Deichsel • The Usefulness of Truth

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The Usefulness of Truth

An Enquiry Concerning Economic Modelling

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1. INTRODUCTION

This is a book about economic methodology. To sum it up in one sentence, this book tries to justify a normative role for methodology by sketching a pragmatic way out of the dichotomy of two major strands in economic methodology: empiricism and postmodernism. As with every one-sentence summary of a complicated subject, this might cause more confusion than clarity, and therefore a more detailed synopsis will be provided in this introduction. In order to avoid possible confusion right from the beginning, it is helpful to state first what I will not attempt to do. As I said, this book is about methodology, and this means that I will not try to write a prescription for how economics has to change in order to become a better science (or a proper science at all). Rather, I will discuss several methodological approaches and assess their aptness for theory appraisal in economics. This is not another book about what's wrong with economics. A central point of the works in this tradition is their presumption that mainstream economic reasoning is methodologically flawed and that economics can only be saved by adopting a new methodology.[1]

The aim of my work is not to develop a shiny new methodology for economics that promises to solve the many problems economic theorising has. My aim is more modest: I try to show what fruitful criticism of economic models[2] can look like. As it turns out, fruitful criticism cannot fundamentally contradict the basic premises of economics, as sketched out below.[3]

My approach to arrive at this is rather simple: I start with the most common views on methodology – empiricism and postmodernism – and then argue why they are ill-suited for giving methodological prescriptions to economics. After that, I look for positions that avoid the errors of empiricism and postmodernism. I will first identify why these two major strands of methodological criticism fail to give helpful methodological advice to economists and second, I will sketch out a pragmatic approach that can do this. More philosophically speaking, I will venture into the defence (and the limits) of a normative role for methodology.

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1 See e. g. Reiss (2007) for a recent work based on this kind of argumentation.

2 I follow Kleindorfer/O'Neill/Ganeshan (1998) in seeing models as *»miniature scientific theories«*.

3 This is the main reason why I do not adopt here a criticism that seems acceptable for individuals: It states that economics is normatively on the wrong track, because empirical results show quite convincingly that the maximisation of satisfaction of individual needs is not what makes people happy. While this may be true, it is a plain rejection of *the* basic assumption of economics, and so it has no constructive use. Besides, economists can defend themselves by pleading non-paternalism.

In spite of the rather philosophical approach, the target readers of this book are economists with an interest in methodological questions. Philosophers with some background in economics may be able to gain insights as well, although they should not expect a detailed discussion of traditional questions of science theory but rather a focus on theory evaluation in economics.

There is no simple way to teach how one should appraise economic models, and I will argue against any easily summarised cookbook-methodology. As is the case with many philosophical books, the benefits of this work probably manifest themselves more indirectly, through an increased awareness for methodological problems and a higher level of reflection when designing and interpreting economic models.

Throughout this book I will speak about »economics«, »neoclassical economics«, »mainstream economics«, »the rational-choice approach« and »the homo oeconomicus method«.[4] As a clear understanding of these notions is of crucial importance for my work, I will provide a characterisation right here. My definition of »economics« includes various types of models for different sorts of problems; I do not intend to define mainstream economics by its scope but rather by its method. The mainstream economic-research programme is well characterised by the following statements:[5]

1. Economics is about explanations for changes in macro-rates as the result of individual actions.
2. Individuals are assumed to maximise their personal welfare and are subject to boundary conditions. This is the so-called rationality assumption, which is as such free of empirical content, because the definition of »personal welfare« depends completely on the respective model and problem.[6]
3. Preferences of individuals are seen as constant. This is again not an empirical statement but a heuristic recommendation, because allowing for changing preferences would deprive economic models of any empirical content, as every outcome could be »explained« by a sudden change of

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4 Note that I do not reject the fact that there is heterodox research in economics that does not fit to the given characterisation. However, this drawback is unavoidable and is true for every definition.

5 My definition is inspired by Erlei/Leschke/Sauerland (1999), p. 51–53 as well as Homann/Suchanek (1989), p. 75 et sqq. and Lütge (2001), p. 85 et sqq. In section 2.3.1.3 you will find a more detailed definition of the economic research programme.

