theory is true—in the logicians terminology. In other words, there is only space for linguistic representation according to the RV. However, this is not the case, not only because, as I mentioned before, there are many other ways in which science proceeds, like through the use of graphs or images, but also because even linguistic representation requires extra-linguistic elements in order to properly represent (Teller, 2008). To be applicable, for instance, the language with which sentences are organised needs other extra-linguistic skills like the ability to discriminate objects, properties or characteristics to which the meaningful units of language are applied. In the case of the syntactic view, for example, even without the need to provide an interpretation, it is necessary to be able to distinguish between a symbol of a function and a variable.

So far I have discussed the intrinsic limitations of the RV regarding the way in which it copes with scientific theories, in particular with their construction and dynamics. Furthermore, I have discussed two other limitations that the view has compared to the semantic view. Nevertheless, I have not yet provided an account of what the semantic view is. Therefore, in the following section I will provide a brief account of the changes that took place in relation to the RV. Nevertheless, it should be stressed that a thorough account of the semantic view would imply a revision of the progress of such a view and its different ramifications—Craver (2002) calls them 'cluster of alternatives'—since its conception even before the demise of the RV, up to the discussions that are held today; an endeavour that goes beyond the scope of this thesis. Therefore, in the following section I will give a brief description of it. In the next section I elaborate in one of the views that have emerged within the semantic view.

The semantic view of theories

As I mentioned in the previous section, there is nothing like *the* semantic view, for there are several views about what models are and how they are related to theories. Nevertheless, it is still possible to define a 'core' of what the semantic view is. Perhaps the most important insight about the semantic view—and hence its name—is that in this view theories are no longer regarded as sentences—syntactic elements—but rather semantic elements. That is, theories are considered in terms of what they mean—semantics—rather than how they are stated—syntactic. With the criticisms to the syntactic view, it was recognised that the relationship between theory and phenomena was much more complex than what theories presented as first-order predicate logic sentences could convey and what correspondence rules could capture. Therefore, there was a shift in the importance of how theories are stated to what theories mean.

> "On grounds such as these, van Fraassen and Suppe urge us to regard scientific theories as the set of models of which the sentences in any particular formulation—that is, the propositions or the meaning of these sentences—are true rather than as anything linguistic or sentential at all" (Hausman, 1992, p. 73).

More specifically, under the model-theoretic approach, theories are abstract specifications of a class of models that specify or define abstract or idealised systems. And models are structures that render true (or instantiate) a theory. In order to illustrate more clearly what this means and what the differences are with the syntactic view, an example in the form of a diagram helps (see next page):

Suppose a sentence written in first-order predicate logic: $\forall \upsilon : N(\upsilon) \rightarrow \neg C(\upsilon)$.

According to the semantic view, this is a sentence—expressed in formal language—that is true of a theory that states, "All Dutch people are discontent". As I mentioned above, it is the meaning—all Dutch people are discontent—what is a theory, rather than the sentence in formal language. By contrast, for the syntactic view the theory would be the sentence in formal language, while the interpretation or meaning, if true of the statement, would be a model. In addition, the sentence in formal language that is true of a theory in the semantic view is itself a model of the theory, for it is the theory—the meaning—expressed in terms of a sentence in predicate logic, just as it could be a graphical representation. Hence, models are in the semantic view representations or instantiations of a theory. In the case of the theory "All Dutch people are discontent" a model could be a data set in which the answer to the question "Do you consider yourself discontent?" is affirmative for every Dutch person, has been recorded. Another model of the theory would be a collection of pictures in which every Dutch person has been photographed and they all appear with discontent faces. As it will be discussed in the next section, these models are homomorphic with real systems. This means that they can be the same, or similar to the real system.

Semantic View

$\forall \upsilon : N(\upsilon) \rightarrow \neg C(\upsilon)$ A sentence that is true of theories [itself a model]

Interpretations:

1) All Dutch people are discontent
2) All German people are discontent
3) All Finnish people are discontent

Theory $\longrightarrow$ A model (an instance of the theory)

Syntactic View

$\forall \upsilon : N(\upsilon) \rightarrow \neg C(\upsilon)$ Theory

Interpretations:

1) All Dutch people are discontent
2) All German people are discontent

When any of them is true it is a model

Diagram 1: The semantic and the syntactic views of theories.

In addition to the relationship of theories, models and real systems presented above, there are other few characteristics of the semantic view. As it was already suggested above, theories, under this view, are extralinguistic structures. They are abstract and detached from the phenomena in their domains. It is in this way that it can accommodate the different ways in which theories can be represented and which allows for the partiality of models. That is, that models themselves do not have to accommodate an entire theory, but rather that various models, in different representations can account for one single theory. Likewise, models can be incomplete in the sense that they need not to account for every aspect they represent. That is, a model can include a certain aspect that corresponds to reality and yet need not to account for describing or explaining that aspect. It may be attempting to describe or explain another second aspect that needs the former in order to provide a satisfactory explanation or description.

Another aspect of the possibility of incompleteness of models is their use of abstraction and idealisation. As Craver (2002) points out, theories (and their models) are usually abstract and/or idealised. Abstract in the sense that they only make use of certain relevant aspects of a real system and assume that others—despite being part of the real system—are negligible for the behaviour of the system being described or explained. Models are idealised in the sense that in some circumstances the real system is represented as possessing different characteristics—ideal—from the ones it actually has, like point masses, frictionless planes, or no air resistance in falling bodies.

Having established some 'core' aspects of what the semantic view is about and making more explicit the advantages that it has over the RV to accommodate scientific theories, their relationship with models and reality, plus the way in which scientific practice *per se* proceeds, I will now turn to elaborate in the recent discussion about models, and how they are used to represent reality, with particular emphasis on the views of Giere (2004, 2006), Mäki (2009, 2011) and Teller (2001). Although these commentators do not share exactly the same view about models, there are some aspects that I consider compatible with each other, and that are useful for analysing the type of problem that we have here, namely the criticisms made by economists to the economics discipline regarding the crisis that started in late 2007.

2. Models, their use in science, and how they are used to represent reality

As I have suggested above, models play a prominent role in the scientific endeavour. They are one of the most important instruments of modern science available to acquire insights about all kinds of phenomena. In economics, in particular, the use of models has a rather long history. Mäki (2009, 2011) points to von Thünen as the deviser of the world's first economic model with his contribution in 1826 on agricultural land use in the Idealised Isolated State. Morgan (2006) on her part indicates how it was in the interwar period that the effective use of the term 'model' began. Specifically, it was Jan Tinbergen who introduced it in 1935 in the attempt to put together the statistics, as direct descriptions of the underlying economic relations that the econometricians of the 1920s believed could be uncovered with statistical manipulation, with the mathematical representation of theories. Yet, effective and explicit study and understanding of what models are, what function they serve, and, in general, what their ontological and methodological statuses are has seldom been discussed in economics[17].

Therefore, it is the purpose of this section to provide a discussion on the current understanding of models in the philosophy of science. In particular, I will address the discussion on how models are used to represent reality. As I mentioned above, this discussion is part of the current understanding of the model-based approach, or semantic view of theories. The reason why the discussion of theories has translated to models is precisely because of the emphasis given to models in science. The way science and theories proceed is much more through the use of models than theories themselves, but it is still a matter of discussion how it is that models represent reality. With the syntactic view of theories, as it was explained above, the vehicles of representation were theories themselves because they were composed of theoretical postulates and correspondence rules. The latter were the link between axioms and the world. With the model-based approach, the problem of the relationship between scientific theories and the world is translated into how this new understanding of model-based scientific theories does the representation. Furthermore, although the semantic view of theories unshackles the burden of having to deal with language in order to analyse scientific theories, in some way it translates the problem of establishing what theories are to what models are. In other words, the question of what models are remains also a matter of debate. Philosophers have different views about what models are and how they are used to represent reality, among other reasons, because of the role models play in specific disciplines. That is, models seem to be playing different roles in the different sciences. In addition, even within the sciences it is not clear that models play exactly the same role in studying different phenomena. For instance, as I will discuss later, Mäki (2009, 2011) uses the example of the Idealised Isolated State to argue that models in economics,

[17] Among those who have worked on this topic are Hausman (1992), Gibbard & Varian (1978), Hands (2001), Mäki (2009), Morgan (1999, 2008), Sudgen, (2001). Indeed, the view that I will present here is similar to Hausman (1992).

although false in all their assumptions, remain truthful about the underlying mechanism being represented. However, the Idealised Isolated State of von Thünen, seems to be the only example available. I will thus provide a discussion that is itself not conclusive, but that both follows one of the lines of argument developed here, namely that *a priori* methodological proscriptive rules have been abandoned, and also leaves the discussion open for further developments.

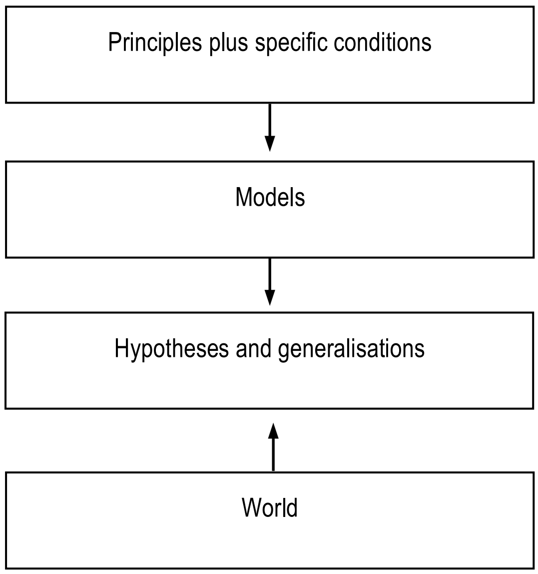
How are models used to represent reality?

The language of science, as any other language, is comprised of syntax, semantics and pragmatics. Giere (2004, 2006) argues that the pragmatics has seldom been investigated in particular because of a misconception of representation in science. Natural language has been suggested to be a cultural artefact: to learn a language means to learn to be part of a culture and therefore pertains to the domain of the pragmatics. Considering that this is the case for natural language, Giere (2004, 2006) considers that the language of science should be seen in this same way. Therefore, with a similar rationale, Giere (2004) argues that the scientific practice of representing the world is fundamentally pragmatic; the syntax and the semantics are just emergent characteristics of the use of a language. Thus, "if we wish to understand these practices, we should not begin with the language itself, but with the scientific practices in which the language is used" (Giere, 2004, p. 743). Making this shift leads us to discard the idea of *representation* held before and to embrace the idea of *representing,* as an activity carried out by scientists. Furthermore, the account leaves behind the idea of representation as a two-place relationship between linguistic entities and the world, as in the syntactic view, and instead considers a wider relationship between the agents carrying out the activity, the object used to represent, the target or the entity being represented, and the purposes that the agents have to do such representation. In synthesis, it is a relationship with the form (Giere, 2004):

$$S \text{ uses } X \text{ to represent } W \text{ for purposes } P$$

Where S refers to the scientist (or group, or scientific community), X is the object used to represent, and W is an aspect of the real world. Taking into account this four-placed relationship, instead of the twofold distinction suggested in the RV—where a theory T would represent an aspect of the world W—corresponds to focussing primarily on the scientific practice. In contrast with the RV where theories were seen in isolation, the role of the scientist and the purpose play a fundamental role in this view. In principle it could be argued that theories, given that they are the traditional medium of scientific representation, could be the objects being referred to as *X*. For Giere, however, if the focus is shifted towards the activity of representing and therefore the other relationships are considered, a model-based approach of theories is more suitable, given the prominent use of models in scientific practice.

On this approach models are derived from principles plus some specific conditions. From models, which are also abstract objects—as will be explained later—together with observations from the world, hypotheses and generalisations—about the world—are produced. The immediate consequence of this arrangement is that models themselves say nothing about the world, which means that they are not considered true or false[18], accurate or inaccurate. They are abstract objects that are not true of anything but them. Only hypotheses and generalisations generated from them and from observations of the world (see figure below, taken from Giere, 2004, p. 744) can be contrasted with the latter and are true or false.



Principles on this account do not have an easy definition. In science, principles are usually taken as universal and true generalisations. That is, as empirical laws that apply in every circumstance.

[18] One of the debates held in the philosophy of science is whether models can be considered to be true or false. Some commentators like Mäki (2011) argue that models can be truth bearers. Others, like Giere (2004, 2006) argue, for the reasons mentioned above, that models cannot be considered true or false. That discussion will not be considered in this thesis.

However, under close scrutiny, these universal and true generalisations have either exceptions or conditions in which they do not apply. Therefore, such definition is either vacuously true or else false (Cartwright, 1983). In order to be able to regard principles as meaningful, not vacuous statements, it is therefore necessary that they describe or refer to something in particular. Thus, although acknowledging that it is rather a trivial way, Giere (2004) points to principles as highly abstract objects that, by definition, exhibit all and only the characteristics described by the principles. Considering that this definition can be rather unsatisfactory, Giere (2004) opts instead to emphasise *the function* of principles, which is to act as general templates for the construction of more specific abstract objects, like models. So, principles refer to very highly abstract objects that serve as templates to construct more specified objects that we call models. In consequence, models in the sciences are "artful specifications of the very abstract models defined by the principles" (Giere, 2004, p. 747). In other words, models are more specified models than the ones defined by the principles. Their usefulness lies on their ability to be designed so that some elements in them can be identified with elements of the real world, in order to produce hypotheses. "This is what makes it possible to use models to *represent* aspects of the world" (Giere, 2004, p. 747).

