

CHAPTER 2

*Macroeconomic methodology – from a Critical
Realist perspective*

The coherence of Post Keynesian Economics lies principally at the methodological level.

(Dunn, 2004: 34)

Prologue

The purpose of this chapter is to give the theory-of-science background for the development of a realist-inspired, macroeconomic methodology that can serve as a foundation for post-Keynesian macroeconomic theory. It is crucial to clarify the methodological fundamentals before any theory is drawn up. Theories and models in economics cannot be plucked out of thin air; they will always be anchored to the chosen method, and therefore it is important to discover whether there is consistency between the employed scientific practice and the theoretical intention. If the goal of a macroeconomic analysis is to provide policy recommendations to improve the real macroeconomic development, then the theory must be anchored to a realistic methodology. If on the contrary the goal is to investigate the existence of equilibrium in a theoretical model, then the method should be chosen in accordance to fit this problem. It is important that the aim of the analysis is recognised when the analytical models for policy recommendations are developed and selected.

In the previous chapter, the significant division of purposes between neoclassical general equilibrium theory and post-Keynesian macroeconomic theory was described. General equilibrium theory has the primary task of analysing and understanding the nature of the functioning of a perfect market system. Here, the existence of equilibrium is a core attribute. The ambition of post-Keynesian macroeconomic theory is to understand and explain trends in past macroeconomic development and provide policy recommendations with relevance for the future. This clear division of tasks is naturally a determining factor for the choice of methodological foundation, which these two macroeconomic schools use to honour their very different analytical ambitions. It can be seen from figure 2.1 below that post-Keynesian macro-theory must have a foundation based on reality and be formed through a reflection of reality. The general equilibrium model-builders, on the other hand, prefer primarily to build on a deductive methodology, where the starting point is a handful of axioms that determine the functioning of the economic system. In this way, the ground is laid for an analysis that can reveal the properties of the system. It is not the axioms' basis in reality, but rather their analytical precision, that is the deciding factor in their selection.¹ The focus on two quintessentially different macroeconomic issues that the two schools wish to analyse and understand also explains why two fundamentally different methodologies are employed. The importance of choosing the right methodology is illustrated in this chapter.

¹ It is in this perspective that the assumption of 'rational expectations' can be understood. Originally, in Muth (1961), the basic assumption of rational expectation formation entailed that actors were assumed to utilise all *available* information as best they could. This assumption at first glance seems realistic and plausible. It only becomes indisputably unrealistic when its content is altered to an assumption that actors have full knowledge of the model's long-term outcome (meaning they can foretell the future). The assumption of full information implies a number of simplifications in the analysis, not least its technical nature, but prevents the model's results from being applicable to reality.

The chapter is also intended as a broader presentation of the theory of scientific method. It contains a number of more common methodological issues that are particularly relevant for interdisciplinary analyses within the social sciences. This approach has been chosen because it simultaneously substantiates the methodology behind post-Keynesian macroeconomic theory.

Introduction to macroeconomic methodology: central issues

Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world...because... the material to which it is applied is, in too many respects, not homogenous through time. The object of a model is to segregate the semi-permanent or relatively constant factors from those which are transitory...so as to develop a logical way of thinking about the latter... (Keynes, 1938, CWK, XIV: 296-7).

The previous chapters included a short presentation of the central macroeconomic characteristics that must be taken into account when justifying the selection of methodology in relation to the theory of science. Macroeconomic theory differs from microeconomics in that it aims at a holistic analysis. Reality must be simplified in order to gain an overview of its entirety, and so a few, central variables (employment, balance of payments, growth, inflation and the national income etc.) must be selected and described together. The next chapter describes how macroeconomic reality can be given an analytical representation in the form of a ‘macroeconomic landscape’. The metaphor ‘landscape’ is used in order to emphasise that we are working with a simplification of reality, and that reality is in a state of constant flux because many other important conditions, in addition to the purely economic, exert influence over the shape of the landscape and the way in which it changes. Finally, this metaphor also highlights the fact that the part of the landscape which we are capable of observing is, figuratively

speaking, just the tip of the iceberg, since all the important factors that lie hidden beneath the surface cannot be represented.

These are the conditions under which a macroeconomic analysis of reality must be performed; we must get to grips with what Keynes called *the system as a whole*. It is therefore not the actions of individuals that are of interest, but rather the interaction of countless individual transactions, conducted within given, yet transitory, structures, national as well as international, that serve as our focal point. For this reason alone methodological individualism is rejected as the starting point in macroeconomics.

The analytical ambition, on the other hand, is to explain the transformation of the macroeconomic landscape as represented by a few central macroeconomic variables. The aim of the analysis is to reach a better understanding of the causal relations constituting the macroeconomic reality that can be described in part through national accounting data and in part through the behaviour of important macroeconomic institutions, such as the government's economic policy.

The microeconomic foundation is *not* of particular interest. It is often the case within post-Keynesian macro-theory that model results could (in principle) be generated by various (and on the micro-level competing) behaviour models. It is therefore not possible to derive post-Keynesian macro-theory, much less the macro-model, exclusively from deductions based on theories of microeconomic behaviour. Fundamentally, the fallacy of composition serves as a barrier to this. On the other hand, a realistic macroeconomic theory requires that the model is not built upon assumptions that are clearly in conflict with *observable* microeconomic (institutional) behaviour².

For instance, there is no *a priori* reason why post-Keynesian macro-theory should

² It was explained in the previous chapter that the contents of the so-called second neoclassical labour market postulate, which concerns firms' microeconomically-based demand for labour, was not in conflict with Keynes's macroeconomic model.

accommodate the assumption of rational expectations – the condition that macroeconomic actors are assumed to have perfect foresight – as this assumption directly contradicts observable microeconomic behaviour. The reader can find a discussion of the meaning of ‘unrealistic’ assumption below.

However, it is a fundamental empirical fact that economic transactions are conducted in spite of an inherent uncertainty with regard to the future. It is methodologically challenging that the future is, to varying degrees, uncertain. Uncertainty exists on three levels. Firstly, the course of economic development is unknown when plans are laid for the future. Secondly, the consequences of economic actions are similarly uncertain. Thirdly, it is at least partly uncertain how macroeconomic actors react, particularly in relation to this non-quantifiable uncertainty. It is precisely because uncertainty is such a dominating phenomenon, that post-Keynesian macroeconomics has been designated the economics of fundamental uncertainty as distinct from economics of risk (Davidson, 1973). It will therefore have a major impact on the analytical results and their interpretation if it is assumed that all actors have perfect foresight, meaning that everyone knows the *same future with* (stochastic) *certainty*. The real methodological challenge in macroeconomic theory lies in the ontological condition that the future is, at least partially, unknown. *Macroeconomic uncertainty* exerts influence over both the present expectations and the consequences of present actions. This holds true for the microeconomic actors’ actions as well as for macroeconomic policy.

The overarching aim of this chapter is to discuss the scientific-theoretical foundation for conducting a macroeconomic analysis based on reality. The post-Keynesian macroeconomic ambition is to understand macroeconomic reality. This

requires ensuring a high level of communication between, on the one hand, ‘reality’, which I will call World 1, that always plays out in a historical context on the *actual level*, and on the other hand, the *analytical level*, which I call World 2, where ‘theory and model’ are formulated and confronted with reality through empirical tests. The more rigorous empirical testing the model can withstand, the greater is its ability to describe historical phenomena using a scientific method and the more faith we can have in the analytical results. These results will, on the other hand, always be both conditional and preliminary and will always be open for improvement. They are *context-dependent* and must be interpreted as such before they are used to form statements about a specific case, which I call World 3, of a likely macroeconomic development that always will be path-dependent.³

This method of alternation between reality and model, where the inductive and deductive methods supplement one another, is called *retroduction* (or abduction) by a number of methodologists practising critical realism (Davidsen⁴, 2001) and (Downward & Mearman, 2006).

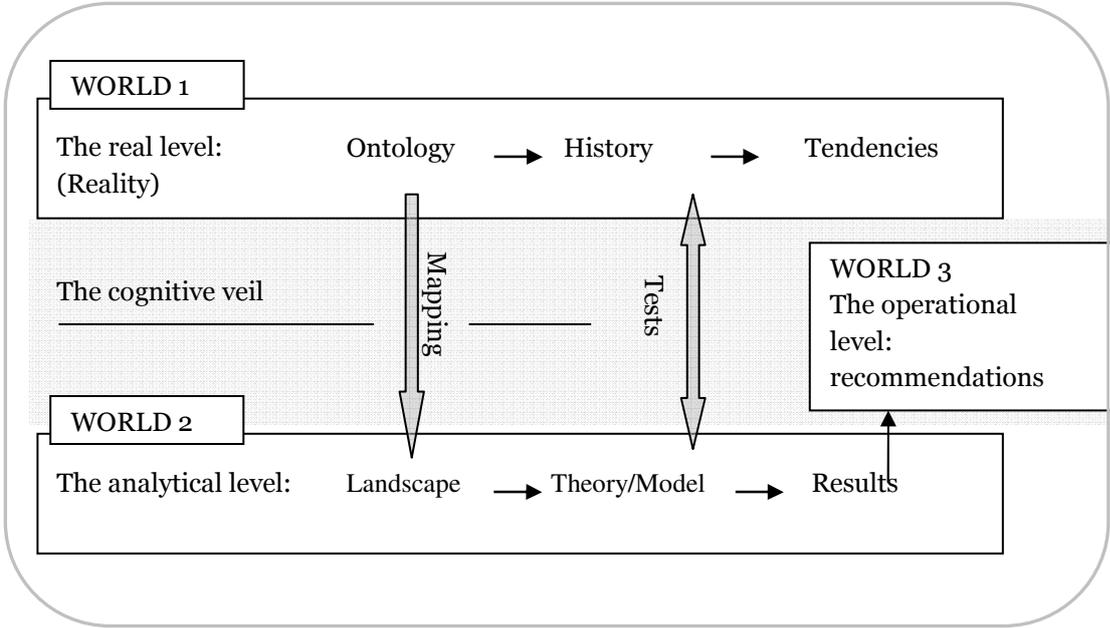
Critical realism, which will be thoroughly illustrated in the following sections, is based upon this retroductive methodology, developed and described by among others the American philosopher Charles Sanders Peirce in the nineteenth century, and ‘reinvented’ within social sciences by among others Roy Bhaskar, Tony Lawson and Peter Lipton used within modern macroeconomics by Philip Arestis, Victoria Chick, Sheila Dow, and many others. This methodology is characterised by explicitly including

³ The inspiration for this three-worlds metaphor is from Popper (1998); but my use of World 3 as a semi-reality, where analytical results is applied, deviates from Popper’s definition of World 3’s concepts.