6 Note that critics of economics often assume the rationality principle to be the empirical claim that humans perform short-term income maximization. As we will see, this has been the source of many misunderstandings. The only empirical requirement for a rational-choice methodology is that humans react somehow systematically to external incentives. See Lagueux (2010) for a recent interpretation of the rational choice approach that is in line with my thought.

preferences.[7] Therefore, economics concentrates on changes of observable boundary conditions.

The above principles are not always explicitly referred to in every economic model. This is especially the case in models of macroeconomics, but as macroeconomics can be reduced to microeconomics[8] the above principles are still in use, even if hidden. With the preliminary remarks done, I will now sketch out an overview of the contents of this book, section by section.

The first section of this work starts with a summary of methodological positions that argue for more empirical work at the core of economics.

Basically there are two different demands from the empiricists: The first one demands that economic models must become more falsifiable and their results must be more severely tested. The second demand claims the behavioural basis of economics (i. e. the rationality assumption) must be enriched or replaced by more empirically founded theories of human behaviour.

This is the most common and best-known form of criticism against mainstream economics, which is why I will portray it in quite some detail and then spend considerable time analysing its shortcomings. Historically, the empiricist school was strong in economics in the German-speaking world, and the Methodenstreit in Germany ran in the opposite direction: The empirically oriented historical school was attacked by Carl Menger, who tried to establish methods that he hoped could lead to more generally valid relations.[9] Even if my presentation, which starts with a short account of the historical philosophical background of empiricism in modern philosophy of science (section 2.1), might suggest otherwise, there already was a lively debate within economics about the status of empirical research. So while the birth of modern philosophy of science has certainly influenced the debate, as can be best seen in the works of the economist Terence Hutchison, it would be wrong to see the demand for more empirical work as an completely external one, originally stemming from a philosophical discussion.

Section 2.2 presents the empiricist stance by condensing the claims of four methodologists of the empiricist camp: Terence Hutchison, Hans Albert, Mark Blaug and Scott Moss. Hutchison and Blaug mostly argue for taking falsification more seriously in economics, while Albert stresses both the importance of falsifiable theories and the need to work with more empirically founded behavioural assumptions. Scott Moss' position is unusual, because unlike classic economists, he works with agent-based computer simulations and claims the agents of simulations should be designed as accurately descriptive as possible and not be restricted by considerations of simplicity.

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7 Again, many critics misunderstand the status of the constancy of preferences, taking it as an empirical statement, and therefore attack it as false.

8 See Boland (1982), p. 141.

9 See Backhaus/Hansen (2000) for an overview of the *Methodenstreit*.

In section 2.3 I will present and discuss arguments against the empiricist claims. Section 2.3.1 is concerned with problems of defining »unproblematic background knowledge«, which is needed for both falsificationism and inductive empiricism. Interestingly, there is an essay by Karl Popper that already sketches out the main argument against falsificationism and empiricism at the foundation of economics: The rationality assumption is justified as an unempirical, heuristic device similar to the causality assumption. Thomas Kuhn's work gives additional insight that this view is indeed constitutive to economics and may be the main reason why empiricist critics are mostly ignored by practicing economists. Imre Lakatos's constructive falsificationism can be seen as a merger of Popper's approach with Kuhn's insights and stresses another reason why the theoretical core of economics has not changed despite constant criticism: there is (yet) no alternative that can constructively replace the rational-choice heuristic. The last subsection of section 2.3 elaborates on the problem of »unproblematic background knowledge« on a more abstract level and presents the concept of holism and coherentism and the Duhem-Quine thesis. These concepts undermine the naïve empiricist view that observation can be a firm foundation on which theory is built.