But, how is that models are used to represent aspects of reality? In virtue of what a model is used to represent something? It is clear that there are many ways to represent something, but what are the representational styles that are accepted in the sciences? (Frigg & Hartmann, 2006). Critics of the model-based approach of theories have raised these same questions. For instance, Frigg (2006) argues that a theory of scientific representation has to come to terms with at least three conundrums. The first one, which he calls 'the ontological puzzle', refers to the nature of models. What are models? Even if I have tried above to account for what Giere defines as models, it is clear that, just like with the principles, the emphasis is given to their function as good candidates to be *used* for representation. The only attempted definition is in terms of the principles, as more concrete abstract objects. The second one, Frigg calls it the 'enigma of representation' and suggests that there needs to be a specification as to what counts as representation. "Models are representations of a selected part or aspect of the world (henceforth 'target system'). But in virtue of what is a model a representation of something else? Or to render the question more precisely, what fills the blank in '*M* is a scientific representation of *T* iff ___', where '*M*' stands for 'model' and '*T*' for 'target system'?" (Frigg, 2006, p. 2). The third and last conundrum Frigg calls it 'the problem of style'. With this problem Frigg calls attention to the idea that not all representations are of the same kind and therefore there are many different ways in which models can represent reality. He thus raises the question about the ways in which scientific models represent. This problem has a further caveat and it is the normative aspect of whether we can distinguish between acceptable and unacceptable styles of representation in science.

Frigg (2006) argues that there are many ways in which the above questions can be answered and certainly not clear whether any particular way can be considered adequate. Nonetheless, any theory of scientific representation has to be able to provide answers to these questions. There are, moreover, at least two aspects that Frigg considers that any acceptable theory of scientific representation should be able to satisfy: first, an account of the relationship between models and knowledge. That is, the answers given to the three conundrums must provide an account of how models provide knowledge. Since representation can serve several purposes, Frigg demands that the type of representation that a theory of scientific representation must provide, is one that advances our knowledge of reality, not one that pleases the eye, facilitates communication, is object of religious devotion or means of ideological identification (Frigg, 2006). The second aspect to satisfy is that it has to be able to explain the possibility of misrepresentation. "Any theory that makes the phenomenon of misrepresentation mysterious or impossible must be inadequate." (Frigg, 2006, p. 3). In the remainder of this chapter I will discuss possible answers these questions.

What are models?

As was mentioned earlier, even though Giere (2004, 2006) discusses what models are, namely that they are abstract objects, he rather emphasises the *use* that models can be given. Merely telling us that they are abstract objects can be considered misleading and even false. Therefore, in conjunction with the definition of principles, from which models are derived, this attempt to define them would indeed be subject to the ontological puzzle of Frigg. In Giere's view, however, models do not have an ontological definition. That is, models are identified by the function that they serve, instead of by some intrinsic characteristics that they possess. Therefore, in the form suggested above, the object to represent the world is actually a model: X is substituted for model,

M. Taking into account this definition based on the function that they serve, works to answer Frigg's demand. This definition of models by their functional role highlights two related points: first, that whatever is used to represent, can be regarded as a model. As Teller (2001) argues on his account of the model-based approach, "*WE* make something into a model by determining to use it to represent"(Teller, 2001, p. 397, emphasis in the original). The second point is the fundamental role that the scientist plays: it is the scientist who determines what a model is when he or she decides to use it to represent an aspect of the world. Teller (2001) argues that critics of the view demand sometimes an ontological account of models. But models cannot be given characteristics *a priori* because models are made once an object is put to the task of representing. "In great many cases models are abstract objects which have been pressed into the service of representations which work by various kinds of similarity, such as similarity of structure, possessed properties, or functional role" (p. 398). For example, in economics models are sets of equations, like the IS/LM model above; graphical representations, like the Marshallian supply and demand 'scissors'; and even physical apparatuses that simulate through hydraulics the workings of the economy, like the Phillips machine devised in the 1940s in the London School of Economics to represent the economy of the UK. Furthermore, Teller argues that it would be a mistake to make the account of models narrower, that is, specifying what can be regarded as a model and what cannot. He claims that science uses many different things as models, and therefore ruling out some of them would yield a wrong description of science. In addition, ruling out others in advance of possible novel candidates in the future would be "foolish *a priorism*".

Hence, it does not seem to be wrong to define models for the use they are given, for there are not any intrinsic characteristics that can make an object a model, but rather they being used to represent is what makes them a model. Although Frigg (2006) demands an ontological definition, he doesn't make it clear why the nature of models would need to be specified by contrast to a defining them for the use they can be given in representing. Thus, accepting that a model becomes a model once one decides to use it to represent, allows us to move on to the second conundrum proposed by Frigg (2006).

In virtue of what models represent reality?

According to Giere (2004) perhaps the most important way in which models are used to represent reality is "by exploiting *similarities* between a model and that aspect of the world it is being used to represent" (Giere, 2004, p. 747). But it is not because a model has similarities with the world that it can be used to represent; as Giere points out, two things can be similar in countless respects, but not anything represents something else. For example, solely by determining that the mug in which I pour my coffee at the office is similar in both shape and colour to my saltshaker at home, does not immediately warrant that one can represent the other. Therefore, *exploiting the similarities* between aspects of the model and the world is what allows the scientist to do the representation. In other words, "it is not the model that is doing the representing; it is the scientist using the model who is doing the representing" (p. 747). The model possesses certain features that the scientist considers are similar to the real system that he wants to represent. As such, an answer to Frigg's question "In virtue of what models represent reality?" would be that models per se do not represent reality but rather that the scientist *uses* the model to represent reality in virtue of the specific features that the model has that he considers are similar to the real system that he intends to represent. This answer, of course, can be considered very unsatisfactory, for two reasons: first, it is almost the same answer to the question of what are models. And second, because the question being asked is what is it that makes a model capable of representing something else and the answer being provided is that it is not models but rather the agent using a model by exploiting similarities what makes it representative. Frigg (2006) is very critical of this response. He makes two points about shifting the question to a mere appeal to intention. First, he argues that such appeal does not solve the answer: "what we have to understand is how a scientist comes to use *S* [M] as a representation of *T* [W] and to this end much more is needed than a blunt appeal to intention" (p. 7). And second, he argues that appealing to intentions makes similarity[19] become irrelevant given that intentions do all the work.

It is not difficult to understand this criticism of Frigg (2006) given that he is arguing for a theory of representation. That is, his idea is to establish whether this model-based view of theories can be considered as well as a theory of scientific representation. However, there are three points that are

[19] Strictly speaking, this is a point that Frigg (2006) makes against isomorphism—another way in which it has been argued that models represent reality—but later in the paper he argues that similarity does not fare better than isomorphism when understood as a response to the enigma of representation.

worth emphasising. First, as I already mentioned, it is easy to see how it can be *desirable* to have a full grasp of how models represent in order to establish a comprehensive theory of scientific representation. But Frigg (2006) doesn't tell us *why* it is the case that a theory of representation *has* to provide an answer to his conundrums, in order to be considered adequate or appropriate. Second, a feature of Giere's account is to emphasise the role that the scientist plays in determining how to use a model to represent an aspect of reality. In other words, as a response to Frigg's second criticism, it is not clear why the intentionality becoming so relevant would be a problem. On Giere's account, the scientist, the model and the purposes all play the same important role in representing an aspect of the world, as was explained above. This is expressed in his four-placed relationship. It is not just a matter of how some model M represents an aspect of the world W. And third, as I mentioned above, some of this issues are still a matter of discussion among philosophers, and it is not the purpose of this thesis to resolve these issues but rather to show how complex the debate is, instead of as straightforward as the critics of the economics discipline tacitly suggest.

Hence, if we take the answer given above as a valid *candidate* for an account of what is it that makes a model represent an aspect of the world, the obvious questions that follow are that, if two things can be similar in countless ways but not for this reason it is enough for some object, similar to another, to represent it, how do we know that the features in the model are similar to the real system? What counts as similar? Are there degrees of similarity? How do we know two objects are similar enough? And what's more, can two objects of distinct nature —abstract and concrete—be compared to decide their similarity? These questions are posed in the form of two criticisms that detractors of this view point out on the issue of similarity: first, they argue that any talk of similarity between an abstract object—a model—and a concrete object is unintelligible. They claim that a concrete object, say a paper cutout of a particular shape that we can see, touch, and measure, cannot be compared with the abstract shape. There is no way in which they can be put next to each other in order to compare them and determine how similar they are. To this criticism Teller (2001) responds by arguing that concrete objects possess properties, which are also abstract objects and that these are the ones that are ultimately compared. Thus, the comparison results between two abstract objects: the properties of the concrete object with the properties of the model. "One makes comparisons between the properties, for example the property of having three vertices, that a concrete object has and the properties that occur as parts or components of the representing model" (Teller, 2001, p. 399).

The second criticism, as mentioned above, is a demand to provide an account of what counts as similarity. In this regard, Brante (2010) argues that Giere does not provide a precise account of the relation of similarity between models and reality, and "appears content with vague, sensitising concepts". I think that Giere is clear enough in claiming that it is for the scientist to decide what actually counts as similarity. Furthermore, he is clear that it is only a matter of fit, or degree, because in the sciences there is not such thing as perfect precision between the models and their intended subjects.

> "Talk of similarity makes explicit the fact that one does not expect there to be a perfect fit of models and the world. It also leaves open what specific relationships among elements of the model are in question and what measure one will use to specify how closely these relationships track the corresponding relationships with the real system" (Giere, 2006, p. 66).

Likewise, Teller (2001) argues that a general account of similarity cannot be given because each particular case is what determines what is relevant to count as similar. Again, it is for the scientist to decide what counts as similar; this only depends on the purposes he has in order to use the model to represent a particular aspect of reality. Giere (2004) provides a nice example to illustrate this point: "it was particular similarities in physical structure that made possible Watson's use of his tin and cardboard model to represent the structure of DNA. He clearly was not saying that DNA is similar to his model with respect to being composed of tin and cardboard" (p.748). For Watson, the relevant similarity was the structure, not the material with which his model was built.

Mäki (2009, 2011) has a similar approach[20] to that of Giere (2004, 2006) although regarding similarity he goes much further in his account of models. To begin with, he does not refer to similarity but rather to resemblance. As I will explain later, resemblance is about how adequate a

[20] One of the differences between Mäki and Giere is the four-place relationship in the activity of representing reality through scientific models. For Mäki this is a six-place relationship, adding two components to Giere's, namely an audience E and a commentary C. In this thesis I will not discuss their differences and the implications of such differences.

model is to represent a target system, instead of about considering which aspects in the model are similar to the target. For Mäki (2009), models are *representations* of a target—that can be a real system, a set of data or a theory. The concept of representation has two aspects: a representative aspect and a resemblance aspect. The representative aspect can be characterised as models being surrogate systems. Mäki (2009) argues that models act as surrogate systems, which means that scientists are concerned *directly* with the properties of these surrogate systems in order to learn *indirectly* about the target. Note that, once again, with this sole aspect of representativeness, the question emerges as to how a surrogate system is similar to its target. For this reason, Mäki (2009) appeals to the aspect of resemblance. This aspect has two elements. First, he points to the importance of acknowledging that scientists use models to learn *directly* about their properties in order to learn *indirectly* about the target. "Models are built and studied because there is no epistemically reliable 'direct' access available to some deep facts of economic reality[21]" (Mäki, 2009, p. 12). To make this point clearer, he makes a distinction between models as surrogate and substitute systems. The difference between them is that if a model is treated as a substitute system, there is no attempt to provide a link with reality; the model is not treated as an instrument to gain indirect knowledge about something else, but rather the model itself is the ultimate interest. Leaving aside the models as substitute systems—assuming that they have no epistemic value to learn about reality[22]—Mäki calls attention to a second element, namely the concept of theoretical isolation by idealising assumptions.

Using the concept of theoretical isolation by idealising assumptions, Mäki tries to point out that it is typically considered more desirable to have a more accurate fit between the relevant aspects of the model and the corresponding aspects of the target. By contrast, he turns around the question and emphasises the differences between a model and a target, considering them rather beneficial. He stresses the significance of idealisations as strategic falsehoods, in order to *isolate* "some important dependency relation or causal factor or mechanism from the involvement and influence of the rest of the universe" (Mäki, 2009, p. 14). In other words, for Mäki the aspect of resemblance is not necessarily between particular components of the model and the target system.

[21] Mäki talks about deep facts referring to the underlying causal mechanisms that produce economic outcomes. These are not evident and are a matter to be discovered.
[22] This is not entirely correct because they can serve as test beds for developing concepts to be later applied to surrogates and thus valuable for ultimately learning about reality.

Rather, he argues that a model with fictional assumptions—where there is not necessarily any similarity between any component of the model with the target system—can be built in order to theoretically isolate an underlying connection between the target and the model, that is not explicit in any of the individual components of each.

In order to illustrate his point more clearly, Mäki (2009, 2011) considers his favourite example "the first economic model of the world", the model of agricultural land use in the Isolated State by von Thünen. In this model—although von Thünen himself does not call it a model—the author invites the reader to imagine:

> "[A] very large town—the Isolated State—at the centre of a fertile plain which is crossed by no navigable river or canal. Throughout the plain the soil is capable of cultivation and of the same fertility. Far from the town, the plain turns into an uncultivated wilderness which cuts off all communication between this State and the outside world. There are no other towns on the plain" (Von Thünen, 1966, p.7; quoted in Mäki, 2011, p. 50).

It is clear that these are all false assumptions: there is not such an isolated state in reality. Or to put it in different words, the differences between this description and any real town in the world are vast. Furthermore, there are additional assumptions such as transportation costs as a function of the distance from the city (neglecting the idea of the availability of roads, for instance), no trade with other towns, and agents living in this town who are strict rational agents, which are also very far from being true. The pattern of concentric rings that emerges out of this model, where the products that need to be carried to the market more promptly end up in the inner rings while the heavy, bulky, and non-perishable elements are left to be grown farthest from the town, in the outer rings, is also false. No such pattern has ever been observed in reality. Yet, the variations of this model continue to be "widely used in economic geography and geographical economics, in subfields such as location theory, urban economics, and regional science" (Mäki, 2011, p. 50). What could possibly be useful about this model if its results are as false as its assumptions? Or, to put it in terms of Giere's account, what could be useful about this model if there is not similarity neither in the elements used in the model nor in the results yielded by the model with the target system?

The point that Mäki is trying to make is that, despite its false assumptions and results, the model accounts for the underlying mechanism of land distribution. The model, with its strategic falsehoods, isolates the causal factor of land use that indeed is present in reality, namely, transportation costs. That is, like in the model, in reality transportation costs are a causal factor of land use distribution.