⁴ This is a Norwegian post-Keynesian methodologist.

of real phenomena like uncertainty and the historical context, and in using open system modelling in its representation of reality.

Figure 2.1: Critical Realism methodology (Retroduction)



Why Realism? – with inspiration from Karl Popper

It is important that the concepts within the methodological discourse are used clearly and, as far as possible, consistently with common practice. To avoid serious semantic misunderstandings, I have composed a word list at the end of the book that gives the definition of terms used in this presentation. In any case, there is a need to make clear that what we mean by the terms ‘ontology’ and ‘idealism’ diverges somewhat from the common philosophical use of the words. Ontology usually means ‘knowledge of that which exists’, but when it is used here it means ‘*the nature of (what exists in) the world; that is, the nature of being*’ (Lewis, 2004: 26). ‘Idealism’ is used about a deductive methodology that is based on postulated axioms, which are *not* subject to empirical testing. The method employed by general equilibrium theorists is an example of an idealistic line of theory that must be understood in direct opposition to realism based in empirics.

The school of scientific methodology called *realism* shares the assumption that a physical/material reality exists independently of social-scientific practice. This approach to social science has the task of creating new knowledge which is in some way independent of the researchers world view and thereby provide a less subjective understanding of the macroeconomic relationships etc. Any scientific practice, meaning the development of theories and analytical models, must necessarily include a reduction of reality which cannot be entirely objective. On the other hand, this quest for *realism* requires that assumptions of reality used in the simplification process are in accordance with empirical observations. One obvious methodological problem related to critical

realism is that exact procedures for how these empirical requirements are best met, cannot be formulated explicitly.⁵

Any model will to a certain degree be unrealistic; otherwise it would not be a model. The demand for realism complicates the leap from the real level to the analytical level. But this is the consequence of the fact that the analytical method cannot be independent of the domain under investigation. There is a difference between the methods of analysis used to describe how, for example, the strawberry market and the labour market function, respectively. The ontology of these markets differ significantly on so many factors, that it would hardly be prudent to use the same *analytical template* on the two markets. An introductory *ontological reflection* would help uncover this issue and therefore ought to be a prelude to any realistic analysis. It is precisely the required correspondence between the ontological domain and the analytical method employed that characterises realism, as opposed to idealism and logical positivism. The requirement of an *ontological reflection* is represented in figure 2.1 by the wide arrow from the real to the analytical level.

But even a thorough *ontological reflection* must, following scientific practice, be of an *a priori* nature and include a number of limitations. Stated simply, analytical results must always be **unrealistic**. As is often emphasised in the realist tradition from David Hume to Karl Popper, the absolute truth can never be found; still, more general theories will, through the scientific process, replace theories with a smaller domain. We naturally find ourselves on a slippery slope, in that the analytical results will always be

⁵ Friedman (1953) cuts through this issue with ease by looking away from the realism of the assumptions. For him, it is enough that the model is good at making predictions, but not *why* it is good at making predictions. For me, it is a perspective that hinders the understanding of the kinds of causal mechanisms that are at play behind the predictions. In this way, it is made impossible to assess the validity of the theory beyond the limited field of prediction where it has been tested. This fundamental weakness with Friedman's instrumental approach is due to the fact that the validity of the results is somewhat doubtful when the realism of the model's empirical basis is not evaluated.

influenced by the unrealistic assumptions and employed methods. There will often be a trade-off between realism in the selection of assumptions and the clarity of the results, the so-called conflict of *Truth vs. Precision* (Mayer, 1991).

Further inspiration from Popper's 'three-stage methodology' and 'three Worlds'.

I am totally on the side of realism...[W]e can draw conclusions about [the theory's] proximity to the truth only if we are realists.

To be a 'positivist' is tantamount to being an opponent of all philosophical speculation and especially an opponent of realism.

I think of myself, then, as a metaphysical realist. (Popper, 1999: 22-4)

Karl Popper stands as an exponent for methodological realism. It is reality (World 1) that we wish to understand. We seek the Truth, meaning the complete explanation of the dynamic relationships that determine development, both physically and socially. Regardless of the fact that there are major differences between natural science and social science, the level of ambition is the same, yet the ambition is unreachable, as human understanding sets limits to what can be fathomed – not least in a world under constant change. This limitation exists also, at least partially, on the analytical level (World 2). There are simply limits to what the human brain can comprehend in an uncertain world that constantly changes. The growth of our knowledge must necessarily lag behind reality. The knowledge which we acquired about World 1 is finally reflected in World 3, which represents our interpretation of the analytical results obtained from World 2. Popper's important contribution to the discussion of the theory of scientific method was that he pointed out that knowledge first becomes science when it has been subjected to empirical validity testing. If a theory cannot be tested against the material

used to formulate a statement, then it is impossible to speak of its validity. According to Popper, the demarcation line for being able to attach the term ‘science’ to a hypothesis is that the hypothesis can withstand a falsification test.⁶ It is important to acknowledge that falsification is a demarcation criterion for the theory’s range of validity. On the other hand, no ‘true’ theories in the strictest sense of the word can ever be found. All theories are approximations of an unknown reality. Einstein’s demonstration of the constant speed of light did *not* make Newton’s theories more or less wrong, used within the theory’s range of validity. The range of validity was merely more precisely defined, and outside of this range, Einstein presented a theory which demonstrated a better approximation of reality. If Einstein had not developed his theory of relativity, then Newton’s theory would probably have continued to be used also outside of its own range of validity, though increasingly with the help of *ad hoc* supporting hypotheses, until another new and more general theory was developed. But when the number of anomalies and their related supporting hypotheses increases, it is often a sign that the existing theory is being deployed outside of its range of validity. This was the case when the understanding of the solar system changed, a process which took more than 150 years. Similarly, it was the case within macro-theory when the term ‘involuntary unemployment’ arose in the period between the two world wars. And it is the case today with economic growth, where the explanation of the stagnant and even reverse growth trends, using neoclassical theory, requires a growing number of supporting hypotheses.

⁶ The problem is known from e.g. the former consumption theory, where it was impossible to test the hypothesis of ‘utility maximisation’ on the basis of observed consumption data alone, except for inconsistencies. For example, one cannot disprove the hypothesis that the consumer had maximised his expected benefit. To do so demands experimental attempts such as Richard Layard (2005) and others have described. Popper names Freud’s psychoanalysis as an example of a hypothesis that must remain a hypothesis as it cannot be falsified, because neuroses are attributed to *unknown* traumas. This does not exclude the possibility that Freud’s theories are apt descriptions – only that they cannot be tested, and until they can, the results cannot be called scientific.

Popper's requirement of falsification has been criticised from many angles by Caldwell (1982), McCloskey (1986), Hausman (1991), Hands (1993, 2001) and others, yet the critique mainly targets Popper's position as a realist (and positivist) and, in the opinion of his critics, his exaggerated faith in empirical tests. But all come up short when alternative scientific criteria are to be formulated. If science is not to erode to relativism, or even more worrying, be decided by power relations, then it is difficult to find a more objective umpire than reality. But this can take a long time, particularly when strong economic or political interests are involved. Seen in a historical perspective, a number of competing theories can exist over long periods. This is not surprising, particularly within the social sciences where reality changes rapidly. These changes will themselves demand a continuous renewal of the knowledge base. Acknowledging this places greater focus on the importance of developing a robust methodology that can help social scientists – in this case macroeconomic theory – to keep up with the times. However, it will be possible to subject the realism of these theories and models to empirical testing against historic material, which can provide an indication of the degree of 'realism'. This testing is represented above by the double-ended arrow in figure 2.1.

Lakatos on research programs

It is one thing to be a theoretician of science and lay down guidelines for how to perform good research, just as Popper formulated his demands. However, it is considerably more difficult to conform to these guidelines in practice. The sociology of science is an independent research area to which Imre Lakatos and many others have contributed. As described in the previous chapter, macroeconomics is divided into schools which increasingly reside in their own 'space'. Members attend different

conferences and write in different journals, and their teachings are presented in separate courses. If these researchers meet by chance in the corridors of the university, they speak of other issues than economics. They have nothing to say to one another. These researchers are simply engaged in different research programmes.

Lakatos characterised a research programme as consisting of a ‘hard core’ and a ‘protective belt’. A few indisputable axioms compose the hard core, while the belt consists of a number of supporting axioms, which can be modified along the way in the event that the results of the model encounter empirical difficulties. This construction gives research-significant inertia. Burned-out research programmes are rarely dismantled, because within an established research milieu it takes a generation to acknowledge the condition of exhaustion, particularly when the hard core is never subjected to real empirical testing. The hard core can consist of basic behaviour-related assumptions (e.g. rationality), assumptions of institutional conditions (e.g. market-clearing), and/or a particular method (e.g. general equilibrium), which are considered as an indispensable part of the research programme. If the hard core cannot be confronted with falsification tests, then – according to Popper – the scientific program remain speculative. In that case the hard core can easily become a creed rather than an empirically proven fact and hereby push the research program toward degeneration.⁷ The lack of serious empirical testing might also hinder that internal inconsistencies are unveiled, because the hard core of the research programme does not become subject to scientific discussion.

These problems with the lack of empirical tests of hard core assumption is well known from neoclassical macro-theory, where axioms of rational actors, market

⁷ The so-called ‘stylised facts’, an expression that originated with Kaldor, involve a development whereby empirically backed statements risk gradually becoming indisputable axioms.

clearing, rational expectations and perfect competition are used unchallenged by reality. Within the borders of general equilibrium macro-theory, these assumptions have not been subjected to a systematic falsification process and so it is impossible to define the theory's range of validity. With this comes the risk that the research programme will at some point begin to degenerate when the theory cannot explain a number of phenomena that lie outside of its range – a kind of indirect falsification. Such a fate befell the 'neoclassical synthesis' in the 1970's, when the coincidence of rising inflation and rising unemployment could not be explained within the model. The same process also characterised a period in the Marxist school, when the breakdown and dilution of the production-determined class society in the West led to the 'melt-down' of the research programme's hard core in that part of the world.

It will become apparent in the following section that a number of the difficulties that these research programmes confronted could have been prevented if only the researchers involved had been more open to Popperian methodology. As has been stated above, Popper rejected the idea that any part of a research programme could be 'above the principle of falsification', meaning beyond the demarcation lines for scientific cognisance. He naturally acknowledged that the initial hypotheses, formulated in World 2, do not just 'appear from nowhere'; they must be a product of *a priori* reflection. The fact that reflection is based on preconceived notions and often unsystematic empirics provides further encouragement to conduct a falsification test. For as Popper formulated the constructive element in his theory of science: '*we only learn from our mistakes; [then] our knowledge grows*' (Popper,1997: xx).