Section 2.3.2 shifts the focus to the problem of theory evaluation in economics by empiricist standards. First of all, there are practical problems for empiricist methodologies in the social sciences, because neither experiments with human beings nor whole economies can be controlled to the degree found in natural sciences such as chemistry and physics. The next subsection discusses the notion of »truthlikeness« and the connected conviction that scientific progress consists in an ever-increasing descriptive accuracy, in the sense of increasing the true consequences of theories and decreasing the false ones.[10] It is argued that this view has several internal problems which can be overcome by an instrumentalist understanding of theories. The next subsection elaborates on this idea and argues that theory appraisal can draw inspiration from economic cost-benefit analyses. This rejects the claim that theories should strictly focus on increasing their corroborated empirical content, but argues that the input for achieving this must be seen as a cost factor that is part of the evaluation. The discussion in this subsection draws inspiration from a recent debate in computational economics where empiricist arguments like those from Scott Moss are counterattacked with arguments that favour simplicity against empirical adequacy. The main point is that good theories should be able to »explain much by little«, which is essentially a cost-benefit argument. The last subsection confronts the empiricist convictions against a priori reasoning by giving examples of purely formal works that are uncontroversial parts of social science and, despite their aprioristic

[10] Note that true and false refers merely to empirically correct/incorrect in the context of the discussion about truthlikeness.

character, tell us something about the world: The impossibility theorems of social-choice theory are the most prominent examples of how purely formal reasoning can be relevant in practical contexts.

Section 2.4 presents case studies in order to relate the discussion more closely to actual practice in economics. The first case study is a longitudinal overview about the development of economic-growth theory. Starting with the classic Harrod-Domar models, it is shown how the switch to the growth theory of Robert Solow and succeeding innovations such as the »new theory of economic growth« and institutional-growth theory led to an increase in empirical content and predictive success without stepping away from a fundament that has virtually no empirical content. The second case study picks up the recent debate between experimental economists and rational-choice theorists. The main results of this case study are the futility of trying to refute the concept of the economic man, if it is understood merely as a heuristic device and not as an empirical statement. Most experiments merely refute the notion of human beings as short-term-income maximisers, a notion that has been rejected for long by rational-choice theorists as well. On the other hand, this is not to deny that experiments in economics can hint at important insights on how to refine economic models and which factors other than money-maximisation need to be integrated. So experimental economics has its merits, but despite its often-aggressive rhetoric, it offers no replacement for rational-choice methods.

Section 3 presents the next step in the methodology and philosophy of science, resulting directly from the problems of empiricist methodologies (as discussed in the previous chapter): postmodern relativism. In section 3.1 developments in the philosophy of science that led to postmodern, relativistic positions. Willard Van Orman Quine was the first philosopher to have had considerable impact on the demise of classic empiricism and even if he surely was not a postmodern relativist, his ideas have been taken up and modified by later postmodern philosophers. His holistic conception as an alternative to foundationalist epistemology is laid out in more detail here than it was in section 2, where only the critical parts were stressed. Nelson Goodman adds a far-reaching relativism to the antifoundationalist programme by stressing the constructive aspects of the way we perceive the world and hinting at possible alternatives. In contrast to Quine, Goodman does not grant science a privileged role in the process of gaining knowledge. Consistency and pragmatic usefulness for achieving the given tasks are, however, criteria that Goodman still defends. Paul Feyerabend defends the most radical relativism and argues for the abandonment of all standards by means of historical examples of scientific advances that have been achieved by ingenious breaks of rules rather than sticking to standards. Feyerabend argues that even inconsistency is no legitimate argument against a scientific theory and illustrates this argument by pointing to inconsistencies in quantum theory.

Section 3.1.2 presents the two best-known postmodern positions in economic methodology that clearly draw their inspiration from the philosophical movements described above: Bruce Caldwell's pluralism and Deirdre McCloskey's rhetorical approach to economics. Both positions recommend refraining from normative issues and making the philosophy of economics a purely descriptive project. Caldwell's pluralism is an attempt to understand various methodologies on their own terms, and McCloskey puts forward that there is much to be learned about the practice of economics through using the tools of literary criticism to analyse economic papers.

In section 3.2 I start with a critical evaluation of postmodernism. Mirroring the above structure, the most fundamental philosophical arguments come first. In subsection 3.2.1 I present and assess the arguments of three authors who have attacked postmodernism from different perspectives. Paul Boghossian's recent work »»against relativism« is a systematic approach to refute basic postmodern convictions. In my discussion, I show why his arguments fail to hit the target. The main reason is that constructivism is an irrefutable position.[11] The only way to refute it would be to demonstrate convincingly that we can be sure to have true knowledge about something. As long as this can't be done, it seems legitimate to replace »truth« with the notion of »belief about truth«.