> "The function of such falsehoods is isolation by idealisation…Idealising assumptions serve the function of neutralising a number of causally relevant factors by eliminating them or their efficacy…By neutralising other subsidiary causes and conditions, they help isolate a major cause and its characteristic way of operation" (Mäki, 2011, p. 51).

As I mentioned above, it is perhaps the case that the Idealised Isolated State is the only example where both assumptions and results are false and still something truthful, like the causal factor behind land use distribution is observed. Therefore, this extreme case as a defence of idealisation goes perhaps too far. In this respect, Cartwright (1999) comments that although she has defended economics from using unrealistic assumptions in economic models, she argues that some of these assumptions might be totally misleading in learning something about the world by using them.

> "[C]riticising economic models for using unrealistic assumptions is like criticising Galileo's rolling ball experiments for using a plane honed to be as frictionless as possible. This defence of economic modelling has a bite, however. On the one hand, it makes clear why some kinds of unrealistic assumptions will do; but on the other, it highlights how totally misleading other kinds can be – and these other kinds of assumptions are ones that may be hard to avoid given the nature of contemporary economic theory" (Cartwright, 1999, p. 1).

In particular, Cartwright (1999) argues that contemporary economic models sometimes make use of certain assumptions that go beyond of what would be required for a Galilean experiment. That is, the assumptions made are not used only as a means to isolate the causal factor under study, but to provide a certain structure for the model to hold. Thus, she argues that the results of the model end up depending on this detailed structure—based on unrealistic assumptions 'of the wrong kind'—that has been given to the model, hampering the possibility to learn anything about the real world. In other words, given that these models are constructed and developed deductively, knowing exactly how the model works and what it does, they are deductively accurate, but they hardly provide any insight about the real world. When some idealisations are appropriate or 'of the right kind' and when they are not, or how to distinguish when a model can be useful to learn something about reality is perhaps something that cannot be established *a priori*. Nevertheless, what is important to underline is that idealisations can at least in some cases be helpful in building models that help us to learn things about the world. Cartwright (1999) however warns us about the existence of those of the 'wrong kind'. Despite this warning, and having considered that idealisations at least in some cases might be useful, it is therefore not straightforward at all that for appropriate model building models should be both 'internally and externally consistent' as the critics of economics suggest.

What is clear, nevertheless, is that for Mäki the aspect of resemblance goes further than for Giere: while for the latter similarity is a matter of degree between elements in the model and the target system, for the former resemblance is a matter of a model being *adequate* to represent the target system. "*Resemblance* is a further relationship between the surrogate system and the target system dealing with how adequately the model functions as a representative." (Mäki, 2011, p. 55; emphasis in the original).

There is one aspect, however, that is very important for both views regardless of their differences: the purposes of the agent. Thus far, although I commented in the first chapter that the purpose that each model has does matter, I have not discussed explicitly the role that they play in the four-placed relationship. For both Giere and Mäki, purposes are what determine the degree of similarity or resemblance needed. For Giere—as for Teller—it is the type of problem that the scientist faces that determines the model needed. This means that depending on the purpose at hand, some elements will need to be similar to the target while others need not and this is solely for the agent using the model to determine. Thus, a philosophical account of similarity cannot be provided *a priori*, for the different circumstances determine what is relevantly similar. Teller (2001) argues not only that an account of similarity cannot be provided *a priori* but also that there is no need for such an account "because the detail of any case will provide the information which will establish just what should count as relevant similarity in that case. There is no general problem of similarity, just many specific problems, and no general reason why any of the specific problems need be intractable" (Teller, 2001, p. 402). Likewise, Mäki (2009) points out that "whether an assumption is duly or unduly unrealistic depends on its location in a theoretical structure and the functions it is designed or be able to serve" (p. 15) meaning that the unrealisticness of an assumption per se is unimportant. It can only be judged in accordance to the purpose it is meant to fulfil.

Styles of representation

The attentive reader might have already noticed that the third conundrum posed by Frigg (2006) has also been resolved in the previous section. Frigg demands an account of the ways in which science represents reality. By answering the previous question, intrinsically it has been suggested that one of the styles of representation in the sciences is that of similarity. There have been other approaches within the model-based view of theories, particularly in the case where models are said to be mathematical models. On this approach, the style of resemblance is isomorphism, which means that there is a one-to-one relationship between a model and a target system. A weaker one is that of (partial) isomorphism (Frigg & Hartmann, 2006). However, the approach of representation by similarity is much more flexible, in the sense that it is less restrictive and can account for inexact models and theories 'in the wild' as Craver (2002) calls them.

There is one last aspect that has yet to be discussed. Frigg (2006) argues that any theory of scientific representation should be able to satisfy two elements regardless of the way the three questions that he puts forward are answered: an account of the relationship between models and knowledge, namely how models are used to learn about the world; and second, an explanation of the possibility of misrepresentation. Regarding the first element, it is clear that the account that has been advanced here is one that has into consideration the access to knowledge. The use of models is intended to acquire knowledge about a target system, which can be a real entity, a set of data or a theory, all intended for gaining knowledge about the world. Nonetheless, it is important to underscore that, once again, the purposes are what determine the outcome. In other words, as long as the purposes that the agent has are related in any way to acquiring knowledge about the world, the approach provides an account of how the models are used to learn about the world. In regard to the second element, although it is not entirely clear what Frigg means with explaining the possibility of misrepresentation, in my opinion, misrepresentation could take two forms under this account. On the one hand, the fact that the only exact model of the world is the world itself. Therefore, in this respect a model will always misrepresent reality in some way or another because our cognitive capacities—at least for now—impede direct access to all the underlying mechanisms that take place in the world. In other words, models are our indirect and imperfect way to access knowledge about the world. This implies that sometimes we need to misrepresent some aspect of the world—by idealising assumptions, for instance—in order to represent another aspect of the world. On the other hand, misrepresentation can occur when a model turns out to be wrong about the intended target. More specifically, misrepresentation can occur both when a model is not intended for a particular purpose—for instance the models the critics are arguing about misrepresent the crisis because they were not aimed to represent it—or when a model fails to represent a target even when it was meant to do so. It is always possible that a model, regardless of the degree of similarity—in Giere's sense—or its adequacy—in Mäki's sense—can be wrong about target system. But this, again, is something to be determined by the scientist, according to the purposes for which the model is being used.

By now, the importance of the four-place relationship offered by the shift of focus to the model-based view of theories, should be evident. The two-place relationship between a theory and the world ignores two fundamental elements of the scientific practice: the scientist—or group of scientists, or scientific community—and the purposes. These two elements, as was discussed above, determine to a great extent those final products that we call models. Perhaps, as a non-postmodern view of science the answers given are rather unsatisfactory: in many ways it can be considered a defence for relativism in which everything is decided by the scientist, depending on the purposes that she has. Perhaps answering the question "How is it that models represent reality?" with "By exploiting the similarities that the scientist considers most appropriate given the purposes that the scientist has" does not take us very far. Nevertheless, what I want to underscore is that the view of similarity, as I already pointed out, first, accommodates indeed the way in which many models in science are used to represent (as I will show in Chapter III with one example) and, second, is only one of the ways in which models are thought to represent reality. Other approaches deal with isomorphism or partial structures. Even if the view of similarity can in some cases be considered unsatisfactory, it is important to note that it is a concept that accommodates in many more respects the actual way of proceeding of scientific practice, as opposed to the syntactic view. Thus, it is in this respect that similarity is important: it is a concept that is flexible enough to accommodate scientific practice, allowing the roles of the scientist and the purposes to be considered in the way models represent reality.

Taking into account the criticisms made by economists about the assumptions of the representative agent and the rational expectations, regardless of whether similarity remains unsatisfactory as a philosophical account of how models represent, what is clear is that there are much more aspects to be considered about models, than the ones the critics do. Given that it is far from straightforward to determine how models should be used, it is at least more fruitful to suspend judgement when it comes to establishing general rules. In other words, we don't have certainty whether there is one, or two, or many particular ways in which using and building models can guarantee success. Hence, remaining as open as possible about how understanding can be achieved, taking into account the history of scientific discovery is perhaps the best thing we can do.

In sum, what I have tried to highlight in this chapter is that realisticness is not necessary to represent some aspect of the world, as the critics of economics demand of economic models. They have argued that there are some assumptions in economics, particularly the representative agent and the rational expectations, that do not correspond to what we observe and thus yield their use in models problematic. The implication is that the use of these assumptions corresponds to a flawed methodology. I have discussed how the concept of similarity, together with the purposes for which a model is used to represent, can be a good account of how scientific models can be used to represent. I have also discussed how this representation can be achieved by the use of idealisations and false assumptions. Although this account of how models are used to represent is itself not unproblematic, and as I commented there is still much debate among philosophers of science whether this is a satisfactory account, it is sufficient to highlight that not being realistic is not a sufficient reason to regard certain assumptions as problematic or despicable.

CHAPTER III:
Taking models for what they are.

What have we learnt from the two previous chapters? In chapter II I argued that criticising the models with an *a priori* view of what good science is can be associated with a modernist attitude, following McCloskey, which resembles the view of science held by the RV. That is, a preconceived idea of what good science is, or how good science is made, and a view in which theories as sentences are the only vehicles of representation. This implies that models, which are ubiquitous in science in general, and particularly in economics, as well as the scientist and the purposes, are neglected in their role they play in scientific representation. More importantly, I presented an account of how models can be used to represent reality. Under this view, the scientist and the purposes play a prominent role in determining, jointly with the concept of similarity, how models can be used to represent reality. I also presented a related view in which the use of idealisations can be seen as a way to isolate an underlying connection between the target system and the model that is not explicit in any of the individual components of the target or the model. In this case it is no longer a matter of similarity but rather of resemblance, as proposed by Mäki (2009). Although this particular case can be considered more an extreme case rather than a typical one, given that the Idealised Isolated State is the only example that has been provided[23], it still highlights the role that idealisations can play, namely to neutralise causally relevant factors in order to isolate a major cause and its way to operate. In this sense, it stresses that the differences between a model and a target system can also be valuable in representing an aspect of the world. The sole aim need not be to attempt similarity between our models and a target system.

Taking into account the previous description of how models represent reality, and going back to the criticisms posed by some economists of the economics discipline presented in chapter I, it becomes clear that the criticisms as posed say very little about the failures they aim to uncover. In relation to the use of the assumptions of the representative agent and the rational expectations,

[23] (Mäki (2009, 2011) is the proponent of this view and so far, this is the only example he provides, which he calls 'his favourite'.

for which the allegations are that the former obeys a strange form of reductionism and that the latter is not externally consistent, it could immediately be argued that not only it is impossible to have a model that is exact to the world—for that would be the world itself—but also that there need not be neither a particular form of accepted reductionism nor the external consistency that the critics demand. Under the account of similarity, it can be said that the models using these assumptions can be similar in some respects to the real world in a way that the agents using these models have considered appropriate according to their purposes. Likewise, it could also be said that these assumptions are idealisations that for their very nature of being idealisations, need not resemble reality. More specifically, the account of how models can be used to represent suggests two things: on the one hand, that a model is conceived once an agent decides to use it to represent some aspect of the world. That act of representation by means of the model occurs by exploiting the similarities between the model and the target system, that the scientist considers appropriate, given certain purposes. This not only means that the model need not attempt to imitate every aspect of the target system that is being represented, but also that there are particular features, which the scientists decides, to be similar to the target system. On the other, that by using idealisations, certain causal factors are neutralised, in order to represent a different causal mechanism and its way to operate in the world.

In consequence, in the following sections I will examine the use of the assumptions criticised in the two ways suggested by the account of representation: By exploiting the similarities between the target system and the model, and by using the assumptions as idealisations. After discovering that indeed there seems to be a conceptual difficulty with the use of the representative agent as an idealisation in a perfect competition setting (section I), I discuss how the representative agent assumption is used successfully—particularly in providing a plausible explanation and having some empirical success—in a model by Kydland & Prescott (1982) about business cycles (section II). This results in having a successful model at the expense of having a conceptual difficulty within the model. In the final section I suggest how the issue can be resolved.

1. The representative agent as idealisation

In his paper, Kevin Hoover (2010) examines the use of the representative agent strategy as an idealisation. Being critical of the microfoundations project himself[24], and aware of the multiple challenges that have been posed to the microfoundations of macroeconomics by him and other authors like Kirman (1992) or Janssen (1993), Hoover (2010) attempts to give the use of the representative agent strategy a chance, by examining whether it works as an idealisation.

For Hoover (2010), reduction—by the use of the representative agent, which he describes as the most popular way of implementing reduction from macroeconomics to microeconomics—can be defended as an idealisation if it can connect the micro with the macro by clear idealising steps that can later be progressively relaxed in order to improve the idealisation. He proceeds by studying a model included in a graduate textbook of macroeconomics by Blanchard & Fischer (1989) that attempts to explicate how the effects of changes in aggregate demand on employment and output can be explained by nominal rigidities, by using a model of price setting in monopolistic competition. Although this is not a representative agent model, Hoover argues that to the extent that models grounded in individual agents are successful and do not contradict the results of the representative agent models, "they constitute both successful microfoundations and a defence of the representative-agent model" (Hoover, 2010, p. 337). After his examination, Hoover concludes that the reduction that takes place in the microfoundations is less than ideal, meaning that it cannot be defended as an idealisation. In particular, although he admits that there are some idealising steps in the reduction that are consistent with achieving the isolation of important causal factors, there are two steps that generate conceptual problems. Although the concerns that Hoover (2010) raises about these two particular steps are legitimate concerns, I don't find the first one as problematic as the second. In other words, while I think that it is not so clear in which way the first one should raise concern about the microfoundations in general, the second one indeed seems to jeopardise the use of the representative agent as an idealisation. I will discuss this later, after having presented Hoover's (2010) view.

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[24] See, for example, Hoover (2004, Ch.3).

Idealisation in what terms?