In this way, Popper places the interaction between the real and the analytical levels at the heart of his theory of science and therefore of scientific progression.

Popper is not a simple 'falsificationalist'

As mentioned above, the requirement of falsification is regarded by a number of proponents (e.g. Lakatos and Blaug) and critics (including Caldwell and Hands) as Popper's most significant, though not only, contribution to the theory of science.⁸ This view is hardly compatible with Popper's insistence upon his being primarily a *critical realist* with the emphasis on *critical* (Boland, 2003). Popper's approach to acquiring knowledge is characterised by Boland, a major admirer of Popper, as being that as a starting point we should admit that we hardly know anything, which is a rather Socratic view of science. In such circumstances, falsification can be a useful tool to delimit what we still do not know. Furthermore, it is important to emphasise that Popper has an understanding of knowledge and the acquisition thereof as being an *open* and never-ending process,

...but always in a state of constant revolution, [because] science is a social enterprise of coordinated criticism rather than coordinated agreement, and there is therefore no doubt that those readers with a Popperian background have always taken 'critical realism' for granted (Boland, 2003: 244) ... Basically, the main question is: do the model's assumptions truly represent reality, that is, represent the real, objective world? (ibid.: 284)

This question is reminiscent of that posed in relation to figure 2.1: How can communication between the real and analytical levels be ensured? It is in its response to this question that *Critical Realism* can make a difference. The 'critical' element lies among other things in its continued insistence and discussion of the importance of ensuring an interaction between theory and reality.

⁸ Blaug (1980) uses the name critical rationalist to describe Popper's theoretical approach, as he narrowly attributes the name 'critical realist' to the limited approach represented by Bhaskar and Lawson described below. Surprisingly, Blaug (2003) later characterises the latter as 'post-modern', using the argumentation extending from the expression 'transcendental realism'.

What is Critical Realism?

There is an emerging consensus that the Post Keynesian approach is consistent with much of critical realism, with open-system theorizing applied to an economy understood as an organic, open system. Different forms of abstraction are relevant to different questions, and different economies; and indeed the study of actual economies required before abstraction can occur involves the application of different disciplines (Dow, 1996:79).

Finally, since Post Keynesian theory starts with observation, the position on empirical matters must be discussed. First, rejecting the subjective/objective dual.... 'Facts' can be observed with some degree of objectivity... Since the group of theories includes formal models which are susceptible to empirical application, Post Keynesians do not reject econometrics (ibid.: 80)

Critical Realism is not a well-delimited theoretical-scientific direction. 'Critical' should be understood in this context as discussing or delimiting. When Popper calls himself a realist, where are the boundaries for his realism? As mentioned above, he uses the expression *metaphysical realist*. Popper goes as far as to describe himself as a non-positivist (Popper, 1999: 24), since knowledge is a dynamic concept in World 3 based on the comprehension of results obtained in World 2 through speculation, deduction and empirical tests.

Roy Bhaskar (1975), one of the relatively new proponents of critical realism, even uses the expression *transcendental realism* to describe his position within the theory of science. He notes especially the meaning of real phenomena that are not readily observable. Hence the term 'transcendental' is used in reference to unobservable structures at the 'deep' cognitive level. Bhaskar can at times present his theoretical discussion in such flowery language that it can give the reader a 'mystical' impression, which diverts attention away from the realist project – to understand reality.

Tony Lawson (1997) is heavily inspired by Bhaskar in his theoretical discussion of (mainstream) economics and reality. His book is primarily a theoretical criticism (in the common use of the word) of general equilibrium theory's split from the real level.

He opens his book with the following ironic sentence: ‘*No reality, please. We’re economists!*’⁹ This is a development which he finds could be attributed to neoclassical economists’ search for a microeconomic foundation, on the basis of methodological individualism, the assumption of market clearing and the required formal deduction by means of mathematically formulated models of analysis. Lawson’s critique, cf. appendix 2, centres on the lack of a proper ontological reflection in this line of theory. The same basic analytical model is used regardless of its subject. The corn market, labour market and money market are modelled on the same template, based on the assumption of rational agents, individual optimisation and potential market clearing.

The common denominator for the three theoretical contributions presented here under the title *critical realism* (Bhaskar, Lawson and Popper) is the desire to achieve congruence between the real level (the ontology of the subject matter) and the analytical level, i.e. the epistemology (theory, model, and method). This science-theoretical orientation should be understood as a reaction to the dominance of positivism within the natural and social sciences.

As different from Positivism

Positivism has been with us for centuries. Its adherents claim that only objective, demonstrable phenomena can be made subject to scientific investigations. It is important therefore to develop methods and instruments that could be used independently of the investigator. Objective measurement and infrangible logic became the trademarks of positivism, which culminated in the enlightenment; but it has confined itself to the natural sciences ever since (Favrholdt, 1998). Phenomena that cannot be sensed cannot be quantified. Positivism was thus originally a justified revolt

⁹ This was the title of his article in *The Times Higher Education Supplement*.

against metaphysics, including the influence of religion on the natural sciences. But within the social and human sciences, positivism was influenced at an early stage in its development by Hume's scepticism, since 'human' values were contained within these sciences and these could be neither measured nor ranked. How can sense impressions that cannot be physically measured be 'objective'? They can only be objective if they are brought about by exercising mutual, interpersonal control (Schulzter, 1960). This scepticism helped push positivism in the direction of less empiricism and more deductive modelling that was not troubled by subjectivism. This development culminated in the logical positivist position from the turn of the twentieth century (associated with the Vienna Circle to which Popper belonged for a short period); this position sought a scientific method that was based on as few and generally applicable empirical 'facts' as possible, from which new conclusions could be deduced on 'objective' grounds.

This tendency can also be seen within economics. Here, utilitarianism, originally developed by Jeremy Bentham near the end of the eighteenth century, has been a particular variant of positivism. Bentham argued that human happiness, or 'utility', should be measured in 'utils', as the net sum of 'pain and pleasure'. The idea was to calculate the number of 'utils' that each person experienced. The problem was how these utils could be measured. In the absence of something better, it was tempting to equate money (which can be measured) with utils. So, the greater the national product in money terms, the greater the level of measured happiness will be. However, the classical economists and the first generation of neoclassical economists (including Marshall, 1890, and Pigou, 1920) were aware that the marginal 'utility' of real income decreases when income increases; but they lacked an objective measurement of this

income effect. Therefore the second generation of neoclassical equilibrium theory, introduced by Robbins (1932) and systematised by Hicks (1939) and Debreu (1953), abandoned the practice of conducting inter-subjective comparisons of utility values. They argued that such a comparison would be normative and therefore unscientific.

It is beyond doubt that this second generation of neoclassical theory, in the version that appears in the textbooks as ‘economics’, is marked by logical positivism, in that a very few axioms serve as the foundation for the deduction of economic laws, *‘whose substantial accuracy and importance are open to question only by the ignorant or the perverse’* (Robbins, 1932: 1). Robbins proclaimed himself a realist: *‘It is a characteristic of scientific generalisations that they refer to **reality**’* (*ibid.*: 104 – my emphasis). One can almost draw a straight line through the history of economic theory from Lausanne (Walras and Pareto), through the London School of Economics (Robbins and Hicks) to MIT (Samuelson and Debreu) to track the development into general equilibrium models with microeconomic foundation and based on logical positivism that constitutes mainstream macroeconomics,¹⁰ particularly after the collapse of the neoclassical synthesis.

Critical realism was originally developed in an attempt to break positivism’s dominance over the natural sciences. In contrast, macroeconomic theory was first truly dominated by logical positivism only within the last 20-30 years of the twentieth century, in the form of general equilibrium models with a so-called microeconomic foundation. In this way, methodological individualism, market equilibrium and deductive reasoning became dominant for macroeconomic theory development and

¹⁰ Beautifully described in Weintraub (1985).

analysis. At the same time, empirical testing came to play an ever decreasing role in the formation, much less the testing, of the models' power of explanation.

The methodological approach of critical realism, on the contrary, places decisive emphasises on the fact that it is *reality* that must be understood and explained, and so methodological practice should be determined by the concrete manifestation of the subject matter. And it is precisely the often complex character of economics that is one of the primary reasons why Lawson insists on the necessity of introducing critical realism into this discipline. In Lawson's words: *'In short, the world is densely (if not exclusively) populated by totalities...that are complexly structured, open, intrinsically dynamic, characterised by emergence and so novelty, and inclusive of totalities and causally efficacious absences, amongst other things'* (Lawson, 1997: 65). These complexities and differences necessitate that every investigation should commence with a characterisation of the social ontology – an 'ontological reflection' to use Lawson's terminology. The cognitive starting point for this ontological reflection should be a preliminary characterisation of the subject matter as it can be observed in reality (World 1). This characterisation forms the basis of the macroeconomics landscape which has to be understood subsequently through a retroductive analytical process conducted within World 2.

The theoretical starting point for critical realism is therefore the socio-economic relationships that are assumed to exist independently of the researcher, but which are undergoing constant change. The development of theory, therefore, does not consist of uncovering an eternal, unchangeable economic structure. Rather, the aim is to explain the causal mechanisms that connect macro-actors and macro-markets under the further

premise that the actors' behaviour and the structures change and exert mutual influence on the macro-system's ontology over historical time.

Seen from a critical realist perspective, macroeconomic methodology does not merely consist of piecing together a jigsaw puzzle where the pieces are known in advance. The pieces are *not* known in advance. They become apparent through the scientific process of open system analysis in World 2, then results are interpreted and subsequently applied to World 3, where they will appear in a case- and context-specific way. Anyhow, the macroeconomic landscape is not static. On the contrary it changes continuously in an unpredictable way. Hence, new knowledge has constantly to be generated, just as the structures into which the pieces of knowledge will fit may also change through time. Critical realism, therefore, is open to methodological pluralism naturally including the use of mathematics (at the analytical level) – which Lawson summarises under the term epistemological relativism.¹¹

On the other hand, he rejects those methodologies that assume that the economic phenomena, including the macroeconomic reality, should simply be a social construction. This critical realist perspective has as its starting point that macroeconomic reality exists, where analysis of the causes of unemployment for example is not a relative question of which discourse is given the highest priority, but rather a matter of finding the most convincing empirically supported explanation.