Daniel Dennett's attacks against postmodernism suffer from a similar problem. All arguments in favour of science and against other belief systems (such as religious ones) are necessarily based on scientific premises. This is true for the evolutionary arguments Dennett gives in favour of science, as credible as they might sound to our ears. Dennett even accepts that science cannot claim to have knowledge about what he calls global truth, but he is convinced that this has no bearing on the local truths it produces. For him, scientific truths are the only acceptable local truths. He takes the success story of science as the main argument for this, but this is of course a cyclical argument: It may well be that science is so successful only because it is the ruling belief system today – then its success would of course be success only on its own terms. Again, the postmodern assumption about the radical description-dependence of truth may not sound plausible, but it is hard to refute.

The last argument against postmodernism does not attack the basic premises but aims to demonstrate the bad effects of postmodern thought by means of a hoax. Alan Sokal managed to publish an article that was packed with intentional errors in the peer-reviewed postmodern journal Social Text and claims that this proves that a postmodern, relativist attitude leads to a lack of standards. While this does not hurt postmodern methodology directly, it

11 As an irrefutable position it could still be an absurd position such as solipsism. The arguments against Boghossian therefore do not show that constructivism is correct, they merely show that Boghossian did not refute it.

suggests that the rejection of universal global standards can lead to a considerable weakening of local ones. Therefore, it may be the case that even an unjustified belief in the existence of global truth is pragmatically preferable to postmodern relativism about truth.

Section 3.2.2 finally turns back to economic methodology. Caldwell's pluralism suffers from the fact that he does not completely endorse the postmodern premise of global relativism. This already shows up in the title of Caldwell's book Beyond Positivism – but a true pluralist cannot claim to be »beyond« anything. I also critique Caldwell's lack of clarity in evaluative standards, because some aspects of his work can be found, where he clearly allows for more than description or internal criticism, but remains unclear what the sources of his evaluations are. While the concept of pluralism may be irrefutable, Caldwell's specific elaboration obviously has internal problems. McCloskey's rhetorical analysis is a much more prominent case, but it is not free from problems, either. Despite her plea for a purely descriptive methodology, McCloskey has loaded her books with recommendations for how economics has to change. This seems contradictory at first sight, but McCloskey's point can be defended by taking her recommendations as purely subjective and local ones: Her criticism of modernism does not necessarily lead to the abandonment of normative voices, but it can lead to allowing *everyone* to give prescriptions – because McCloskey rejects the possibility of being in possession of a privileged position. Even the fact that she doesn't stick to her own standards of Sprachethik does not critically hurt her position, as this argument contains a tu quoque fallacy. While it is hard to refute McCloskey's postmodern view that economics should be evaluated not by its correspondence to external facts but rather by its rhetorical persuasiveness, her approach – using the devices of literary criticism to analyse economic papers – is not convincing even by her own standards. Even if her analyses are entertaining and insightful to read, she has as of yet failed to persuade economists or methodologists that persuasiveness is a good standard to adopt.

Section 3.3 sums up the results of chapter 3. It turns out that the postmodern rejection of global Truth cannot be refuted, but this does not necessarily lead to giving up prescriptions on a local level. When it comes to economics, however, the two most prominent postmodern authors fail to give useful and accepted advice even on a local level. They fail to achieve their self-set goal of improving the critical discussion of economic models.

The last major section tries to overcome the dichotomy of empiricist and postmodernist methodological positions by offering a pragmatic way out. Where postmodern methodologies are often based on their rejection of empiricist positions, there is no principal reason why empiricist arguments should play no role on a local level. The most promising way towards a useful concept of theory evaluation seems to look first for a characterisation of economics that economists can accept and then search for quality criteria that are in line with that description. This rules out fundamental criticism, of course, but if the aim of theory appraisal is improving a critical discussion about models, fundamentalism does not lead very far – rather, it is a rejection of its basic premises. A pragmatic point of view that focuses on evaluating the quality of solutions for given problems[12] is much more likely to settle a discussion about models than are general methodological arguments derived from philosophical positions such as empiricism.