In order to examine whether the use of the representative agent works as an idealisation, Hoover (2010) first explicates what he means by idealisation. Or, in other words, the terms in which he assesses the reduction from microeconomics to macroeconomics. In order to do this, Hoover (2010) distinguishes between two accounts of idealisation: Nowakian idealisations and substantive idealisations. Nowakian idealisations come attached to a particular way of viewing theories. In the Nowakian sense, a theory, which is a formal structure, is idealised when some elements of that theory are set to limiting values, ceasing to contribute to the explanatory machinery of the theory. Theories are therefore divided into elements of secondary importance, which are the ones set to limiting values, and of primary importance, being the ones that fulfil the explanatory role. If the secondary elements that were once idealised are set piecemeal back to their 'original' or 'real' values, then the theory is said to go through a process of concretisation. When the idealising assumptions have all been concretised, then the full theory is recovered.

Substantive idealisations on the other hand, are very similar to the account of idealisation that I gave in the previous chapter that Mäki (2009) defends. They isolate causal mechanisms from other causal elements in order to exhibit their operation without the latter causal elements. As Hoover (2010) points out, Nowakian idealisation is a matter of theoretical structure, of distinguishing the primary from the secondary elements. By contrast, substantive idealisations are determined with concrete situations, like experiments, or with models. In other words, substantive idealisation corresponds to establishing an approximation to a particular target.

The reduction

The basic model described by Blanchard & Fischer ([1989] 1993) used by Hoover (2010) is an economy in which there is a number *n* of goods, where all are imperfect substitutes—meaning there is monopolistic competition—and money. Each good is produced by one producer, who chooses the nominal price of the product and the level of production, given the demand function that she faces. Each producer is also a consumer, who derives utility from consumption of goods and the services of real money balances. Demand functions depend therefore on relative prices and initial real money balances.

The individual preferences of producers and consumers are captured in the utility function with the following form:

$$U_i = \left(\frac{c_i}{g}\right)^g \left(\frac{M_i/P}{1-g}\right)^{1-g} - \left(\frac{d}{ß}\right) Y_i \qquad (1)$$

where i = 1, 2, …, n is the number of individual[25] consumers/producers; M_i are money holdings of the ith individual; Y_i is the output of good i produced by the ith individual; C_i is consumption, which is defined by the following function, which aggregates the consumption of individual goods by an individual i:

$$C_i = n^{1/(1-\theta)}\left(\sum_{j=1}^{n} C_{ij}{}^{(\theta-1)/\theta}\right)^{\theta/(\theta-1)} \qquad (2)$$

where C_{ij} is the consumption of good j by individual i; and P is the general price level, defined as the weighing of the prices of individual goods (P_i):

$$P = \left(\frac{1}{n}\sum_{i=1}^{n} P_i{}^{1-\theta}\right)^{\frac{1}{1-\theta}} \qquad (3)$$

Finally, agents maximise their utility function (1) subject to the following budget constraint:

$$\sum_{j=1}^{n} P_j C_{ij} + M_i = P_i Y_i + \bar{M}_i = I_i \qquad (4)$$

where $\bar{M}_i$ is the initial endowment of money for the ith agent. The solution to this problem is obtained by deriving demands and supplies for each good and for money holdings for each individual—demand for goods and money holdings for consumers and supplies for producers—all of which are a function of the relative prices of goods.

According to Hoover (2010), the solution to this problem can be interpreted in two ways: first, as a general equilibrium solution in which all agents are taken into account, where a set of prices is found that determines an equilibrium between supply and demand. Given that all consumers have been assumed to be identical and producers assumed to produce with the same production

[25] In what follows, unless stated otherwise, when I refer to an individual or agent I am referring to an individual consumer/producer.

technology, the price in equilibrium is the same for all of them and the derivation of the model for every agent is based on symmetry. Second, as an aggregate solution in which various macroeconomic aggregates are derived from the individual solutions obtained in the general equilibrium solution. It is in this way that reduction takes place, for the macroeconomic description of the model has been derived from the microeconomic general equilibrium solution: "Each of the macroeconomic variables is an aggregation of the microeconomic variables, and the functional forms of the macroeconomic relations are determined by the functional forms and parameters of the microeconomic relationships" (Hoover, 2010, p. 342).

Hoover (2010) points to this reduction being made in steps: first, by using the idealisation that goods are assumed to be identical in production technology. Although they are assumed to be imperfect substitutes, their production technology is assumed to be identical, leaving the differences between the goods to other dimensions different from production technology. Hoover (2010) uses the example of the beer industry, in which the assumption means that two different companies use the same brewing technology and face the same production costs but use different recipes to differentiate their products. The second step is by defining a single utility function by appealing to the idealisation that producers are also consumers and therefore maximise the same utility function. Aside from that, Hoover (2010) points out that, for instance, the functional forms of the equations, or setting the number of producers, consumers, and goods to the same number *n* are "pedagogical tricks" that make the model more tractable and do not seem to raise particular problems to the microfoundations[26].

Reduction doesn't work as idealisation either

Hoover (2010) points out that although there are a number of idealising steps that are consistent with the concept of achieving substantial idealisation, there are two conceptual difficulties in the microfoundational reduction. The first one is related to the idealisation mentioned above in the second step of the reduction process, which is basically about avoiding the coordination problem. Hoover finds problematic the idealisation of giving a single agent the double role of being a

[26] Given that Hoover (2010) does not consider these 'pedagogical tricks' to be relevant for appraising the project of microfoundations, I prefer not to mention them for the simplicity of the argument. For a detailed discussion of the functional forms and the roles of the parameters see Blanchard & Fischer, [1989] 1993, Ch. 8.

consumer and at the same time a producer—integrating the choices of producers and consumers in a single utility function. The reason for this, Hoover (2010) argues, is that in none of the possible cases that the idealisation could take if it were relaxed, the coordination problems that can emerge between agents is possible, despite such possibility being a live one. As I pointed out above, for Hoover (2010) reduction can be defended as an idealisation if it can be made by clear idealising steps that can be relaxed later. In consequence, in the idealisation made by Blanchard & Fischer (1989) of collapsing the decisions of producers and consumers into a single utility function, although the possible conflicts between agents are eliminated, how the coordination actually occurs, or, in other words, how prices are set, is obscured with this idealisation: "Rather than isolating the essence of the coordinating problem, the microreduction dissolves it with a particular assumption that amounts to assuming it away" (Hoover, 2010, p. 344).

According to Hoover (2010), using this form of reduction impedes that coordination problems arise, even at the most disaggregated level in which producers and consumers are all separate agents. To see this, he first explores what can happen at, say, both extremes of the idealisation: on the one hand, where there is only one representative agent, and on the other where all consumers and producers are different agents. In the first case, simply adopting the representative agent, one single agent in the economy who is both a producer and consumer, the coordination problem does not arise, but is discarded by Hoover as uninteresting and subject to the criticisms made to the representative agent already mentioned. In the second case, in which it is assumed that every agent is different to each other, each agent is a price taker—given that they are each too small relative to the market—and a way has to be found in which their choices are made mutually consistent in order to reach equilibrium. That is, they each face a set of common prices that has to be adjusted until equilibrium is reached, where there are no excess demands or supplies. However, in this case, it is not specified how a set of prices is set if every agent is processing the information separately: "[t]he explicitly modelled agents set quantities in response to market prices, but who sets market prices? And on the basis of what knowledge?" (Hoover, 2010, p. 343).

Hoover (2010) points out that theorists in economics have two approaches to this issue, but none of which is satisfactory. The first one is by focussing only on equilibrium states without a real concern for how such price setting process takes place. In other words, this approach consists of simply ignoring the problem and asserting that the real concern is in the actual equilibrium states rather than in the process of getting to such equilibriums. Hoover (2010) argues that such a strategy does not work because even the proofs of the existence of equilibrium in a general equilibrium setting rely on fixed-point theorems, in which a process of 'trial and error' occurs until equilibrium is reached. With fixed-point theorems a fixed point is found only when a mapping between a set of prices and excess demands at those prices does not propose a new set of prices—because the excess demands are zero—after a process of proposing new sets of prices that would reduce excess demands. Therefore, "[t]he mathematics demonstrates that far from economising on information, something in the economy must do the work of the mapping and process prices in response to *all* of the excess demands" (Hoover, 2010, p. 343). In other words, according to Hoover (2010) the process of price setting cannot simply be abstracted, for even the mathematical proof relies on the process of price setting to determine whether an equilibrium exists.

A second approach taken by theorists is to attribute the process of price setting to an auctioneer. Although in some sense the strategy of the 'auctioneer' suggests an actual centralised figure who announces prices, calculates excess demands at the announced prices and recalculates prices to reduce individual excess demands until they are all zero, in other sense it is rather understood as a way to resemble the behaviour of the economy *as if* there were such a central figure. As I already pointed out, even when the possibility of lack of coordination between individual agents exists, this strategy rules it out, for the assumption that the economy behaves *as if* such auctioneer were there implies that coordination does take place. In this respect, Hoover (2010) argues that the auctioneer is not an idealisation of the exchange process but rather, "…a particular, and particularly unhelpful, concretisation, which suggests falsely that the best analogue to a decentralised economy is a command economy in which information is processed centrally" (p. 343).

Having established that the idealisation does not work on the extreme cases, namely as a representative agent and as separate individual maximising agents, Hoover (2010) points out that for the particular case of monopolistic competition, it does not work either. He illustrates this by recurring to the way in which the equilibrium value of the aggregated price level is derived from the definition of the price level and the relative price that each individual consumer/producer chooses in equilibrium:

$$\frac{P_i}{P} = \left(\left[\frac{\partial\theta}{\theta-1}\right]\left[\left(\frac{g}{(1-g)n}\right)\frac{\bar{M}}{P}\right]^{ß-1}\right)^{\frac{1}{1+\theta(ß-1)}} \qquad (5)$$

Regarding this particular solution—recall this is the optimal relative price level that each individual chooses that maximises her utility function—Hoover (2010) brings to the fore two considerations: first, that the simplicity of the solution, given that most of it depends on parameters, is deceptive. The only reason for which such a solution is as simple as it is, is because it has been assumed that all individuals are identical and therefore one single solution applies to all of them by symmetry. If every agent were considered different from the rest, instead of assuming that they are identical, the parameters of every single agent would be different and would have to appear in this solution. Second, that a solution in terms solely of individual concepts cannot be obtained because the application of the price level each individual chooses in equilibrium inevitably requires knowledge of a general price level *P*, which is a macroeconomic quantity.

Taking into account these two points, namely that one of the equations in the solution to the individual model is much simpler than it should, and that there is no way in which appealing to macroeconomic quantities can be avoided, Hoover (2010) concludes that "[r]ather than isolating the essence of the coordination problem, the microreduction dissolves it with a particular assumption that amounts to assuming it away" (p. 344). For him, the reduction based on monopolistic competition puts each producer/consumer in a position that is analogous to that of the auctioneer, given that in order to set a set of prices that maximises their utility, each of them needs to be aware of the general price level. In other words, while it is assumed that information is processed individually, at the same time the solution requires an appeal to a macroeconomic construct that presupposes the knowledge that only the auctioneer—a centralised figure with all information available—could have. Therefore, "any objection to the auctioneer in the setting of perfect competition must be multiplied by *n* in the setting of monopolistic competition" (p. 344).

As I mentioned above, although this is a legitimate claim, in the sense that when it comes to explicating how coordination takes place, none of the possible scenarios—a disaggregated perfectly competitive extreme, where consumers and producers are all separate agents; a single representative agent; or a monopolistic competition scenario—can provide an account of how such process takes place, there are at least five points that should be considered before establishing that idealisation cannot be defended, based on these grounds.

First, Hoover's argument relies on a single view of idealisation, following Nowak, which takes idealisation as selecting particular elements of a theory and setting them to an extreme value. This means that for Hoover the problem of coordination is not an appropriate idealisation because the element "coordination" is not an element which values are set to particular extremes—like, say, perfect coordination—in different cases. Instead, making the particular assumption that coordination problems do not arise solves the problem of coordination, either by openly ignoring the problem, by arguing that interest lies in equilibrium *per se* and not how equilibrium is obtained, or by appealing to the role of the auctioneer. Under his view of idealisation, Hoover's (2010) claim would be legitimate, at least to a certain extent. As I commented above, Nowak's view of idealisation takes theories to have primary and secondary elements. The latter are the ones that, when set to extreme values, are considered idealised. A theory is considered 'complete' when all of its elements are set to their real values, or, there are no idealisations in the theory. This implies, in my opinion, that theories are considered to have a one-to-one relation with reality when they are 'complete'. This is a very particular view of theories and their relation with reality, that, although I will not comment on, the discussion of how models are used to represent reality in Chapter II should make evident, at least, that this view of how theories are related to the world is not the only view and that it can be contested from different directions. Leaving aside such discussion, although it is not clear in the particular case of the aggregation problem what would count as the theory, and what as the primary and secondary elements, it is clear that in the particular case of the aggregation problem, there is not a degree of coordination that is being established by setting a particular element to a certain value. Instead, it is simply being assumed that coordination takes place. So, although it can be argued that under his view of idealisation his claim is legitimate, Hoover (2010) disregards any other view in which idealisation does not consist of setting determinate elements to an extreme value. In this respect, for instance, in his account of how false models can contribute to truer theories, Wimsatt (2007) argues that there are several ways in which a model can be false, and yet be useful. In particular, he introduces seven[27] ways in which a model can be false, of which he considers two the most productive cases of falsity, and other two that can sometimes be able to produce useful insights[28]. One of those he considers to be the most productive cases of falsity is particularly relevant to the case at hand, given that it provides an alternative view of idealisation that does not involve setting extreme values, but rather considers how an aspect can be an approximation of an element found in nature. Under this view of idealisation, a model can be considered an idealisation if there are some cases in which the model can be applied as an approximation, despite the conditions not being ever found in nature:

> (2) A model may be an *idealisation* whose conditions of applicability are never found in nature, (e.g., point masses, the uses of continuous variables for population sizes, etc.), but which has a range of cases to which it may be more or less accurately applied as an approximation. (Wimsatt, 2007, pp. 101-2, emphasis in the original).