The basis for realism (as opposed to idealism¹² and relativism) is that 'reality' does exist independently of which hypotheses the natural or social scientists develop. This view encapsulates a clear dissociation from the idea that it a scientific task to analyse 'nature' or 'society' as just an ideological abstraction (idealism) or a social

¹¹ The attachment of epistemological relativism to ontological realism facilitates a judgemental rationality (Lawson, 1997: 59).

¹² A pure ideological system is without an empirical foundation.

construction (relativism) whose existence and manifestations are determined only by the research traditions and their interpreters detached from reality, as might be the case when *logical positivism* or *postmodernism* is employed. In this respect, Lawson is in complete agreement with Arestis (1992) and Dow (1996) and stands clearly on a realist position.

Critical realism seeks congruence between ontology and epistemology

As already described, Lawson does not attempt to hide the fact that he has taken significant elements of his ontological reorientation from the science-theory discussion among natural scientists. In particular, he often cites Bhaskar (1975), *A Realist Theory of Science*, where a research programme based upon *transcendental realism* in biology is presented as inspiration.¹³

The need to escape the restrictions of positivism and create a more accommodating methodology arose within the natural sciences as early as the end of last century. To a certain extent, the need had always existed. But its necessity was made explicit through Einstein's observation of inexplicable phenomena which justified a renewed reflection of the nature of the physical world; ultimately extending the range of validity to include his theory of relativity. The research domain for classical physics was at that time limited to Newton's Laws of Motion¹⁴, which stood in the way of understanding a number of real phenomena. They could simply not be explained using 'Newton's method'. For example, classical physics could not explain the constant speed

¹³ It is a well-known problem within the natural sciences that whole entities cannot always be analysed on the basis of smaller entities (atoms). Biological organisms are not wholly described by their chemical structure, for example. Medical analyses must include both the biological and the human and social factors if they want to claim to be complete. F. Capra has written two important books about this subject, *The Tao of Physics* and *The Turning Point* respectively.

¹⁴ One should not underestimate how much a leap in the direction of a realistic explanation of natural phenomena Newton's theories were in their day, which only underlines that the critical perspective, in all scientific research, should never slip out of view.

of light, much less the random motion of electrons. The classical model of analysis had to be supplemented with, and in some cases replaced by, broader theories and models that were in better communication with the ‘new’ knowledge in physics. This did not render classical physics superfluous, but rather uncovered a number of previously unknown (‘deeper’) structures of World 1 that could be incorporated into the analytical World 2 and give a richer understanding of World 3.

In Bhaskar’s terminology, such a new discovery in the deep stratum is merely an example (among many) of the fact that behind the observable ‘reality’ exist structures, mechanisms, powers which play a significant role for macroeconomic development. Precisely for this reason, the framework for understanding reality (the interaction between the real and the analytical level), according to Bhaskar, ought to be established as an *open system*, capable of adapting new phenomena and producing new knowledge, under the influence of, among other things, these transcendent and, just as importantly, fluctuating real phenomena and structures. Here we confront a well-known ‘classical’ problem. Heraclitus is remembered, among other things, for his statement that you cannot step into the same river twice, for fresh waters are ever flowing in upon you. The water is continuously renewed, the banks eroded and the landscape can be hit by an earthquake – the future is uncertain. Thus, even seemingly unchangeable ‘physical circumstances’ will undergo constant changes – some naturally faster than others. A deeper understanding of these physical and social processes requires the development of open research programmes, as has been demonstrated numerous times even throughout the history of natural science. This does not necessarily mean that the existing research programmes are not useful, but that their range of relevance is limited by the available knowledge. Such are the conditions also within all sciences, and therefore the theory of

scientific methodology is so important for our scientific understanding. Let me give an example:

The starting point for research programmes based on realism (including positivism) is that reality exists independently of the scientists' observations and interpretations. The earth does not change its orbit, and the sun continues to rise every morning, despite the fact that science's view of the solar system changed from revolving around the earth to revolving around the sun. Perspectives on the cosmos have since changed numerous times. Hawking (1988) and others have demonstrated that science will continuously change our understanding, in this case of the universe – without actually ever reaching a full understanding.¹⁵ But – and this is an important addendum – solar eclipses, following Newton's work, could already be explained with astonishing precision, something Einstein's subsequent theories have changed very little. Within 'macro-natural-science' there are some areas where the ontology's constancy is so dominating that it is possible to establish analytical 'subsystems' which are approximately comparable to closed deductive systems where everything seemingly is predictable.

However, the social sciences do not share this constancy. As macroeconomic system changes over time, individuals and institutions are influenced by the new events they experience. If a government or central bank governor demonstrates a systematic pattern of reaction over a number of years, then the economic actors will begin to

¹⁵ This is a fascinating book about the history of the natural sciences, whereby unexpected observations, when first seen, were pushed aside, understood as the result of analytical or observational mistakes, and only much later became the foundation for a reorientation of the dominant theories. Such examples can also be found within macroeconomics – a number of 'inexplicable' phenomena during the crisis in the 1930's were a source of inspiration for establishing the Keynesian research programme, whereby the domain of the previously closed model was reduced to that of a special case.

calculate this economic policy into their expectations of the future. In this way, the effect of economic policy does change through time.

The link between World 2 and World 3

According to Popper, scientific explanations are only approximations of the real world. Researchers are often inspired to use colourful images and metaphors when they translate their analytical results into descriptive explanations that can be utilised in World 3; World 1's true nature remains (partially) unexplained. Gravity is an example of one such metaphor from the world of physics. It seems to provide an explanation of the planetary orbits, and in this respect, predictions have had an emphatic influence; but if we ask for the causal relations behind gravitation, then researchers come up one answer short. It is a similar case with electricity, described as the 'movement of electrons', or with the DNA molecules that carry our genes. These metaphors are best understood as a creative use of language, rather than the expression of the true understanding of a number of physical phenomena.

There has also been a great deal of linguistic ingenuity in the social sciences, including macroeconomics. Terms such as voluntary unemployment, natural and structural unemployment and cyclical unemployment came into fashion in the 1980's when unemployment peaked and mainstream theorists were unable to provide a convincing analytical explanation of the causes of the greatly increased unemployment. When it became necessary to offer some advice on a possible reduction of unemployment in World 3, these metaphors were used to establish a causal relationship that could legitimate a reduction of the wage level.

In fact, social researchers may develop metaphors or adopt concepts from other research area natural sciences or humanities that may lead the interpretation of the

analytical results astray and confuse the political implications of the results in World 3. Take for instance the metaphor of ‘sound finance’ applied to a public sector surplus, which in a case of recession might be a rather ‘unhealthy’ fiscal policy. Using the term ‘sound’ whatever gives a signal of something beneficial, which depending on the context might be or might not be attractive. Another example is the assumption of ‘rational expectations’ which sounds like a reasonable behavioural practice. Who would ever assume that economic agents form ‘irrational expectations’? Anyway the concept of rational expectations could easily for linguistic reason be misleading, because ‘rational’ does not mean ‘best possible’ expectation based on *available* information, but expectation based on full and correct information about the future. Hence, rational expectations could much better be called ‘ideal’ expectation, which would clearly communicate an analytical difference from ‘realistic’ expectations. Such kind of misnomers may cause misunderstandings especially when analytical results are transferred to world 3¹⁶ Otherwise there might arise a net of miscommunication between the ‘actual’ reality and the political reality. It is important that linguistic barriers, and thereby cognitive barriers, are not erected between the analytical and the political domain on the basis of misleading metaphors. The assumption of full macroeconomic foresight, denoted as rational expectations, is another example of a metaphor that creates linguistic confusion. The wording may have flair, but it obscures the far more important methodological issue, that the analytical results are based on the assumption that the future can be known with certainty. This is an idealistic assumption far from reality which influences the analytical results. Something that has to be

¹⁶ There is an eye-catching example from the Danish political debate. A group of neoclassical economists were asked to make a report on the economic development in the Danish economy the coming 35 years. They used a general equilibrium model, DREAM, where agents were assumed to form rational expectations. The concluding policy recommendations were delivered to World 3 without any reservations related to the unrealistic assumptions underlying the calculations made by the DREAM-model.

discussed before the results are communicated to the public domain of World 3. I will later, in chapter 7, return to further analytical implications of using ideal assumptions especially when they constitute a part of the hard core of the research program and hidden behind a veil of linguistic metaphors such as sound finance and rational expectations. It is sufficient to mention here that any assumptions ought to be tested empirically and the outcome of these tests should have an influence on how the analytical results are passed on as policy advices in World 3.

The analytical level (World 2) will always be different from World 1. That is the whole meaning of constructing an analytical model. But if it happens that clearly unrealistic assumptions are introduced perhaps for a practical purpose and a subsequent falsification test of this assumption is omitted – perhaps with an argument that we are in any case looking at a hypothetical long-term models that require observations of twenty or thirty years into the future before it can be empirically tested – *then anything goes*. Without a firm grounding in reality, World 2 can take any hypothetical shape, and the normative considerations associated with the chosen, but untested axioms can be difficult for anyone, even experts working outside the hard core of the research program, to detect and assess.

Andersen (2000) (cf. appendix 2.1) goes one step further when he compares a neoclassical analysis in world 2 with laboratory trials, where the whole macroeconomic system is made ready for experiments. The laboratory outcome is treated as the best possible description of reality, which is accordingly offered as the best advice regarding the real world. Lawson would claim that an epistemological error is made when World 1 is being equated to World 2. Even if the laboratory were the very best presentation of our (limited) knowledge of World 1, it would be a misrepresentation of the analytical

results to conclude that they represent reality. In other words, World 2 will always be a logical construction which reproduces elements of World 1 in stylised form and can, of course, never be a 1:1-projection. Therefore, policy advice should always be made conditional and modifications clearly expressed.

There is a significant distinction in the theory of science between whether it is the epistemology that analytically determines the target field or whether, on the contrary, it is the target field's ontology that sets the (quite often very demanding) requirements on the epistemology.

Critical realism is a coherent argumentation that explains why it is most relevant, particularly within the field of macroeconomics, to adopt the latter position. This is done despite the fact that full correspondence between the three worlds can never be achieved, since the macroeconomic reality, on the basis of its ontology alone, is both *open* and *indeterminate*.¹⁷ This circumstance must be taken into account for the subsequent presentation in World 3 of the analytical results obtained.

A methodology based on critical realism is therefore a possible solution for achieving a more general¹⁸ macroeconomic understanding.