Section 4.1.1 draws inspiration from Daniel Hausman's seminal book *The Inexact and Separate Science of Economics*, which gives a constructive characterisation of the economic method and justifies the utility-maximisation assumption as a legitimately used a priori principle. Hausman argues that the impossibility of controlling for disturbing factors in economics is the reason why economics must be based on credible and pragmatically convenient assumptions about the relevant causal factors. This is contrary to empirical work on the basis of economics, but it also makes the falsification of the theoretical core impossible. Hausman argues that this core of economics is the reason why it is separate from the other social sciences – but it is also the source of its inexactness, as economic theory can only predict typical macrotendencies of behaviour and remains silent on the exceptions. Hausman offers a first step out of the dichotomy of empiricists and postmodernists, because he normatively justifies the core of economics by its pragmatic values. However, his reconstruction offers little insight for the evaluation of economic models.

Section 4.1.2 is essentially a pragmatic reinterpretation of Milton Friedman's classic methodological paper and is a first step towards filling the gap that Hausman left concerning criteria for theory evaluation. In my reinterpretation I first argue against the common misunderstanding that Friedman believes the assumptions of theories to be irrelevant. Additionally, in contrast to a widespread view, Friedman is not arguing for an instrumentalist view of science that makes prediction the only goal for theories. Rather, he offers a radically problem-oriented methodology that looks for the most efficient means to solve a given problem. Friedman's idea is to justify economic models by means of abduction, i. e. the claim that they are the best explanation for an observable outcome. The best explanation is not the one that is based on the most descriptively accurate assumptions, because highly accurate micro-assumptions make fruitful theorising difficult. Therefore, good assumptions have to be simple and lead to successful prediction of the phenomena

[12] Note that I am using the term »problems« in its broadest sense, so that even philosophical problems are »valid« problems. Note however that there are problems where »the quality of solutions« cannot be assessed yet – in such cases, pragmatic theory evaluation must suspend judgement.

under scrutiny. For Friedman, economic theorising is rational reconstruction, and good economic models explain much by little.

Section 4.1.3 takes a central idea of Friedman's methodology to the next level: it argues for an economic assessment of economic theories.[13] This leads to two separate developments that can contribute to a new, pragmatic way of normative reasoning in theory evaluation. The first idea is to apply the concept of cost-benefit analysis to theory choice. Economic theories of science have rendered Friedman's claim to explain much by little more precise by offering a radically problem-dependent way of assessing theories. Economic science theory is completely pragmatic when it comes to theory evaluation: It accepts there is no single right criterion for judging science, and so the only evaluative question that makes sense is whether a theory is the best and most efficient means to solve the problem it set out to solve. This differs from empiricist methodologies as well as from the postmodern, relativist approaches.

The second idea taken from economics and applied to the evaluation of science argues that the institutional structure of a science is the best starting point for improving its quality. From this point of view, science is seen as a collective process in which individuals seek to maximise their reputation and do not necessarily care much about good theories. Only via the invisible hand of the marketplace of ideas that is constrained by scientific institutions – such as the requirement to publish results, the higher recognition for work that has been anonymously peer-reviewed, and the winner-takes-all principle (among others) – can the selfish motives of individual scientists lead to epistemically desirable outcomes. This twist allows for accepting that various »irrational« social factors are interfering with science while still arguing for an epistemic privilege of scientific knowledge. In such a way, normativity has shifted from individual theories to a meta-level of analysing and improving the organisational structure of science as a whole.[14]

The next major section (4.1.4) gives a rather scattered overview about arguments that strengthen the pragmatic position.

First, I discuss more general arguments from Larry Laudan, Bas van Fraasen and the methodology of causal holism by Thomas Boylan and Paschal O'Gorman, which all offer methods of theory evaluation without falling back on the dogmatism of naïve empiricism.

In the next subsection I turn to pragmatic approaches to model building in economics. There is a detailed discussion about the justification of modelling practice in economics, accepting the pragmatic premise that nei-

13 This may sound cyclical at first, but I borrow an argument from Gerhard Vollmer to show why this circle is a virtuous rather than a vicious one.