In this sense, although the auctioneer is never found in a competitive economy, it can be regarded as an approximation of the process that goes on in price setting. The approximation, it should be noted, is not, in my view, about being proximate only to a particular value in the range of possible values that an element can take—say, whether to assume the values of planetary diameters as zero or the actual 'real' number in a theory of planetary motion governed by Newton's laws. Approximation can also be considered in other dimensions, like a conceptual dimension, in which it is about introducing a concept or characterising a particular feature in a way that is not observed in nature. A good example is the concept of rationality used in economics, to regard individuals as acting in the best way they can in order to accomplish a particular goal, given some binding constraints. This idealisation—which Hoover calls the Fundamental Idealisation of Economics—is not about setting a value to an extreme, like attributing extremely high IQs to people, but rather about introducing a characterisation of a type of behaviour considered reasonable to attribute it to human beings. Like Hoover (2010) acknowledges, Karl Popper named such an idealisation "situational logic" or "situational

[27] Even though I will not present or discuss all the 7 ways, I will maintain the numbers assigned to each of them in the original text.
[28] Wimsatt not only argues that they are useful but categorises four different kinds of uses that can be given to false models. I will not go into the details here. See Wimsatt (2007, Ch. 6).

analysis", meaning that a situation and a goal were characterised, in order to make predictions about how people will make their best in order to achieve a goal in a particular situation. Similarly, as I already mentioned, although the auctioneer is never found in nature, it can be defended as a characterisation of the way in which price setting occurs. In this sense, the second approach taken by theorists, in which they argue that the economy behaves *as if* there were such an auctioneer reflects exactly this view of idealisation as an approximation. Deciding in what cases it can be used as such, as I have been arguing throughout this thesis, depends upon the purposes for which the idealisation—and the model—are being used. This is related to the second point that follows.

Another way in which models can be false—though useful—according to Wimsatt (2006) is by being incomplete. That is, by leaving out one or more causally relevant variables.

> (3) A model may be *incomplete*—leaving out 1 or more causally relevant variables. (Here it is assumed that the included variables are causally relevant, and are so in at least roughly the manner described.) (pp. 101-2)

This means that not every causally relevant variable, like coordination in this case, would need to be included in the model as relevant. But, not including does not mean that coordination does not take place, but rather that how it comes about is considered relevant for a particular purpose. Therefore, it is assumed that it simply happens. This idea is related to the second point that I think should be considered regarding Hoover's conclusion. As I already pointed out, Hoover (2010) argues that the problem of how coordination takes place is not solved, but rather assumed away. However, he does not tell us why not solving the problem of coordination in the model represents a problem for the model of Blanchard & Fischer. In other words, just as he admits in the discussion of formal idealisation, that the "distinction between primary and secondary factors must clearly be understood *relative to the desired target of explanation*" (Hoover, 2010, p. 335; my emphasis), it is not clear why coordination would need to be explained in the model if it is not relevant for the purpose that Blanchard & Fischer (1989) have. Blanchard & Fischer's model, as they suggest in the introduction of the chapter in which the model is presented, is about finding "reasons for nominal rigidities and their role in the transmission mechanism of aggregate demand movements on output" (Blanchard & Fischer, [1989]1993, p. 374). In particular, they are interested in the costs of changing prices, as a source of nominal rigidity. Therefore, they study two scenarios: one, in which producers set prices. This means that producers have monopoly power and therefore the equilibrium reached is inefficient. In this scenario, an external shock in the supply of money has no effects on aggregate demand, as in perfect competition. In the second scenario, by contrast, it is assumed that individuals face costs in changing prices. Therefore they may decide not to change prices in response to small shifts in aggregate demand. In consequence, Blanchard & Fischer find out that movements in the money supply may lead to movements in output. What they find more interesting, however, is that the welfare effects of output movements are likely to be much larger than the costs of changing prices that each individual faces. In other words, each producer individually may not be so much affected by deciding to leave the price unchanged, but the aggregate effects can be large. Given that the authors are trying to compare these two scenarios, or more specifically, to explain how costs in price setting, a nominal rigidity, as derived from a rational individual decision, affects aggregate demand, it is not clear why they would need to explain how coordination between agents actually takes place.

Another way of looking at the purposes, aside from the one just mentioned above about the phenomenon itself—how a nominal rigidity affects aggregate demand—is about the *type* of claim being put forward. I have argued that Blanchard & Fischer's purpose was to *explain* a particular phenomenon. Nevertheless, a little bit more precision regarding what to *explain* means is perhaps necessary insofar as the type of claim can also determine the role that assumptions can be given—or not—within a model. Let us call this the epistemic purpose. In this respect, depending on the epistemic purpose, it could be the case that Blanchard & Fischer's model should not be incomplete regarding the coordination problem and Hoover could be right with his claim.

So, what are these epistemic purposes? There are at least three different purposes that philosophers have attributed to models in the literature. The first is that models are used to provide evidence for the truth of a causal hypothesis. In this case, the model is like a Galilean experiment (See Cartwright, 1999) and works exactly as the 'isolation by idealisation' concept defended by Mäki (2009, 2011) that I already discussed. Here the model isolates the single cause from the disturbing factors; despite the model containing many false assumptions, a true causal mechanism is represented. A second purpose of models is to establish a possibility claim. This idea has been discussed by Reiss (2008) and later by Grüne-Yanoff (2009) and states that models provide possible hypotheses of how a phenomenon could have come about. In more common parlance, models offer *possible* explanations[29]. Lastly, in the same spirit that models provide us with elements in the context of discovery—rather than of justification—is a view defended by Anna Alexandrova (2007) where models are regarded as 'open formulae' that help us discover causal hypotheses[30]. Here models are not explanatory but rather "function as frameworks for formulating hypotheses" (Alexandrova, 2008, p. 396). This means that models play an important role in formulating hypotheses but they don't fully specify them.

If Blanchard & Fischer were attempting to provide evidence for the truth of a causal hypothesis, we would have to agree that although their model uses false assumptions, the causal mechanism isolated by the model would be true. This would imply that there is at least some *prima facie* evidence that suggests that indeed coordination is not a substantial element of the causal mechanism being isolated. Although we do have reasons to believe that market imperfections can cause nominal rigidities and that these in turn can cause aggregate fluctuations, it cannot be ascertained that the coordination problem is merely a disturbing factor. This, in my opinion, implies that although Hoover (2010) could not argue that coordination is a necessary element of the mechanism, it cannot be ascertained either that it is not. But, is this what Blanchard & Fischer are trying to establish? No. In fact, they are rather providing a *possible* explanation for aggregate fluctuations as a cause of nominal rigidities caused by market imperfections where price setting occurs. At the beginning of the chapter where they introduce the model, they argue that the model they are about to present is an example of the different explorations of the different market imperfections that result out of optimising behaviour. As it was already mentioned, previous Keynesian models assumed that wages and prices were sticky and that therefore quantities fluctuated in response to variations in aggregate demand. However, the stickiness of wages and prices was simply assumed and often the results obtained depended on the different rationing rules assigned to the economy ad hoc. As a response, a new research strategy emerged:

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[29] Reiss (2008) argues, more precisely, that models provide only *prima facie*, not sound, evidence, and therefore require further empirical work in order to make valid inferences from them.

[30] Alexandrova (2007) argues about models as 'open formulae' referring specifically in the context of experimental and policy economics. Although such claim cannot be easily generalised to theoretical models, it is one possible purpose that models have.

> "Recent research has started from explicitly specified market imperfections and attempted to derive price or wage stickiness and other macroeconomic implications by examining optimal behaviour under such imperfections. A wealth of imperfections and thus of different and potentially conflicting explanations, have been explored and the current state of affairs is still one of exploration rather than synthesis" (Blanchard & Fischer, 1993, p. 373; footnote omitted, emphasis added).

Considering that Blanchard & Fischer are using their model as a potential, possible explanation, there is no reason, regarding the epistemic purposes, to demand the inclusion of a particular assumption. As they admit, there are even 'potentially conflicting' explanations. Thus, to establish a particular claim about the phenomenon being studied, further evidence would need to be provided.

The third point that needs to be considered is Hoover's claim about individuals imminently having to refer to a macroeconomic aggregate, the price level, and therefore not being able to avoid the macro-micro relation. This claim requires two points to be considered. In particular because what Hoover finds problematic is the attempt to derive macroeconomic entities from microeconomic ones if the macro ones cannot be dispensed with. First, the microfoundations, in spite of aiming to derive macroeconomic results from individual rational behaviour, does not amount to ignoring or neglecting the use of macroeconomic entities. That is, it is possible to commit to the attempt of deriving aggregate results from individual behaviour and at the same time recognise the independent character of macroeconomic concepts. Second, not even the strongest sense in which Hoover (2010) argues that microfoundations is taken by theorists, disregards the use of macroeconomic entities as theoretically useful. At the beginning of his paper, Hoover (2010) argues that there are at least three distinct theses regarding what the microfoundations are about. The first one is that, "individuals lie behind aggregates in the sense that without individuals there would be no aggregates" (p. 330). This one he considers an uncontroversial weak ontological claim. The second one is, "How individuals behave affects or conditions how aggregates behave" (p. 330). He argues that it merely suggests that individual behaviour be examined in order to get insights about aggregates, and that even Keynes seemed to adhere to such a statement. Finally, thesis three, which he argues that is the one most economists nowadays adhere to, states, "aggregates are nothing else but summary statistics reflecting individual behaviour" (p. 331). Hoover (2010) then argues that this thesis amounts to the following: "When the microeconomic properties are taken into proper account, there simply are no residual ontologically distinct or explanatorily efficacious macroeconomic properties" (p. 331). This means that macroeconomic properties do not exist, or do not need to, and that theoretically they are redundant and therefore 'inefficacious', if a thorough understanding of individual behaviour is available. In other words, he argues that thesis three amounts to discarding both the existential and theoretical possibility of macroeconomic entities. However, this is not what thesis three states. At most, what this thesis says is that macroeconomic aggregates are a construct made of microeconomic entities, but it does not say anything about its theoretical efficacy or usefulness.

Fourth, the argument that Hoover (2010) provides about assuming away the coordination problem is a criticism that has long been made to general equilibrium theory since Walras, who attempted a solution using the concept of *tâtonnement* or groping[31]. The solution to the problem via the assumption of the auctioneer, although not stated by Walras himself, was probably initiated by Schumpeter and Samuelson, based on Walras' *Elements of Pure Economics*[32] (Watson, 2005). The coordination problem, as suggested above, is about finding what makes a market economy. Or, in other words, about "how a market economy of autonomous individuals can function smoothly in the absence of a central authority to coordinate economic activity" (Watson, 2005, p. 143). As such, this problem is still much unsolved because so far an economic explanation of how this process takes place has not been provided. Indeed, according to Watson (2005), Walras' failed account can be considered as the most successful of such attempts; a reason that draws Watson (2005) to believe that perhaps the coordination problem is 'fundamentally irresolvable' solely in terms of pure economics[33]. Therefore, the point that I want to make is that this is not a problem exclusive of the microfoundations but rather of economics in general.

[31] Although there is not an exact translation of *tâtonnement* into English, groping has been the word that has been adopted in the English translations. Nevertheless, in great many cases commentators continue to refer to *tâtonnement*.
[32] The treatment given by Walras to the role of *tâtonnement* varies significantly between the first three editions of the *Elements* and the fourth one. While in the first three editions he treats *tâtonnement* as a dynamic process allowing transactions to take place in disequilibrium, eventually reaching equilibrium, in the fourth one this is a process that takes place at one single moment. Given these differences, the role of the auctioneer in the two interpretations varies.
[33] Instead, he argues that the conditions under which market institutions function properly are more than purely economical and are related, as Adam Smith suggested, to the constitution of the individual as a moral being.

Therefore, it would have to be established first whether this is a fundamental failure of economics, say, not to be able to explain what is it that actually makes a market, or if it is more a matter of having guided research interests in a different direction, neglecting some questions at the expense of others. According to Colander, et al. (2009), as was previously discussed, the coordination problem is one that has been neglected in economics that requires attention in order to be able to have a more thorough understanding of economic crises. But, whether it is, as I mentioned, fundamental for economics as a discipline, is both beyond the scope of this thesis, and up to the detractors to show how it is that failing to explain coordination yields economics 'a dismal science'.

In addition, Hoover (2010) expresses this problem in terms of how price setting takes place. He argues that the assumption of the auctioneer rules out the possibility of not reaching equilibrium even in perfect competition, where the possibility that coordination between agents doesn't happen is a live one. Given that individuals have to refer to the aggregate price in order to make their optimal choice of price in the monopolistic setting, Hoover (2010) argues that every individual in the model needs to have the qualities of the auctioneer. Therefore microeconomic reduction is implausible. Although, as I already pointed out, this is a problem for general equilibrium—and economics—for the assumption is made that equilibrium arises with a set of prices that 'is simply there'. In practical terms, this means that individuals would need to be aware of a general price level out of the whole economy in order to make their decisions, knowing exactly the price of each good, and the number of goods produced, in order to make a weighed average—remember the general price level as stated in equation (3) is defined as the weighing of the prices of individual goods (P_i). Although for an entire economy this might be quite implausible, for a monopolistic competition model, might not be the same. Assuming the role of the producer, if we go back to the example of the beer industry, it is not unlikely that Heineken, as competitor or Leffe or Jupiler (or more exactly Anheuser-Busch InBev, the brewer company) has knowledge of the prices of their beers[34]. The point is that although in a complete general equilibrium setting such an assumption is implausible, it is not the same case for monopolistic

[34] The example of beer is particularly interesting, given that only one company, Anheuser-Busch InBev—the biggest brewer worldwide—has a portfolio of more than 200 brands and which, with the other 3 biggest brewers, has a global market share of nearly 50%. They produce both Jupiler and Leffe (as well as Hoegaarden, Stella Artois, Budweiser, Beck's, among at least other 193)(Reuters, 2010).

competition, for it is quite likely that very close competitors keep track not only of their competitor's prices, but also of their marketing campaigns, etc. Hence, in the particular case of the model by Blanchard & Fischer (1989), this is yet another reason why the claim of Hoover (2010) that the coordination problem yields a problem for the microfoundations, has to be qualified.