¹⁷ As will be described later in the chapter, an *open* system is not only understood as a negation of a *closed system*; it is not either/or. The word 'open' is used in the sense that within the selected cognitive frame there is openness to everything that is possible – also the unforeseeable. An open model can include a closed model as a special case. In the same way, *determinate* should not merely be understood as the duality of *indeterminate*, rather as one possibility in an indeterminate (and therefore open) system.

¹⁸ Once again we run into semantic ambiguity. Keynes, in the title of his masterpiece, *The General Theory*, gave the word 'general' an ontological meaning, in that his new theory could explain a greater number of real phenomena than the existing theory. In connection with general equilibrium theory, the term *general* indicates that the analytical model deals with a greater number of markets, in contrast to partial equilibrium of just one market.

Critical realism: understanding the complex and stratified reality

Tony Lawson (1997, 2003)¹⁹ delimits *ontology* as follows: *ontology is the Nature of Social Reality*. He follows up the definition with a number of examples, almost all of which (surprisingly enough) have a background in natural science. He suggests for example, that a pneumatic drill can be a handy tool if we need to drill a hole in material made out of concrete – given the ontology of concrete. However, if we attempted to use the same drill to make a hole in a glass window, things would certainly go wrong. Why? Because we have not made the window’s ‘nature of being’, or ontology, clear to ourselves – with catastrophic results.

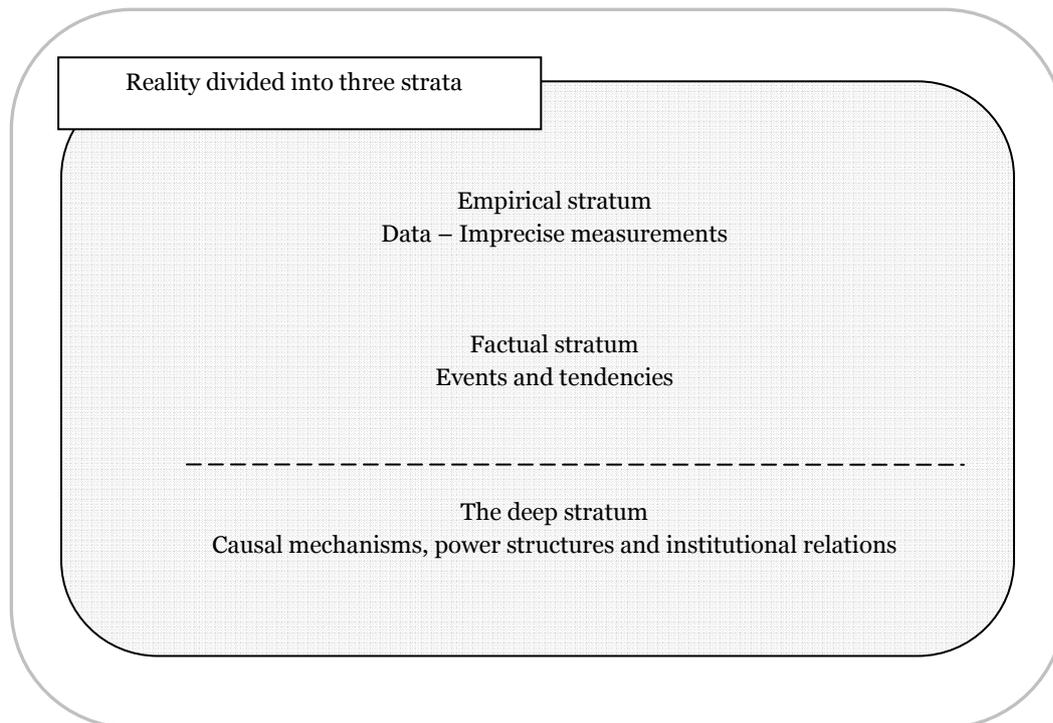
Understanding the object’s ontology is of great importance for acquiring relevant knowledge for any subsequent analysis. This conclusion also applies to the work of discovering causal macroeconomic mechanisms. Lawson points out that it is important to differentiate between the target field’s ontology and the knowledge that it is possible to obtain about the macroeconomic landscape. Lawson (1997: 33) quotes Bhaskar’s warning, mentioned above, against the erroneous epistemological conclusion that is reached if a statement about the target field’s ontology is reduced to (and actually equated with) a statement about the epistemological knowledge that we can gather exclusively on the analytical level (cf. above and appendix 2). On the contrary, it is the nature of the target field that determines the type of macroeconomic knowledge that can be acquired at all (the epistemology is limited by the target field’s ‘being’) – and so it also determines which questions can be answered meaningfully. The connection between ‘what is’ and the ‘knowledge of what is’ is established through adapting the epistemology to the ontology, which, if done correctly, can produce reliable results that

¹⁹ His books are by no means beach reading. They are quite clear in their critique of neoclassical, mainstream economics; but, though they are both over 300 pages long, it is surprisingly difficult to grasp *Critical Realism* as a scientific-theoretical tool.

constitute new, though still uncertain, knowledge of macroeconomic relationships. This means that it is important for the selection of the analytical method whether it is the labour market, the banking system, the exchange rates or energy supply that is the subject of analysis. This will be discussed in more detail in the following chapters. These four macroeconomic institutions have different characteristics in the form of formal power relations (legislative action), formal/informal agreements (e.g. wage negotiations, changes of interest and exchange rates) and the organisation of the market(s) being analysed. To the extent that a common ‘drill’ can be used to investigate and devise theories about the macroeconomic importance of these four very different institutions, the drill’s size and shape must be adapted to the social ontology of the target field.

Following this introductory and relatively general discussion of the importance of understanding the target field’s social ontology, it is now possible to make the presentation more concrete. Lawson argues that our knowledge of reality can be advantageously depicted in stratified form. He works with three different levels of cognitive data organised in three different levels: the empirical, the factual, and the deep stratum (cf. figure 2.2).

Figure 2.2: Stratified reality grounded in Critical Realism



The empirical stratum is the surface of the macroeconomic landscape. Here we have a number of observations from the national accounts, labour market statistics etc. But we know that all macro data are only estimates and, for that reason alone, there must be a certain amount of (statistical) uncertainty associated with all these numbers. In addition to this, we should remember that the definitions of data have to correspond with the prevailing theory. The classic example is the division of the demand components in the national accounts – here the influence of Keynes is unmistakable. These data are the immediate empirical representation of the landscape's appearance.

The next question in describing the landscape's appearance is whether there is a detectable pattern within these data. Can any tendencies be established that statistically demonstrate a robust significance, that goes beyond ordinary statistical randomness? Such tendencies cannot be immediately observed but can be said to exist in the factual stratum. These tendencies will appear as postulates, until they have been subjected to a

retroductive scientific process in which the formulation of hypotheses interacts with the empirical observations and are substantiated through a statistical testing procedure. In this way, an effort is made to discover the causal mechanisms that are the specific scientific result of macroeconomic analyses.

The empirical and factual strata are both a part of positive as well as critical-realistic reasoning. But, critical realism differentiates itself from positivism by contending that more general knowledge can be reached by discovering the causal mechanisms which are rooted in the deep stratum and not directly observable. In chapter 3 I will characterise these causal mechanisms within macroeconomics as macro-behaviour functions, grounded in empirically tested and stable relations, but not directly observable. These macro behaviour functions (causal mechanisms) cannot be analytically deduced as micro behaviour on a grand scale. They are aggregate items to which, in the majority of cases, no specific aggregated micro activity is associated. On the contrary one single macro number is caused by a myriad of individual and interrelated activities.

However, a few of the so-called macro-institutions stand out as dominated by individual activities. For example, the decision of the central bank to change the discount rate can be directly referred to as one specific activity. In fact, such a change will usually be followed by an 'official' explanation. In this case, the causal mechanism is apparently observable. But it is only 'apparently', for what lies behind the central bank's decision? This brings us to the important question about the macroeconomic method: how to uncover the causal mechanisms that lie behind a macro behavioural relationship. How to detect the relationship between cause and effect within macroeconomics, for instance between an external influence (e.g. the discount rate) and

an observed trend in a data stream (e.g. private consumption)? Similar questions can also be posed concerning the decisions taken by a finance minister with regard to a change of the tax rate, expenditure, welfare payments, etc. Why did he do it and what effects are likely to be expected on, e.g., employment, income distribution and public finance? Here we are down at the ‘deeper’ stratum of the macroeconomic landscape; that which Bhaskar aptly describes as the transcendental level. It is the part of the landscape’s topology which we cannot readily observe as it lies buried beneath the surface.²⁰

It is important that the researcher is aware of the meaning of the three ontological strata outlined in figure 2.2 and of the relationship between them in order to be able to formulate relevant hypotheses. The empirical and factual strata can (to varying degrees) be observed, while phenomena in the ‘deep’ stratum, by virtue of its nature, must remain largely hidden. Knowledge about the deep stratum phenomena will always be limited by the uncertainty that is related to its unobservable character, which can only be uncovered by indirect methods and empirical falsification trials.

It is a challenge to do research on phenomena that are not readily observable. In the deep stratum, we cannot even give a preliminary answer. In this case, *unexpected* observations might be a source of inspiration for new discoveries.

Within the framework of an *open* system, ‘inexplicable’ events will lead to a search for more general hypotheses within the existing research programme. For example, Keynes considered persistent involuntary unemployment as a kind of inexplicable phenomenon within the neoclassical macroeconomic theory. Throughout the 1920s he tried to reformulate the existing framework to make it capable of

²⁰ I have in another context used the iceberg as an image of the ontological stratification. The empirical top can be observed above the surface, but it is the 90% under the surface that is critical for shipping.

explaining this new tendency. But, in the end he had to acknowledge that the neoclassical research paradigm could not give a satisfactory explanation, i.e. an explanation that corresponded with empirical observations. Hence, he had to search for a new methodological paradigm to explain the hitherto inexplicable. At that stage the scientist will find himself in the speculative domain with a genuinely open research agenda, where our method of research, our understanding of the social ontology, has to be reformulated before new scientific knowledge can be established.

This is the *raison d'être* of following a Critical Realist scientific procedure in an attempt to understand the apparently inexplicable. For Keynes the explicit inclusion of *uncertainty* became the challenge and the key to a more realistic understanding of macroeconomic development. Uncertainty is present in social systems for many reasons, but especially on one account is social science different from natural science, that is people's ability to learn from previous experiences. Social behaviour is (partly) self-correcting through a cognitive process, which by itself makes it impossible – contrary to laboratory trials – to repeat the experiments in an unchanged form. Every macroeconomic study must therefore be evaluated in light of the present context and people's past experiences. An assessment of the context as well as past experiences are crucial for determining the generality of the conclusion being drawn from the study in question.