Finally, again regarding the general price level in equation (3), Hoover (2010) argues that application of equation (5), requires the knowledge of equation (3), which is, as I already mentioned, constructed as the weighing of individual prices of goods and, "a rather odd aggregating function that bears no obvious relationships to any of the standard formulae for calculating price indices used, for example, by national statistics bureaux" (p. 344). Even though it is clear that Hoover (2010) is more concerned about individuals having to refer to an aggregate price level, it is not clear at all why the form of the function not being related to the standard formulae used by national statistic agencies would be a problem. In fact, the construction of indices by national statistics agencies is a process that has to take many variables into consideration, and where values, or subjective judgements, play an important role. Reiss (2008), referring particularly to the construction of the Consumer Price Index in the United States, has argued that the Index could be constructed using different formulae, which would in turn give a different estimation of what actual inflation was. Using some formula inflation would likely be underrepresented, while using another would be overrepresented, depending on the target population for which purchasing power should remain constant. In this sense, therefore, it is not clear why not having a relationship with standard formulae becomes a problem for the microfoundations.

As I mentioned above, Hoover (2010) points to two conceptual difficulties that yield reduction as an idealisation less than ideal, or improper. I argued so far that the first conceptual difficulty, namely assuming away a coordination problem does not necessarily imply that the representative agent is an inappropriate idealisation. The second difficulty is related to an inconsistency in the assumptions made in the model. In particular, this difficulty is about an inconsistency in aggregation. In perfect competition, to assume that individuals are relatively small compared to the market implying that they are price takers, is to assume that $n \to \infty$, that is, that the number of agents tends to infinity. According to Hoover (2010), this is a properly formulated Nowakian idealisation, which in practice, may prove to be a useful substantive idealisation. However, in the assumption of the representative agent, an *improper* idealisation is introduced, because on the one hand it is being assumed that $n \to 1$, or that there is only one individual—the representative agent—but on the other hand, at the same time, it is assumed that $n \to \infty$, because it is assumed that the representative agent follows the rule of perfect competition, or price taking. Therefore, to when it is assumed that there is a representative agent who is also a price taker, there is an inconsistency in the idealisation being made. As Hoover (2010) puts it, "the representative agent is—inconsistently—simultaneously the whole market and small relative to the market." (p. 345). This inconsistency is not an external inconsistency, in the sense in which Colander, et al. (2009) suggested, namely that assumptions in the model do not match empirical findings, but it is an *internal* inconsistency. This means that this is no longer an issue about how a false model, in the sense of having some assumptions that are not real, can be used to represent reality, but rather about an inconsistent model representing reality. A model that within its own domain, in its own workings, possesses elements that contradict each other. Although for Hoover (2010) a model that makes use of these improper idealisations is not 'scientifically useful', it should be remarked that this case is not unique. As Bailer-Jones (2003) has discussed, there are models like the Bohr model of the atom, that makes contradictory assumptions with regard to the electrons. According to the model, electrons are assumed to move around the nucleus on a circular orbit, while they also maintain their energy level, an assumption that is false, because if they were to move in a circular orbit, they should be experiencing acceleration. As a result of the circular orbits, electrons should be losing energy and gradually spiral towards the nucleus. Thus,

> "this proposition is in disagreement either with the principle of energy conservation or with the facts about the motion of the electrons. Bohr's model of the atom entails some propositions that are unquestionably true, e.g. those regarding the charge distribution in the atom, but others can hardly be considered as true as long as they contradict other (more accepted) principles or facts" (Bailer-Jones, 2003, p. 68).

Therefore, the Bohr model is an example that shows that contradictory assumptions are not exclusive of economics, and that not for this reason models with this sort of assumptions can be considered despicable or not useful. That is, possessing contradictory assumptions does not seem to be conclusive of the uselessness of a model. Likewise, as I will show in the following section, despite the use of such an internal inconsistency, the model of Kydland & Prescott (1982) can be considered successful in some respects.

2. Kydland & Prescott (1982)

In spite of just having discussed how the representative agent fails as an idealisation if perfect competition is also assumed, there is an example that, making use of the just mentioned inconsistent idealisation, achieved not only a plausible alternative explanation of the occurrence of business cycles but also significant empirical results, which fostered research interests in that direction. The example is the paper 'Time to Build and Aggregate Fluctuations' by Kydland & Prescott (1982).

As I mentioned in the first chapter, the Keynesian tradition was in its heyday during the post-war period. As a result, macroeconomic policy was mostly based on this tradition, and stabilisation policy was guided mainly by the trade-off that allegedly existed between inflation and unemployment, suggested by the Phillips curve. Although the Phillips curve itself was not a theoretical development of the Keynesian tradition—it was an empirical finding—it was used in the Keynesian tradition, given that it did not have a good theoretical ground to explain inflation. In the 1970s, the rational expectations revolution, particularly the 'Lucas Critique' was very important to support the claim that stabilisation policy based on macroeconometric models was inappropriate. Aside from this theoretical inconsistency, there was also a practical issue that contributed to the demise of Keynesian macroeconomics: stagflation (Mankiw, The Macroeconomist as Scientist and Engineer, 2006). The events of the 1970s, namely the colossal increase in oil prices, disrupted the previous inverse relationship that used to be observed between unemployment and inflation, given that what was observed was high inflation with decreased productivity and high unemployment. Consequently, the previous idea that output fluctuations were mainly driven by changes in aggregate demand was also called into question. The experience of the 70s suggested that these output fluctuations were the result of a shock in supply.

Kydland and Prescott (1982) explored this idea, following Lucas in that macroeconomic aggregates should be derived from a microeconomic structure that could be reliable, as consumer's preferences are likely to maintain stable, in contrast to postulated aggregate relationships. In the end, "they showed that technology shocks, i.e., short run variations around the positive growth trend for technology that makes economies grow in the long run, could be an important cause of output fluctuations" (Bank of Sweden, 2004, p. 14). In particular, they suggested that if it was assumed that capital took more than one period to be built and become effective, technology, which is believed to make economies grow in the long run, could also explain short run aggregate fluctuations. As I emphasised already in chapter two and earlier in this chapter in the discussion of reduction as idealisation, purposes are a fundamental element in a model in their use to represent. According to the purposes, some features need to be similar to reality, while others can be assumed or idealised away. In other words, purposes matter to distinguish the more important elements from the less important ones. In the case of Kydland & Prescott (1982), as the title of the paper suggests, they were interested in the idea that capital goods take time to be built. By contrast to the common assumption used in growth models that investment technologies are a single period production, they argued that, "a thesis of this essay is that the assumption of multiple-period construction is crucial for explaining aggregate fluctuations" (Kydland & Prescott, 1982, p. 1345). In synthesis, they proposed a possible explanation for business cycle fluctuations that focussed on technology shocks that affect aggregate supply, assuming that capital takes more than one period to be effective.

A few characteristics of the model

Keeping into account this purpose, Kydland & Prescott (1982) set up a model with the following characteristics (Kydland & Prescott, 1982; Bank of Sweden, 2004)[35]:

a) The model is a dynamic stochastic general equilibrium (DSGE) model.

   The model is dynamic in the sense that it takes into account the evolution of time; stochastic, in the sense that the economy is subject to random shocks, for example in technology and imperfect indicators of productivity; and general equilibrium meaning that feedback effects are taken into account.

b) Expectations are rational. This means that the unbiased prediction of the future evolution of prices is an element present in the optimising behaviour of individuals.

[35] The model itself is complex in several dimensions: from its construction to its estimation. In consequence, I will present only some general features and not the preferences of agents or the detailed production technology of firms.

c) The model considers one consumption good. This assumption, although typically used in microeconomic models were markets for particular goods are studied (as opposed to macroeconomic models that should capture the economy as a whole), can be seen, using Hoover's (2010) approach, as a Nowakian idealisation where the number of products approaches a limit at 1. It can be seen as secondary element (in terms of importance) given that what is much more relevant in this case, is that consumers value not only consumption but also leisure—as fluctuations in employment are central to the business cycle. In fact, what is more important regarding preferences is a "non-time-separable utility function that admits greater intertemporal substitution of leisure—something which is needed to explain aggregate movements in employment in an equilibrium model" (Kydland & Prescott, 1982, p. 1351). Besides, Kydland & Prescott (1982) argue that household productivity theory and cross-sectional evidence support the use of such utility function.
d) There is one *type*—though infinite in number—of an infinitely lived consumer. This is a standard assumption in growth theory. Although this is quite an implausible assumption, it is important to note that the model is a growth model, which means that it is meant to capture the long run. Therefore, it is not necessary to capture that there are many different individuals with different tastes who eventually die. What is important in this model is that agents are homogeneous in their valuation of leisure and consumption. It can be interpreted as a dynasty in which the sequence of parents and children has altruistic preferences with regard to their offspring.
e) There is one type of production technology. As it was already mentioned, the most important feature in production in this model is that time is required to build new productive capital. Kydland & Prescott (1982) argue that neither the neoclassical nor the adjustment cost technologies—two basic technologies that were commonly used in empirical studies of aggregate investment behaviour—are adequate.
f) There is perfect competition: markets have no frictions and therefore any equilibrium is Pareto optimal.

What did the model achieve?

With a model with the previous characteristics, Kydland & Prescott (1982) accomplished at least three things[36] according to the Bank of Sweden (2004): first, they proposed a possible explanation[37] of business cycles fluctuations. This explanation suggested that, "technology growth might be an important determinant, not only of long-term living standards, but also of short-term fluctuations, to the extent that technology growth displays variations over time" (Bank of Sweden, 2004, p. 14). The way in which a technology shock would transform into output fluctuations was through an impulse of a temporary technology shock that would have a propagation mechanism, shaping the path of future macroeconomic variables. In particular, a positive technology shock in, say, period *t*, would imply two different effects, one direct and another indirect. First, a direct effect is that the growth rate in total factor productivity becomes higher than the average. Having higher-than-average total factor productivity means that each factor used in the productive process is more productive than usual. For example, suppose a textile factory in which in order to produce a certain fabric, two workers are needed to operate one machine that produces 1 square meter of fabric per hour. With a new technology, that same machine is replaced for another that only requires one operator in the machine and now produces 2 sq. m of fabric. This means that now one worker produces 2 sq. m of fabric per hour, instead of ½; and one machine produces 2 sq. m of fabric per hour, instead of 1. The second indirect effect is that when there is higher productivity, wages raise. Workers observe that wages have increased and they have a bigger incentive to work, instead of having more leisure, given that the latter becomes more expensive in terms of opportunity cost. More labour supply translates into increased output in period *t*. In addition, there is a third effect, but contingent on whether the

[36] There are actually some methodological contributions that are considered quite significant, like solving the model with the use of numerical analysis to characterise the equilibrium (due to its complexity, compared to other models attempted before), or the method of calibration in the empirical approach of the model. Nonetheless, I will not use them for my argument because they are considered significant for the type of research that they fostered, and I am interested only in what the model itself achieved. In addition, part of this legacy in the method is one of the criticisms made to contemporary macroeconomics, and to analyse whether those contributions have been positive or negative is another matter, different from the matter I am studying here, which is the use of certain assumptions, like the representative agent or the rational expectations.
[37] Kydland & Prescott (1982) refer sometimes to a 'theory' instead of using the term that I have used, 'possible explanation'. Nevertheless, a possible explanation is what they seem to have in mind when they talk about a theory. Prescott, in an interview published in Snowdon & Vane (2005) argues that models are not what are criticised, but rather theories, referring to the explanations produced by the models.

technology shock is anticipated. If such is the case, there is an increase in investment in previous periods—given that the shock was anticipated—which translates in an increase in the return of capital. This, as well, raises output. Furthermore, there are dynamic consequences of the raise in output in period *t*. Depending on the consumer preferences and the expected durability of the shock, the increase in growth is either consumed or invested. And this, in turn, depends on the preferences for consumption smoothing. Kydland & Prescott (1982) used actual data to measure technology growth—using Solow's residual—that present high positive autocorrelation. This means that as the technology shock is above average, investment is also high, producing a raise in the capital stock in $t+1$. Thus, incentives for the supply of labour are likely to be higher than average, and—if the increase in the capital stock is large and the technology growth is sufficiently autocorrelated—labour supply in period $t+1$ and investment will be more above the trend than in *t*. The effects of the impulse eventually die out, given that the technology growth process is mean reverting and the decreasing returns to capital bring investment back to trend. In a similar way, recessions are caused by lower than average technology growth. In this way, business cycles are explained.

The second achievement of the model of Kydland & Prescott (1982) is that it showed that many qualitative features of business cycles—like the co-movements of macroeconomic variables—could be generated via technology shocks. In other words, their model, which was based on supply technology shocks, could generate the co-movements of macroeconomic variables that are typically observed in business cycles. For instance, consumption, investment, the capital stock and employment fit the 'stylised facts' that have been established for business cycles. That is, throughout the time that business cycles have been studied, it has been established that certain variables move in the same direction of output, called procyclical, while others move in the opposite direction, called anticyclical. And, despite the controversy that exists about what causes the aggregate fluctuations in the economic activity, there is reasonable agreement about the basic business cycles facts (Snowdon & Vane, 2005).

The third achievement of this model is that it could generate significant cycles quantitatively. According to Kydland & Prescott (1982) the test of the model was in whether a set of parameters could be found that first, did not contradict relevant microeconomic observations, and second, for which there were macroeconomic variables that quantitatively fit the observed data of the U.S.

economy: "The test of the theory is whether there is a set of parameters for which the model's co-movements for *both* the smoothed series and the deviations from the smoothed series are *quantitatively* consistent with the observed behaviour of the corresponding series for the U.S. post-war economy" (Kydland & Prescott, 1982, p. 1359). That is, aside from accurately describing the direction of the movements in some of the macroeconomic variables, there also had to be 'good fit' between the variables in the model and the sample values for the U.S. economy between 1950 and 1979, which is the data with which the authors worked, also to determine the component of growth in technology, using the Solow residual. In this respect, the authors report that "the model is consistent with the large (percentage) variability in investment and low variability in consumption and their high correlations with real output. The model's negative correlation between the capital stock and output is consistent with the data though its magnitude is somewhat smaller" (Kydland & Prescott, 1982, p. 1364). Furthermore, according to the Bank of Sweden (2004), "calibrated with parameters from microeconomic studies and simulated with impulses from an estimated technology growth process, Kydland and Prescott's baseline model generates output fluctuations that amount to around 70 per cent of those observed in postwar U.S. data" (Bank of Sweden, 2004, p. 16). In addition, the authors report (see table below, taken from Kydland & Prescott, 1982, p. 1364), that the fit between the estimated autocorrelations of real output and the sample values of the U.S. economy is very good.

TABLE II

AUTOCORRELATIONS OF OUTPUT[a]

Order of Autocorrelations	Model Means (Standard Deviations) of Sample Distribution	U.S. Economy Sample Values for 1950 : 1–1979 : 2
1	.71 (.07)	.84
2	.45 (.12)	.57
3	.28 (.13)	.27
4	.19 (.12)	−.01
5	.02 (.11)	−.20
6	−.13 (.12)	−.30

[a] The length of the sample period both for the model and for the U.S. economy is 118 quarters.

Taking into account that for Kydland and Prescott (1982) the test of the model was to observe fit between the model and both the output smoothed series and the deviations from the smoothed series, regarding the smoothed series they comment that the model is also consistent with the data:

> "The smoothed output series for the U.S. post-war data deviated significantly from the linear time trend. During the 118-quarter sample period this difference had two peaks and two troughs. The times between such local extremes were 30, 31, and 32 quarters, and the corresponding differences in values at adjacent extremes were 5.00, 7.25, and 5.90 per cent, respectively. These observations match well with the predictions of the model. The mean of the model's sampling distribution for he number of peaks and troughs in a 118-quarter period is 4.0—which is precisely the number observed. The mean of the number of quarters between extremes is 26.1 with standard deviation 9.7, and the mean of the vertical difference in the values at adjacent extremes is 5.0 with standard deviation 2.9. Thus, the smoothed output series for the U.S. economy is also consistent with the model" (Kydland & Prescott, 1982, p. 1366).

As it was presented in the features of the model above, it is clear that it makes use of the inappropriate idealisation that Hoover (2010) highlights, namely the inconsistency that emerges when it is assumed that there is a representative agent who is also a price taker. As I already mentioned, this amounts to assuming that the number of agents in the economy approaches a limit at one—therefore being a representative agent who maximises a utility function—and simultaneously assuming that this agent is a price taker, for this implies that instead, the number of agents approaches a limit in infinity. Yet, despite the use of such an inappropriate idealisation, I also indicated how the model was reported as successful both in providing a plausible explanation of business cycle formation based on technology shocks that affect aggregate supply, and in having a reasonable degree of empirical fit that could explain the aggregate fluctuations in the U.S. economy in the period 1950-1979.

Going back to Hoover's concept of substantive idealisation, as I already mentioned, he argues that substantive idealisation is a form of approximation. But, he considers a substantive idealisation successful if, as an approximation, the model has some kind of empirical success: "A substantively idealised model is successful if it fits the data or predicts, or meets some other measure of differential empirical success, well enough within some acceptable limits of approximation, provided that its success is traceable to—that is, *depends essentially on*—the model. (Hoover, 2010, pp. 336, emphasis on the original).

Regarding the notion of dependence, Hoover (2010) suggests that although it is vague, for there are variations in the relationships between variables that are indistinguishable, it is necessary in an account of substantive idealisation. In particular, Hoover (2010) means by dependence that the fit between the model and the data would not have been achieved had there been an alternative characterisation of the model. Even though assessing whether the results of Kydland & Prescott depend on the precise setup of the model is beyond the scope of this thesis, there are two elements that suggest such dependence: first, the importance that time-to-build has in the model. That is, the assumption that capital takes more than one period to become actually productive. Or, as Kydland & Prescott (1982) put it, "half ships and factories are not part of the productive capital stock" (p. 1345). In this respect, the authors make a special remark about how their results are sensitive to the specification of investment technology: "Even when the adjustment cost [an alternative to time-to-build] is of this small magnitude, the covariance properties of the model are grossly inconsistent with the U.S. data for the post-war period" (Kydland & Prescott, 1982, p. 1367). And second, the model that they use is robust to changes in the values of the parameters: "with a couple of exceptions, the results were surprisingly insensitive to the values of the parameters" (Kydland & Prescott, 1982, p. 1366). Although there are discussions in philosophy about the extent to which robustness can provide confirmation to a model, it is still considered useful by scientists and to have epistemic value[38]. This insensitivity to changes in the values of the parameters, furthermore, leads Kydland & Prescott (1982) to believe that their finding could be not only particular to the U.S. economy, but rather a characterisation of business cycles in general: "The fact that the covariations of the aggregate variables in the model are quite similar for broad ranges of many of the parameters suggests that, even though the parameters may differ across economies, the nature of business cycles can still be quite similar" (Kydland & Prescott, 1982, p. 1366). Thus, the model seems to be quite successful: aside from the possible explanation that it offers and its empirical success, it seems to be in accordance to what Hoover (2010) demands from models that act as approximations to a certain phenomenon.

It has to be accepted, however, that this and, in general, real business cycle models have received a lot of criticism, in particular regarding their empirical testing. Hoover (1995), for instance, has

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[38] Such a topic is beyond the scope of this thesis. For a discussion see Kuorikoski, Lehtinen, & Marchionni (2010) and Odenbaugh & Alexandrova (2011).

put into question the method of calibration, arguing that "although calibration is consistent with appealing accounts of the nature and role of models in science and economics, of their quantification and idealisation, its practical implementation in the service of real-business-cycle models with representative agents is less than compelling" (Hoover, 1995, p. 40). Furthermore, he argues that "the calibration methodology, to date, lacks any discipline as stern as that imposed by econometric methods" (p. 41) as a response to the belief—and the defence—of calibration as a "simple but informative form of estimation" (Bank of Sweden, 2004, p. 14). With regard to the empirical results of the models, Eichenbaum (1991) has argued that the evidence is "too fragile to be believable" (quoted in Snowdon & Vane, 2005, p. 336). Nonetheless, despite these arguments, there are commentators that suggest that in the context of evaluating models like the one of Kydland & Prescott (1982), given the simplicity of the model—compared to the fact that it was a dynamic stochastic general equilibrium model, completely microfounded—the value of formal econometric test results to establish whether the model fits the data adequately, is rather limited.

> "It can be argued that the value of formal econometric test results is small in the present context, i.e., in indicating whether models like that of Kydland & Prescott (1982) do a good job of matching the actual data. *Any* model that is both manageable and theoretically coherent will necessarily be too simple to closely match the data in all respects, so with the number of quarterly observations available such models will inevitably be rejected at low significance levels in formal tests against generalised alternatives. (McCallum, 1988, p. 27; emphasis in the original).

Although McCallum admits that it is unclear that the models (Kydland & Prescott's and other variations) provide good data matches according to the model builders criteria, it is important to highlight that McCallum's remark suggests, at least, that econometric methods that disprove the model's empirical success cannot be considered decisive in determining the success of the model. In this respect, it would be important to consider that our methods to establish empirical success need not necessarily be decisive and much less infallible.

In sum, the model yields a possible explanation of business cycles, describing how an impulse, like a technology shock, would have a propagation mechanism affecting output, and provides empirical results that, though contested, the authors consider quite remarkable, given the simplicity of the model. As I discussed above, however, the model makes use of an improper idealisation by possessing elements that contradict each other. But, as I also discussed above, this is not unique of this model; other models in the sciences, like the Bohr model of the atom, also has contradictory assumptions. Should we agree with Hoover that a model that uses contradictory assumptions is not scientifically useful? Does this mean that the model and its findings should be discarded altogether? Or, does this mean perhaps that we could ignore the inconsistency because empirical results, though contested, support the success of the model? What can we thus infer from these two contradictory facts about the model?

3. Taking models for what they are

In the face of this dilemma, there are four possible directions that could be taken. First, taking the position that in any way, regardless of the results that the model delivers, an internal inconsistency cannot be considered an appropriate scientific model. This would lead, of course, to discarding the model on the basis that it uses inconsistent assumptions. In turn this would imply that it would remain unexplained why, despite the inconsistency, the model could still be considered successful according to the purpose of the model. Leaving aside the improper idealisation for a moment, if the purpose of the model is taken into account, which was to determine whether the use of time-to-build investment technology could account for aggregate fluctuations, it can be argued that the model is successful. As I mentioned already, it not only provides a possible explanation of how business cycles can come about, but also has a reasonable degree of empirical success.

Perhaps a very critical reader could argue that the example of Kydland & Prescott is the worst I could have chosen to argue for the success of the rational expectations-representative agent models. In this respect, there are two points that I would like to emphasise. First, my argument should not be taken as one defending the rational expectations-representative agent models. My point is rather that such assumptions, depending on the purposes that they are given can prove to be useful. Second, I have selected such an example, precisely for its controversy, to show that very controverted examples, despite what can be considered their failures, can provide us with something useful. In the particular case of the model of Kydland & Prescott—and also the one discussed above by Blanchard & Fischer—they both have provided us with possible explanations of how some phenomenon that we observe in the economy might have come about. It might not be much, but at least it can be argued that the literature they have fostered, by offering possible explanations, is colossal. This means that at least, by providing some hints about some phenomenon, more investigation in that same way has been fostered. In my opinion that is already a huge accomplishment and a strong reason why this first direction of discarding the models, should not be taken.

Second, the opposite direction could be taken by ignoring the inconsistency in the idealisation, and relying solely on the success of the model with regard to the purpose that it had. This would imply that whatever the effects of using the model with an internal inconsistency would be ignored. If such were the case and the model used as a basis to support a particular policy, it would be problematic.

A third way could be to find a concept that would guide us in deciding which of the two previous routes, either internal consistency or empirical success, should be favoured. That is, to find a reason that would, on epistemic grounds, justify why either of the previous routes should be taken. This would mean that based on some additional concept, we could decide why we could take the first or the second route. Although I think that such a concept cannot be found in order to determine *a priori* whether internal consistency or empirical success should be favoured, it is still possible that such a concept could be found, and therefore the possibility should remain open.

Finally, the fourth route, which I favour, would be to take the models for what they are. That means that the model, though helpful and successful in some respects, is also *only* a representation of the world, which means that it is not the world itself, but rather an abstract, separate construct, that mimics some aspect of the world, in a very particular way. This means, in other words, that models should not be taken for granted; they work as heuristic devices that help us think about particular phenomena and give us reasons to favour or disfavour a particular idea about the workings of economic phenomena. However, they don't give us straight answers about how a particular phenomenon comes about. In the case of Kydland & Prescott (1982), their model suggests how output fluctuations could occur due to a technology shocks. Their possible explanation includes a mechanism through which the phenomenon comes about. Our confidence in such an explanation is increased, as their model is able to fit the data. Nevertheless, it does not tell us that this *is* the way in which business cycles occur. Indeed, business cycles nowadays are thought to be caused by both aggregate demand and supply.

Favouring this route, instead of the other three mentioned above, can be supported by resuming the discussion about how models can be used to represent reality. This view suggests that the similarities that can be exploited between a model and reality can be sufficient to make a successful representation of the world, depending on the purposes of such representation. But it also highlights the fact that having similarities between a model and its target does not amount to having an exact replica. Or, put in a different way, it does not obscure the fact that the differences that remain between the model and the world are also significant and therefore give us reasons to be wary about our models. In synthesis, having an inconsistency in a model is not necessary to be cautious about our models, for we have already sufficient reasons to be so, but the inconsistency is also not sufficient to consider our models or their assumptions despicable.

Robert Sugden (2011) has a similar view with regard to the value that possible explanations have. In his paper, although he does not argue directly about the value of possible explanations, he does find value in the possible explanations that models can provide. In particular when a model is not considered as a unique element that provides *one* 'outcome' but rather as part of a process of discovery that takes place with several models and researchers. More specifically, Sugden (2011) argues for what he calls explanations in search of observations. These are explanations provided by models for phenomena of which an instance cannot yet be found in nature, but that we have reason to believe it might be an explanation. According to Sugden (2011) there are two reasons why a model can be taken to provide an explanation: first, because the model itself, although an imaginary construct, is a credible world[39]. Second, because we can make an inductive inference from the model. By this Sugden means that the mechanism in the model produces certain patters

[39] Sugden has defended an account of models in economics as 'credible worlds' meaning that although they clearly possess false elements, and therefore are constructs not directly related to the actual world in which we live, they describe worlds that according to our knowledge, are credible, because they possess characteristics that could be true. For a discussion see Sugden (2001).

that are similar to those observed in the world. Given this similarity between the effects of the model and the effects in the world, we can infer that the causes are the same. That is, that the known causes of the model can be inferred to be the same causes in the real world. In this sense, the model offers an explanation about a phenomenon.

> "The model is a self-contained construct, which can be interpreted as a description of an imaginary but credible world. The workings of the model generate patterns in the model world that are similar to ones that can be observed in the real world. The model provides an explanation of the world by virtue of an inductive inference: roughly, from similarity of effects we infer a similarity of causes" (Sugden, 2011, p. 733).

Later, he argues that models that are related to those that we consider offer explanations about phenomena that we observe in the real world, can also offer explanations even if the phenomenon itself, has not been observed yet.

> "The model world exhibits a regularity, induced by a mechanism in that world. The modeller concludes that there *may be* a part of the real world in which a similar regularity occurs (or has occurred or will occur) and that, *if that were the case* the model would give some support to the hypothesis that, in that part of the world, a mechanism similar to that of the model is (or was, or will be) at work" (Sugden, 2011, p. 718).

I disagree with Sugden in that he seems to believe that a model possessing similarities—in particular in the outcome—with a phenomenon observed in the world is sufficient for the model to provide a potential explanation. Furthermore, in my opinion, he misinterprets Giere when he argues that, "as Giere's account of science implies, there is nothing more to scientific explanation than finding similarities between models and real-world phenomena". On the one hand, as I discussed thoroughly in Chapter II, Giere is arguing about how models are used to *represent* reality, not about scientific explanation. Although there is a relationship between representation and explanation, they are not the same thing. One can represent one thing with another without necessarily intending to explain something about it, like in the case of a painting, say, Picasso's *Guernica*. Although the *Guernica* represents the bombing of this town in the Basque Country, it does not necessarily explain something. On the other hand, as I also discussed above, two things can be similar in many respects and not for that reason one represents the other, let alone explain the other. In addition, Sugden doesn't suggest any possible way in which a model and its target would need to be similar in order to be able to infer a possible explanation. And as I already suggested, possessing similarities is not sufficient to explain something. The purpose of the model and the agent using the model also determine how the model represents. A further aspect is whether it provides a possible explanation, evidence for a causal hypothesis, etc. Nevertheless, I agree with Sugden in the value that he gives to the possible explanations that a model can offer, especially when one considers modelling as a process of ongoing discovery.