In a macroeconomic research programme based on critical realism the researchers set themselves the task of understanding the 'external' reality and describing the structures and causal relationships that can substantiate (and explain) *observed* developments within the macroeconomic landscape. This scientific work can most advantageously be conducted as cooperation between a number of social science

disciplines. Concepts such as power, institutions, and social structures are fully understood in a concrete, historical context where economic, political, legal, physical and cultural factors are intertwined. The search for universally valid, context-independent macroeconomic ‘laws’ is therefore doomed to fail (Hoover, 2001).

It is here that methodology enters the picture. However, before we begin this discussion, it is important to round off the section on *ontology* by emphasising that it is the open, stratified and holistic perception of reality, and the science-theory implications that derive from it, that are the distinct ‘trademark’ of critical realism. The true science-theory challenge then consists of developing a theory and method that can bind these three substantial strata together. This is a prerequisite for uncovering the causal mechanisms resident in the macroeconomic landscape that manifests in observed events – perhaps even in the form of a statistical trend.

The level of ambition within macroeconomic science should be high, but the results concerning the understanding of the deep stratum will rarely be able to live up to such a high level of ambition. The ontology of the target field is often too fluid and our understanding of the deep stratum too diffuse for this. So, it is all the more important to employ a science-theory strategy that is based on a continuous, open and critical discussion. There are no pre-programmed answers here and therefore no easy answers to macroeconomic questions. To sum up in brief: there can be no ‘critical realism’ without ontological reflection.

Critical Realism: Ontology + Epistemology → Suggested Causal Relationships

Lawson’s methodological reflections based on critical realism are the basis for the heading’s three parts: ontology, epistemology and causal relationships. The

methodology sets up a logical sequence. 1) Describe the characteristic structures of the target field assessed in relation to the cardinal question: ‘what are we looking at?’. 2) Move to the more practical approach: ‘given the social ontology, how do we organise the analysis in a consistent way?’ 3) The answers to 1) and 2) constitute ‘what kind of new knowledge can be achieved from the analysis’.

On the analytical level, we are looking for a method of theory-construction that can form the basis for developing hypotheses about causal mechanisms that can substantiate and explain tendencies in the factual stratum, which in practice means the most robust empirical relationships. As Lawson emphasised (1997), we are not looking for theoretical consistencies of the type *whenever x, then y, without exception*.²¹ The actual social ontology is simply in most cases an obstacle to the discovery of such *precise* predictions. In this way, critical realism challenges Friedman’s methodological conclusion that the accuracy of predictions is the best criterion for assessing the quality of analytical models.²² So *instrumentalism* is rejected, since it does not attach importance to the matter of securing congruence between ontology and epistemology.

The idea that a laboratory experiment can be used in macroeconomics is, as explained above, for similar reasons regarded as methodologically misleading from the perspective of critical realism, because there is very little congruence between the open ontology of the macroeconomic landscape and the epistemology of predictions independent of the unrealisticness of assumptions (Mäki, ???). Critical realists would say that the methodological notion that a laboratory trial can be used as a general macroeconomic method is *misplaced concreteness* (Daly, 1997). The ontological basis for controlled experiments is rarely, if ever, present when the macroeconomic landscape

²¹ This issue will be explored in the section about the use of econometric methods.

²² Described in *Essays in Positive Economics* (1953).

is researched (cf. below on the difference between open and closed system analysis). In addition, it would for practical reasons be impossible to conduct a series of identical, macroeconomic experiments that would be numerically sufficient to reduce the statistical randomness related to any laboratory experiment. Instead, macroeconomists must work with observations from time series. This is problematic in itself because of the changing macroeconomic landscape. This will be discussed separately in chapter 6.

On the practical level, therefore, it is trivial that predictions will seldom be fulfilled. The crucial criterion, therefore, is not the precision of the prediction, but its relevance to the work in World 3 at the ‘political’ level. It is of vital importance, in this connection, to understand the qualitative difference between working with *open and closed systems*, respectively. It was a failure to acknowledge this important difference that helped to bring about the collapse of the ‘great macro-econometric models’ in the early 1970’s. However, it was not a critique of the closed and mechanical nature of these models that was prevalent at the time, but rather, as described in chapter 1, a critique from the neoclassical economists that the models lacked a basis in axiomatic micro-theory. From this perspective, one could say that the macro-econometric models were not closed enough. They were accused of being specified in too *ad hoc* a manner, which reduced their range of validity in a forward-looking perspective. This was the core of the so-called Lucas critique (see Lucas & Sargent, 1979). They claimed that the most stable socio-economic parameters could be found in microeconomic behaviour, in the form of constant consumption preferences and production conditions.

The critique by Lucas and Sargent presented here can be directed at every form of scientific work that bases itself on simple verification of the past and of theories that are limited to the factual level. The Lucas critique is correct on this methodological

point: that empirical regularities should be explained by stable causal mechanisms that are rooted in ‘the underground’ and referred to in neoclassical terminology as ‘deep parameters of individual preferences’.

Seen from the perspective of a critical realist *methodology*, it is equally important to recommend that phenomena from the ‘deep stratum’ are included at both the real and the analytical levels, recognising that the observed surface phenomena must necessarily be dependent on the underlying structures. These structures can be of a behavioural or institutional nature. However, there is a tricky methodological problem associated with pinpointing these causal mechanisms: they are often non-observable and under constant change. The empirical material that we have readily available is macro-data of varying quality. The statistical correlations uncovered are in any case contextual and often characterised by random occurrences, since the underlying ‘mechanisms’ are not necessarily constant over time, as there were changes in the river of Heraclitus. The main reason for the sceptical attitude towards statistically established correlations as seen in Lawson (1997) and others, is their ontologically superficial and analytically random characters. So, statistical correlations cannot stand alone. They are only meaningful when supplemented with a theoretical, explanatory model that corresponds to the concrete macroeconomic ontology. However, statistical tests, if interpreted with respect to the underlying statistical material, can be a bridge between the analytical and the factual levels, inspiring further work to discover the causal mechanisms in the deeper stratum. In this way they can become an important input as part of a retroductive working method.

Retroduction

We are now ready to assess the science-theory working method, which is a combination of induction and deduction, so-called *retroduction*, that Lawson recommends (cf. also Nielsen and Buch-Hansen, 2005) as a procedure of the critical realist methodology in developing social science theories. Retroduction starts with an ontological reflection; but where do the organising categories for this reflection come from? Here of necessity a significant amount of previously acquired experience and conventions is used. This is a preliminary characteristic of the target field, that should subsequently be investigated with the aim of improving the knowledge base. This reflection should, at the very least, not be in direct empirical conflict with observable data.

It is possible to deduce (preliminary) theories on this (preliminary) empirical base, preferably by including bold hypotheses concerning the structures in the deep stratum. These theories must then be confronted with reality through a constructive falsification test that can be quantitative and/or qualitative. An indication of the theory's range of validity can be achieved in this way. Not least, the limits of the range of validity can inspire clarification and further development of our understanding of the causal mechanisms. This empirical testing is of an inductive nature. Should the same phenomenon appear repeatedly, then the macroeconomist, with inspiration from Hume as well as Lawson (2003:145-6), should ask '*Are there reasons to believe that all swans are white?*'. Which underlying mechanisms could have brought about this seeming regularity? It is questions such as these that must be answered through a retroductive practice so that we can obtain new knowledge, rather than merely observing a statistical correlation.

The retroductive practice is based on an interaction between ‘common sense’, deduction, observation and induction. It is especially important to have this interchange when uncovering causal relationships in open systems. This will also help to ensure correspondence between the real level and the analytical level, and thereby prevent the occurrence of epistemological errors.

It is important to recognise, therefore, that the essential mode of inference (practice of method) sponsored by transcendental realism is neither induction nor deduction but one that can be styled retroduction or abduction or ‘as if’ reasoning. (Lawson, 1997:24)This consists in the movement, on the basis of analogy and metaphor amongst other things, from a conception of some phenomenon of interest to a conception of some totally different type of thing, mechanism, structure or condition that is responsible for the given phenomenon (Lawson, 2003: 145).

Instead of seeing induction and deduction as polar opposites and therefore mutually exclusive practices, Lawson encourages us to consider these two very different principles for design of hypotheses as being complementary. *Retroduction* can be described as a method that includes the main elements of induction (observations and apparent regularities), which are subsequently given a (hypothetical deductive) theoretical foundation in respect for the ontological character of the target field.²³

In this respect, retroduction is clearly distinguishable from pure deduction, which is briefly outlined in appendix 1, as an axiomatic logic without real empirical testing of the selected axioms. Retroduction²⁴ on the other hand, combines the observed regularities (induction) with hypothetical deduction (conditional inference), which can,

²³ It is acceptable that the explanation is counter-intuitive (cf. for example the savings paradox) but impermissible that it be in conflict with empirical observation. The theory may conclude that the sun is the solar system’s gravitational centre, as long as the theory also can explain why ‘the sun moves across the celestial sphere’.

²⁴ The more I work with these terms, the more I find that the word ‘reason’, or in Latin ‘*ratio*’ covers this methodological practice, which bases itself on applied sense (that which we in generally refer to as ‘common sense’): ‘Are there reasons to believe that x has been caused by a mechanism (let us call it ‘ f ’) mainly depending on y, \dots, z ? Are there reasons to believe that the tendency behind $f(y, \dots, z)$ also will be valid in the future taking properly into consideration that (a) the system is open, (b) the structure is uncertain and (c) causal relationships are stochastic (with a hardly known mean and variance)’. Lawson (1997).

for example, be stochastic. Induction helps to ensure correspondence with ‘the reality of life’, while deduction can maintain a logical consistency in the development of theory. There is not *one* particular approach that is correct, but the selection of the method of analysis is of critical importance and should therefore be given adequate attention. It should be the character of the ‘problem area’ that (co-)determines how the analysis is conducted in practice; cf. Lawson’s metaphor of the pneumatic drill.