> "A community of scientists might have the collective aspiration to create a family of models and hypotheses as successful as those of Newtonian physics; but that is not what day-to-day modelling is about. The aim of an individual modelling exercise is to construct a model world that exhibits some similarity to the real world" (Sugden, 2011, p. 735).

Considering that the aim of individual models can be more modest than establishing truths about the world as Sugden suggests, it is perhaps easier to agree how valuable it can be when a model offers a possible explanation about a particular phenomenon. In particular, when one considers our limited knowledge about the world and therefore what it means to establish a possible explanation and a possible route of further research. In this sense, against what the critics of economics suggest, there are no reasons why some particular assumptions used in modelling should be discarded, as if we possessed the knowledge that they are no good for the ongoing process of discovery that is science.

CONCLUSION

Perhaps it could be argued that I am being too indulgent with the accomplishments of Kydland & Prescott's model in order to turn away the critics of the economics discipline. With the use of their model as an example, I have argued how, despite the fact that some models are built with unrealistic assumptions like the representative agent and the rational expectations, the models can still be considered valuable and to some extent successful. Given that the model of Kydland & Prescott and the methodology used have been highly controverted, it could seem as if I have moved the goalposts to dismiss the critique.

However, my sole interest in this thesis has been to argue that the way in which models are used is much more complex than the critics seem to suggest. Thus, from the general statements of the critics about how wrong or inappropriate some assumptions of economic models can be because of their unrealisticness, I have tried to disentangle the claim into some of the different elements that have to be considered first in the assessment and the use of scientific models. Once the elements are examined, the criticisms are dwindled.

The elements that I have considered are part of an account of how models are used in scientific representation. With this discussion I have attempted to show that the purposes with which models are built in the first place and thus the intentions of the agent about what is being represented, play a fundamental role in the design of the model with regard to the assumptions that are made. Here, the view that has been mostly discussed is one of similarity, where the scientist, according to the purposes, determines what is important in the model to be similar to the target system. Thus, with a model being similar to the target system, in the respects that the scientist considers appropriate according to the purposes at hand, a representation of such target system can be made. Realisticness is not necessary. The discussion of similarity is connected to idealisations and how they are used in models given our incapacity to build models as exact replicas of the world. An extreme and rare, but definitely interesting way in which idealisations can be used, is to isolate an underlying causal mechanism in order to establish the truth of this mechanism in the real world. Other—more modest—uses are to ignore some aspects of a particular phenomenon—like the coordination problem in the model of Blanchard &

Fischer—to focus on the aspect of interest. In this respect, it was also shown how the use of idealisations is very complex, given that not only the epistemic and phenomenological purposes need to be considered, but also, that incompatibility between idealisations made within one model does not necessarily invalidate their use, as the Bohr model of the atom demonstrates. An aspect, of course, that would have to be studied further would be to enquire about the cases in which their incompatibility yields problematic outcomes and how this would be determined.

Another aspect that has to be studied and that lays beyond the scope of this thesis is how to decide when a model can be regarded as 'reliable enough' in order to use it as support for determining a particular public policy. I have argued that a model having unrealistic assumptions is not sufficient for it to be regarded as useless. On the contrary, I have argued that they can have *some* value. But of course, this value can be insufficient when it comes to making decisions that can affect significantly the well being of individuals.

Thus, although my contribution could be regarded as rather minute, it is just necessary to consider the elements that I have discussed, to discover that there can be at least *some* scientific value in the models that the critics try to denigrate, like the model of Kydland & Prescott. This model provides a possible explanation of how aggregate fluctuations occur, supported by some empirical evidence that, although contested, is not entirely inessential. Insofar as this scientific value remains in models like this one, no matter how minimum it is, to disparage this and other models because of the assumptions that they use, is a colossal mistake. My contribution has been to attempt to rectify such mistake.

BIBLIOGRAPHY

Acemoglu, D. (2009). The Crisis of 2008: Lessons for and from Economics. *Critical Review* , 185-194.

Alexandrova, A. (2008). Making Models Count. *Philosophy of Science* , 383-404.

Backhouse, R. (1995). *Interpreting Macroeconomics: Explorations in the history of macroecononic thought.* London: Routledge.

Backhouse, R. (2002). *The Penguin History of Economics.* London: Penguin Books .

Bailer-Jones, D. (2003). When Scientific Models Represent. *International Studies in the Philosophy of Science* , 59-74.

Bank of Sweden. (2004, 10 11). *Advanced information on the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel.* Retrieved 03 22, 2011 from Nobel Prize: http://nobelprize.org/nobel_prizes/economics/laureates/2004/ecoadv.pdf

Blanchard, O., & Fischer, S. (1993). *Lectures on Macroeconomics.* Massachusetts Institute of Technology.

Brante, T. (2010). Perspectival Realism, Representational Models, and the Social Sciences. *Philosophy of the Social Sciences* , 107-17.

Cartwright, N. (1983). *How the Laws of Physics Lie.* New York, NY: Oxford University Press.

Cartwright, N. (1999). The vanity of rigour in economics: Theoretical models and Galilean experiments. In P. Fontaine, & R. Leonard (Eds.), *The Experiment in the History of Economics.* New York.

Cassidy, J. (2009). *How Markets Fail.* Farrar, Straus and Giroux.

Colander, D. (2010). The economics profession, the financial crisis, and method. *Journal of Economic Methodology* , 419-27.

Colander, D., Goldberg, M., Haas, A., Juselius, K., Kirman, A., Lux, T., et al. (2009). The Financial Crisis and the Systemic Failure of the Economics Profession. *Critical Review* , 249-67.

Craver, C. (2002). Structures of Scientific Theories. In P. Machamer, & M. Silberstein (Eds.), *The Blackwell Guide to the Philosophy of Science* (pp. 55-79). Oxford, UK: Blackwell Publishers.

da Costa, N., & French, S. (1990). The Model-Theoretic Approach in the Philosophy of Science. *Philosophy of Science* , 248-65.

Davies, H. (2010). *The Financial Crisis: Who is to Blame?* Polity.

French, S. (2008). The Structure of Theories. In S. Psillos, & M. Curd (Eds.), *The Routledge Companion to the Philosophy of Science* (pp. 270-80). Abingdon, UK: Routledge.

Frigg, R. (2006). Scientific Representation and the Semantic View of Theories. *Theoria* , 49-65.

Frigg, R., & Hartmann, S. (2006 йил 27-Feb.). *Models in Science (Stanford Encyclopedia of Philosophy).* Retrieved 2011 йил 20-Apr. from Stanford Encyclopedia of Philosophy: http://plato.stanford.edu/archives/sum2009/entries/models-science

Gibbard, A., & Varian, H. R. (1978). Economic Models. *The Journal of Philosophy* , 664-77.

Giere, R. (2004). How Models Are Used to Represent Reality. *Philosophy of Science* , 742-52.

Giere, R. (2006). *Scientific Perspectivism.* Chicago, IL: University of Chicago Press.

Glanzberg, M. (2009). *Truth*. (E. N. Zalta, Editor) Retrieved December 7, 2011 from The Stanford Encyclopaedia of Philosophy: http://plato.stanford.edu/archives/spr2009/entries/truth

Grüne-Yanoff, T. (2009). Learning from Minimal Economic Models. *Erkenntis* , 81-99.

Hands, D. W. (2001). *Reflection Without Rules.* Cambridge: Cambridge Univeristy Press.

Hausman, D. (1992). *The Inexact and Separate Science of Economics.* Cambridge: Cambridge University Press.

Hesse, M. (1978). Habermas' Consensus Theory of Truth. *PSA:Proceedings of the Biennial Meeting of the Philosophy of Science Association* , 373-96.

Hodgson, G. (2009). *Petition Revitalizing Economics After the Crash*. Retrieved September 27, 2011 from Petition Revitalizing Economics After the Crash: http://www.ipetitions.com/petition/revitalizing_economics/

Hodgson, G. (2011, May). Reforming Economics andter the Financial Crisis. *Global Policy* , 190-195.

Hoover, K. (1995). Facts and Artifacts. *Oxford Economic Papers* , 24-44.

Hoover, K. (2010). Idealising reduction: the microfoundations of macroeconomics. *Erkenntnis* , 329-347.

Hoover, K. (2004). *The Methodology of Empirical Macroeconomics.* Cambridge: Cambridge University Press.

Hume, D. ([1748] 1955). *An Enquiry Concerning Human Understanding.* (C. Hendrel, Ed.) Indianapolis: Bobbs & Merrill.

Janssen, M. (1993). *Microfoundations: a critical inquiry.* London: Routledge.

Keynes, J. M. (1936). *General Theory of Interest, Money, and Employment.*

Kirman, A. (2010). The economic crisis is a crisis for economic theory. *CESifo Economic Studies* , 498-535.

Kirman, A. (1992). Whom or What Does the Representative Individual Represent? *The Journal of Economic Perspectives* , 117-36.

Krugman, P. (2009, Sept. 2). *How did Economists Get it so Wrong? - NYTimes.* Retrieved Sept. 28, 2011 from The New York Times: http://www.nytimes.com/2009/09/06/magazine/06Economic-t.html?pagewanted=all

Kuorikoski, J., Lehtinen, A., & Marchionni, C. (2010). Economic Modelling as Robustness Analysis. *British Journal for the Philosophy of Science* , 541-567.

Kydland, F., & Prescott, E. (1982). Time to Build and Aggregate Fluctuations. *Econometrica* , 1345-70.

Lawson, T. (2009). The current economic crisis: its nature and the course of academic economics. *Cambridge Journal of Economics* , 759-77.

Mäki, U. (2011). Models and the locus of their truth. *Synthese* , 47-63.

Mäki, U. (2009). Realistic realism about unrealistic models. In H. Kincaid, & D. Ross (Eds.), *Oxford Handbook of the Philosophy of Economics* (p. XXXX). XXXX: Oxford University Press.

Mankiw, N. G. (1990). A Quick Refresher Course in Macroeconomics. *NBER Working Paper Series* (WP: 3256), 1-40.

Mankiw, N. G. (2006). The Macroeconomist as Scientist and Engineer. *The Journal of Economic Perspectives* , 29-46.

Martin, M., & McIntyre, L. C. (1994). Introduction. In M. Martin, & L. C. McIntyre (Eds.), *Readings in the Philosophy of the Social Sciences* (pp. xv-xxii). Cambridge: Massachusetts Institute of Technology.

McCallum, B. (1988). Real Business Cycle Models. *NBER Working Paper Series* , WP: 2480.

McCloskey, D. (1983). The Rhetoric of Economics. *Journal of Economic Literature* , 481-517.

McCloskey, D. (1998). *The Rhetoric of Economics.* Madison: The University of Wisconsin Press.

Morgan, M. (2008). Models - The New Palgrave Dictionary of Economics. In S. Durlauf, & L. Blume (Eds.), *The New Palgrave Dictionary of Economics Online.* Palgrave MacMillan.

Mueller, D. (2003). *Public Choice III.* Cambridge, UK: Cambridge University Press.

Odenbaugh, J., & Alexandrova, A. (2011). Buyer beware: robustness analyses in economics and biology. *Biology and Philosophy* , 757-71.

Reiss, J. (2008). *Error in Economics: Towards a More Evidence-Based Methodology.* Abingdon: Routledge.

Reuters. (2010, February 8). *Top four brewers make half global beer market*. Retrieved November 25, 2011 from Reuters: http://uk.reuters.com/article/2010/02/08/uk-beer-idUKTRE6173IZ20100208

Roubini, N., & Mihm, S. (2010). *Crisis Economics: A Crash Course in the Future of Finance.* New York: The Penguin Press.

Snowdon, B., & Vane, H. (2005). *Modern Macroeconomics: It's origins, development and current state.* Cheltenham: Edward Elgar Publishing Limited.

Stiglitz, J. (2010). *Freefall: America, Free Markets, and the Sinking of the World Economy.* W.W. Norton & Company.

Stiglitz, J. (2011, May). Rethinking Macroeconomics: What Went Wrong an How to Fix It. *Global Policy* , 165-75.

Stiglitz, J. (2009). The Anatomy of a Murder: Who Killed America's Economy? *Critical Review* , 329-39.

Sudgen, R. (2001). The Status of Theoretical Models in Economics. *Journal of Economic Methodology* , 1-31.

Sugden, R. (2000). Credible worlds: The status of theoretical models in economics. *Journal of Economic Methodology* , 1-31.

Sugden, R. (2011). Explanations in search of observations. *Biology and Philosophy* , 717-36.

Suppe, F. (2000). Understanding Scientific Theories: An Assessment of Developments, 1969 - 1998. *Philosophy of Science* , S102-S115.

Teller, P. (2008). Representation in Science. In S. Psillos, & M. Curd (Eds.), *The Routledge Companion to Philosophy of Science* (pp. 435-441). Abingdon: Routledge.

Teller, P. (2001). Twilight of the Perfect Model Model. *Erkenntnis* , 393-415.

The Economist. (2009, July 16). What went wrong with economics and how the discipline should change to avoid the mistakes of the past. *The Economist* .

Watson, M. (2005). What Makes a Market Economy? Schumpeter, Smith and Walras on the Coordination Problem. *New Political Economy* , 143-161.

Weintraub, E. R. (1979). *Microfoundations: The compatibility of microeconomics and macroeconomics.* London: Cambridge University Press.

Weintraub, E. R. (1977). The Microfoundations of Macroeconomics: A Critical Survey. *Journal of Economic Literature* , 1-23.

Wimsatt, W. (2007). *Re-engineering philosophy for limited beings: Piecewise approximations to reality.* Cambridge, MA: Harvard University Press.

Wittgenstein, L. (2001). *Tractatus Logico-Philosophicus.* London: Routledge.