Lawson often uses metaphors to suggest phenomena in the deep stratum, which by their nature cannot be subjected to direct observation. This gives an apparent parallel to Friedman’s instrumentalism; but it is an illusion, since the ambition of critical realism is to replace metaphors with actual, realistic explanations of causal mechanisms. The better (more realistic) the theories of ‘macroeconomic behaviour’ that can be established, the more the use of ‘as-if’ metaphors can be forced into the background. It is unlikely that they can be completely removed, however, since a lack of knowledge (and observations) forces us to work with an open (and therefore partially underdetermined, not to mention non-ergodic) explanatory model of the underlying (and presumably open) structures. As a part of the critical realist methodology, there will always be the speculative ‘as-if’ element serving as a hypothetical explanatory element.²⁵

Lars Pålsson Syll (2001), one of Sweden’s most enthusiastic advocates for the use of critical realism in socio-economics, introduces a section in his book on *Economic method* with the title *Vad är en relevant förklaring* (i.e. ‘What is a relevant explanation’) in the following way:

²⁵ The use of ‘as-if’ assumptions, the somewhat archaic use of language and the assumption of a transcendent level, have together contributed to giving critical realism linguistic trappings that upon a superficial reading point in the direction of rhetoric, as used within the post-modern tradition. It is possible that this led to Blaug’s (2003) aforementioned confusion, and consequent rejection, of critical realism as an irrelevant economic methodology.

No clear criteria can be found for what a satisfactory explanation should be... (On the contrary) a relevant explanation should be correct in the observation, that it is in accordance with reality and that it should be useful (ibid: 112).²⁶ (My translation)

From theory to practice

This chapter should have explained the methodological criteria for obtaining relevant knowledge about macroeconomic reality. It includes a lengthy discussion of the importance of ensuring correspondence between the real level and the analytical level. These two levels cannot be separated within macroeconomic science, which ought to be reflected in the methodological practice. This argumentation can, without difficulty, be developed to include all social sciences, since the methodological levels are interlinked, just as the various disciplines are difficult to separate completely. Economics, politics, sociology and law are artificial divisions when one paints with a broad brush. The disciplines are socially embedded and exercise mutual influences. Yet, to make this thesis more specific, I have decided to focus on the macroeconomic domain, which can help give the methodological considerations a more concrete, and consequently, operational character.

There is also the fact that macroeconomic analyses must be context-dependent. What field of socio-economics are we looking at, and how can the general socio-economic relationships be described? A contextually-embedded macroeconomic landscape will therefore be presented in the next chapter, not as a fixed, unchanging framework for analysis – quite the opposite. I would better call it a type of reality checklist.

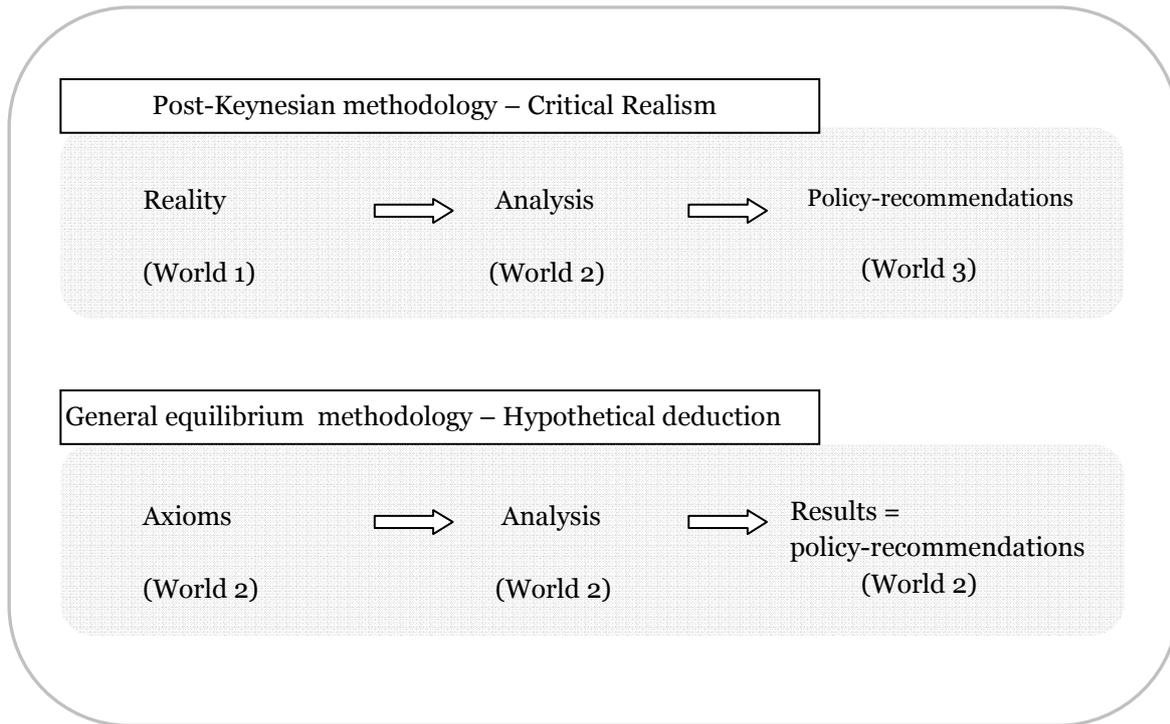
²⁶Here Pålsson Syll refers to Sheila Dow (1996: 18) and others as supporters of this view, and thereby underlines the affinity with post-Keynesian methodology.

Reality – the round trip

In the above argumentation, the idea has been put forth that when realism serves as the basis for macroeconomic methodology, the analytical level cannot be viewed in isolation. It is from our image of reality – the ontological reflection – that the activating questions must spring. These questions should be answered on the analytical level in a constant interaction between theory, model formulation and empirical testing – the so-called retroductive process. The result of such a contextual analysis must finally be ‘brought back’ to reality where it is intended to be utilised (World 3). What can we the social scientists conclude, as an answer to the introductory question, and with what (un)certainty and limitations can the answers be formulated? The model of analysis is not reality, so the results of the model – the new knowledge – must, to a certain extent, be brought back to reality. There is a methodological gap here which can easily be overlooked. It occurs (too) often that there are just two lines drawn under the analytical results. This is ‘the most qualified answer’ to the question posed. In this way, the analytical level and the operational level are equated, so that the analytical results are left unmodified. The absolutely necessary, yet often unanswered, question is how do we get from the analytical level to policy recommendations while maintaining a scientific basis?

Let me illustrate this problem with a figure:

Figure 2.3: Two different methodologies



The analytical level (World 2): Axioms, analysis and results

As is shown in figure 2.3, the activities in World 2 (the analytical level) play a dominant role within general equilibrium **macroeconomics**.²⁷ The analysis centres on the mathematical formulation of the axiomatic basis with maximising individual behaviour, full predictability, market clearing and long-term equilibrium, all of which are predetermined axioms. In Lakatos’s terminology, these axioms constitute the ‘hard core’ of this research programme which cannot be challenged and therefore have never been subjected to actual falsification. It is upon this *analytical* basis that the mathematically formulated general equilibrium model has been developed and discussed. And this shared axiomatic foundation must be the basis for Andersen’s (2000) statement that ‘there are practically no methodological differences within

²⁷ Note that I constantly underline that it is **macroeconomics** that is the object of my analysis. For me, one of the more dangerous generalisations can be seen in Lawson when in fact he is largely concerned with microeconomics and unconditionally calls his books *Economics and Reality* and *Reorienting Economics*’.

macroeconomic theory’, which is why ‘the method is not the message’; only one research programme is recognised by the ‘mainstream’. As long as the axiomatic foundation and the hypothetical-deductive method are not challenged, there is simply no methodological difference that can be questioned. The discussion of methodology plays out within a narrowly defined World 2. Consider also Andersen’s remark about the lack of realism behind the assumption of, e.g., rational expectations (which, however, does not dispute the fact that policy recommendations can be directly derived from the results of analysis). A mathematical, deductive model cannot be ‘wrong’ if the mathematical operators are used correctly. The postulated theoretical relationship and the dynamic structure can be given various mathematical representations, though never more different than that these models all converge towards general equilibrium.²⁸ The last condition is of course also axiomatically determined. The general equilibrium models, therefore, rarely overstep the boundary separating the analytical level from the real level. The analysis consists primarily in finding *the* solution to the closed model under different structural conditions. The analytical outcome is quite often a demonstration of the ‘distortions’ caused by external effects and government regulations compared to the perfect competitive market model, and policy recommendation consists of the so-called welfare gains which can be obtained if these distortions were eliminated.

The *post-Keynesian school*, on the other hand, maintains that it is necessary to include *fundamental uncertainty* that characterises the real world, to the analytical level. It is not the question of individual rationality that is debated, but rather how the macroeconomic representation of individual behaviour, subject to uncertainty, can be given a realistic, operational and rational representation. The ontological reflection does

²⁸ ‘The models must have “appealing characteristics”; if one does not ensure that they converge upon equilibrium, one cannot know where they end up.’ Such was the reasoning I was given for the assumption of long-term equilibrium during a verbal discussion.

not provide any immediate justification for assuming that individuals do *not* behave rationally on the basis of the knowledge they possess, and the norms and habits that make up their social and historical reality. The core of this post-Keynesian discussion of methodology is rather about how *individual*, rational behaviour can be represented within the framework of an adequate macroeconomic model so that the existence of fundamental uncertainty is attributed the analytical importance it requires. I will address this question below and assess to what extent a mathematical formulation of macroeconomic behaviour under uncertainty is a relevant method of analysis (cf. Chick, 1998).

Precisely because the post-Keynesian school gives decisive significance to uncertainty in its ontological description, it has been a challenge to the theory of science to achieve correspondence between the ontology and the choice of a method that can analytically include fundamental uncertainty on both the individual and the structural levels. It is precisely on this point that *critical realism* has caught the attention of post-Keynesian researchers, since this school of the theory of science, as already mentioned, strives for congruence between ontology and epistemology.

The post-Keynesian macroeconomic landscape is therefore assumed to be ‘populated’ with rational actors equipped with limited (and uncertain) knowledge. These actors act within a structure of macro-institutions, norms and habits in which explicit and implicit individual and social contracts are established. Through these contracts, a varying level of regulation, redistribution and limitation of macroeconomic uncertainty is achieved. These structures co-determine the causal mechanisms that drive macroeconomic development. But as Keynes already pointed out in 1934, see p. xx, there is nothing that *a priori* promises that the causal mechanisms will interact in such a

way as to justify the existence of long-term equilibrium, much less a long-term equilibrium with full utilisation of resources; and even if such an equilibrium did exist, there was no real probability of it ever being reached at the macro level through the market mechanism. Nothing can be stated *a priori* on this subject, but past macroeconomic experience does not demonstrate empirical support for the relevance of such conditions. Therefore, in *The General Theory*, Keynes changed his stance on analytical method in relation to his earlier books and abandoned the assumption that long-term market-clearing equilibrium was empirically relevant. He did so on the basis of an intensive ontological reflection, which at the beginning of the 1930's brought him to the preliminary conclusion that even a well-organised market-economic structure did not necessarily include the realisation of long-term general equilibrium. But, as already mentioned, the altered macroeconomic reflection was still based on the assumption that the individual actors behave rationally, given the knowledge they have about present macroeconomic developments and about future individual behaviour, cf. chapter 4.

Building bridges between the real and analytical level

A theory-of-science orientation that calls itself critical realism must naturally have a theory for how 'reality' can be included as a part of the overall methodology. The real level and the analytical level cannot be kept separate. Critical realism is characterised by the existence of a constant interaction between observations and analysis that provide opportunities for new and 'deeper' understandings of the basic causal mechanisms. Although we never achieve a complete understanding of reality, the ambition is to improve our understanding. Let us briefly include the Popperian perspective. Scientific results should be characterised by the fact that the underlying, analytical proposition as a part of the research process has been confronted with the part

of reality that is observable one way or the other. This research procedure is a part of embedding the analytical level into the real level. If the hypothetical statements cannot be rejected on the basis of the available empirical data, then we have expanded our knowledge of reality.

Methodologically, it is a serious challenge to cross the divide between reality and analysis, since here the researcher moves from being an observer to being an operator. Here, stylised observations are combined with theoretical models so that a broader, yet also more abstract, cognition can be reached in the form of analytical results.

I have called the initial operation of this retroductive process for an ontological reflection in the form of drawing up a sketch of a macroeconomic landscape a kind of 'mapping'.

Next follows the formulation of hypotheses and empirical testing, which will later often be followed by necessary reformulations and more testing. These results must then be brought into harmony with reality, taking the most demanding assumptions into consideration. To what extent do they compromise the generality of the analytical results? Are the results relevant for the formulation of policy recommendations at the real level of World 3, where they will form part of the basis for decision making? Some relevance could eventually be achieved through conditional, path-dependent projections of a limited scale.

If the ambition is to reach results that contain relevant statements about reality, it is important that the analysis is not begun with clearly unrealistic axiomatic foundations. For such assumptions cannot avoid distorting the results in relation to reality, whereby they lose their generality. This was for example the background for

Keynes to call his macroeconomic theory *The General Theory*, as it included ‘neoclassical’ general equilibrium (full employment) as a special case.²⁹

The design of an analytical model is an important, although in the broader methodological perspective limited, issue. It is only included here to show that the disagreement does not lie exclusively in whether but also *how* the divide between the real and the analytical level (where some theoreticians are exclusively located) can be crossed.

The analytical level is subordinate to the real level, in more ways than one. The important assumptions on the analytical level – not least the axioms – should also be evaluated on whether they are ‘realistic’. All assumptions are to varying degrees unrealistic. A classic example of such an unrealistic assumption is ‘perfect foresight’ or permanent ‘market clearing’, which on the other hand have a crucial impact on the analytical result.

It is here that Popper’s scientific method comes into the picture. His requirement for falsification testing should be taken seriously, as it is the most important demarcation between science and ideology. This requirement is relatively easy to formulate, but as Blaug (1980) pointed out, it is often more difficult to perform in practice. The consequence of a positive outcome from a falsification test (that the hypothesis in its current form must be rejected) should not be over-interpreted, since all hypotheses/analytical results are ‘false’, in a theory of science perspective. A demand for full agreement between reality, theory and empirical tests would inevitably lead to scientific nihilism. This is a view that a number of Popper’s critics have attributed to

²⁹ Keynes writes near the end of the *General Theory*: ‘Our criticism of the accepted classical theory of economics has consisted not so much in finding logical flaws in its analysis as in pointing out that its tacit assumptions are seldom or never satisfied, with the result that it cannot solve the economic problems of the actual world’ (p. 378).

him, while Popper himself is more concerned with the strength of empirical corroboration that can be attributed to the theory based on the available evidence.

Critical realism seeks to unite reality (World 1), analysis (World 2) and practice (World 3) through the acquisition of new knowledge that is constantly confronted with reality. It is a methodology that should be used in a complex world with conflicting interests and an incomplete understanding of reality.

This is also the macroeconomic methodological challenge.

Appendix 2.1

An example of the hypothetical-deductive method, limited to the analytical level within ‘modern’ macroeconomic theory – the general equilibrium model.

One of Denmark’s most acknowledged macroeconomists, Torben M. Andersen, stands as an exponent of hypothetical-deductive methodology. Here the analytical model is represented by idealised macroeconomics. In 2000, he wrote a review article on the status of modern macro-theory. He presented on that occasion the methodological foundation as follows:

‘The purpose of theoretical analyses is to construct a laboratory for testing various hypotheses. Modern macro-theory has the following methodological similarities:

- ... [it is] based on the assumption of (individual) optimising behaviour, that is systematically driven by economic incentives. (If this were not the case, then the problematique falls outside of the economist’s realm of expertise.)
- ...the analytical framework consists of general equilibrium models.
- ...individuals have an infinite timeline (or an overlapping generations model for identical agents).
- ...individuals maintain rational (model-consistent) expectations.

The aspects of method enumerated here serve the purpose of giving the analysis consistency and discipline, insofar as we wish to discover if a given problem can be described as a variation in the systematic economic behaviour. In some parts of the newer macroeconomic literature, there is a tendency to say that ‘the method is the message’. This is misleading. The selection of method gives the analysis discipline and consistency, and thereby demarcates a ‘laboratory’ for the analysis.’ (Andersen, 2000:21-22, my translation).

The methodological foundation for so-called ‘modern macroeconomics’ is described in an admirably precise way. The mathematically formulated general equilibrium model constitutes the practical device for developing and testing hypotheses concerning the understanding of macroeconomic development. In the laboratory of the thought experiment, the social ontology plays no direct part; all results are measured according to the ideal. The connection with reality is conveniently replaced by a non-existent ideal. It is, in the true meaning of the word, a closed model, where everything is under control. It could not be written any clearer than here, that the method defines the practice upon which the analytical results depend.

In short, the neoclassical school has opted to place its main emphasis on the hypothetical-deductive method, with its theoretical-scientific roots in the tradition of logical positivism. Neoclassical macro-theory is built on a foundation of a few fundamental hypotheses/axioms concerning: (1) individual rational behaviour, (2) market clearing and (3) a stable long-term equilibrium. Work is conducted within the method-related confines that Léon Walras established in the 1870’s, to be later perfected in the Arrow-Debreu models in the 1950’s. The model-related foundation is a deductively derived general equilibrium model from which the subsequent macroeconomic analyses are conducted (cf. Andersen, 2000). This research strategy means that already in the background, a dissonance exists between the subject’s ontology, characterised by macroeconomic uncertainty, and the practised epistemology, seen in the founding method of analysing by means of a deterministic (closed) system. The laboratory model is, in a scientific-theoretical perspective, intended to be closed, again because the intention is to conduct controlled experiments. The degree to which macroeconomic uncertainty can be explicitly included in such ‘laboratory trials’ is discussed in chapter 5. Is a deterministic model relevant for analyses where ontological uncertainty plays such a dominating role? The degree to which the basic axioms and the method employed can limit the results’ generality in relation to reality will similarly be discussed in a later chapter in relation to the meaning of the so-called ‘fallacy of composition’ in macroeconomic theory. This particular discussion requires that an alternative scientific-theoretical methodology is made explicit.

Appendix 2.2

Lawson's four critical theses against 'modern economics' practised within the framework of 'the laboratory model'

Thesis 1: Academic economics is currently dominated to a very significant degree by a mainstream tradition or orthodoxy, the essence of which is an insistence on methods of mathematical-deductive modelling.

Thesis 2: This mainstream project is not in too healthy a condition.

Thesis 3: A major reason why the mainstream project performs so poorly is that mathematical-deductive methods are being applied in conditions for which they are not appropriate.

Thesis 4: Despite ambitions to the contrary, the modern mainstream project mostly serves to constrain economics from realising its (nevertheless real) potential to be not only explanatorily powerful, but also scientific in the sense of natural science, (Lawson, 2003: 3)

As has been put forth in the four theses stated above, Lawson is sceptical (to put it mildly) in his assessment of the relevance of the work being conducted within the economic laboratory's four walls. His main objection is the exaggerated use of mathematics on a social ontology that is not suited to analysis by the use of mathematically formulated models. It requires that the elements and reciprocal relationships included in the analysis are deterministically defined, that 'the area is closed off' from further influences, and agents act individually and repetitively. These are conditions that are poorly reflected in the ontological reality, which Lawson hopes to be able to observe:

My concern at this stage, though, is to emphasise that with mathematical methods being insisted upon by the mainstream but regarded as inessential by heterodox traditions and others, we can see that the various strands of orthodoxy have not only a common, but also a distinguishing, feature after all. This, as I say, just is the insistence that the mathematical-deductive methods be used in just about all endeavour to advance knowledge of phenomena regarded as economic... (Lawson, 2003: 8)

Lawson focuses on the use of mathematics and the precedence of the deductive method as the demarcation line for whether mainstream economists observe a theory, a method and an analysis for 'economics' – see e.g. Varian (1999), cited in the introductory chapter.

Although I (in line with a number of Lawson-cited and internationally renowned economists – including Nobel prize winners) often feel that the requirement to use mathematical deduction has gone too far, not least because its use goes beyond the relevant and valid domain of deductive method in economics, this is *not* the debate I wish to have here. It is, in the end, up to each and every researcher to ensure congruence between ontology and method. It hardly brings the science of economics any further to quarrel over what is or is not the right method in economics. Basically though, I do not see a demarcation line determined by the method as being so central, in that nearly all economics uses symbols and logical operators – some formulated verbally, others with Greek letters. As long as one method is not forced upon the whole of macroeconomic reasoning. In that case the question is not so important. This point is aptly described in Chick's work (1998), 'On knowing one's place: Formalism in Economics.'

I would rather see the requirement of correspondence between ontology and epistemology formulated in a more explicit way, by developing macroeconomic theory and models used as a basis for the eventual categorisation of macroeconomic theory. I am here influenced by Keynes's clear distinction between 'economics' which has an explicit ontological assumption that the market is self-regulating, and 'economics', where the social ontology is under constant change and the development is path-dependent without being self-regulating. However, in his 1936 book, Keynes portrays the equilibrium model as an integrated special case, as a part of a new, open-system ontology. The open-system is thus an overarching term, wherein closed equilibrium economics can be used, where a number of quite often rather unrealistic assumptions must be fulfilled. I find this to be a better explanation than giving a very formal analysis of why 'closed equilibrium economics', in the best-case scenario, has such a limited usefulness (at least within macroeconomic theory). I will expand upon the issues surrounding the use of formalised analysis in macroeconomics in the appendix to chapter 6.