14 Such a meta-analysis is neutral regarding the aims of science as it relies completely on means-ends rationality. In section 4.1.4.3 I discuss the status of some traditional criteria that define what distinguishes science from other enterprises and that are relatively stable over time.

ther pure empiricism nor postmodern relativism is the end of the story, and that a problem-oriented, normative discussion of the right means for solving given problems is possible. I discuss approaches from several authors such as Nancy Cartwright, Marcel Boumans, Eric Schliesser, Hal Varian, Mary S. Morgan, Frank Hindriks, Anris Vilks, Karl Homan, Andreas Suchanek and others that give subtle arguments in favour of the radical abstraction of mainstream economic models and, by this, help to develop quality criteria that are more fitting to economic models than the dogmatic demand for more empirical work.

The last subsection gives an overview of different traditional criteria for theory evaluation and how they can be applied to economic theories.

Section 4.2 is devoted once more to strengthening the pragmatic approach. This is done through an attempt to refute its main opponent: scientific realism. I start with a presentation of arguments in favour of scientific realism that are borrowed from the debate in general philosophy of science and then present the positions of the two best known realists in economic methodology, Uskali Mäki and Tony Lawson. In this discussion I lay out arguments for why the pragmatic approach is not hurt by these positions. Most notably, I demonstrate why a pragmatic defence for realism is ill-conceived. In a more detailed analysis, it turns out that most of Mäki's demands are compatible with the pragmatic approach that I endorse, while Lawson adopts a radical methodological realism that leads to problems similar to those faced by the empiricist methodologies (discussed in chapter 2).

Finally, chapter 4.3 takes stock of all of these arguments and tries to answer the crucial question of what is left for theory evaluation. To be sure, rule-based single-criterion methodologies are rejected, as methodologists are not in a privileged position to tell economists what to do. However, if they have some knowledge about economic methods, they can assess (just as well as economists) whether or not a model will genuinely contribute to solving the problem it set out to deal with. The criteria to judge this are often implicitly given by the description of the problem itself: E. g. if a model is meant to be useful for policy consulting, its predictions must be empirically validated; if it is merely a non-calibrated rational choice model, it can only claim to reconstruct long-run tendencies, though it may offer explanations for observable correlations. Models can be best criticised by asking whether they keep the promises they make[15]: This radical problem orientation solves many difficulties of theory evaluation. For example, theory-ladenness is less of an obstacle when science is not supposed to deliver objective descriptions but rather answers to problems, because posing a problem already presupposes and accepts much theoretical background. So problem orientation accepts that science does not start in empty space but is always embedded in a con-

15 Note again that this allows for purely theoretical models as well. However, it does not allow for making empirical conclusions out of purely theoretical models.

text that defines problems, background knowledge and the actual aims of science. However this does not lead to relativism but makes a discussion about the means for arriving at a given end possible in the first place.

In the concluding chapter 5 I give first an application of my methodological position and then reflect on the role that is left for philosophers in methodological discussions.

Section 5.1 discusses four cases of economic models and shows how a problem-focused discussion can point to problems and help to refine models. Case 1 discusses the problem of inadequate assumptions leading to »astonishing« results of a simulation without the authors' noticing the source of error. The second case sheds light on the rather common phenomenon of over-interpretation, i. e. the fact that economists tend to make claims about real, causal factors when all they have delivered is a rational reconstruction by means of abduction. Case 3 illustrates a milder form of problems with interpretation, where careful authors hesitate to draw strong conclusions and so do not draw any conclusions at all, even if they promised to contribute to the solution of an empirical problem. This is due to the fact that many economists are unaware of the methodological status of their formal analyses. The fourth case finally shows a model that fails to solve its problem because it is neither descriptively adequate nor able to deliver predictions and therefore cannot be said to explain anything. The authors, however, seem unaware of all of this. These case studies show what fruitful criticism of economic models looks like in the problem-oriented, pragmatic framework I endorse.

The second conclusive section finally discusses the question of whether there can be a special role for philosophers in economic methodology. It should be clear that there is no justification for making philosophers the final judges in theory evaluation. This is, however, not necessary: Even without being final judges, philosophers can play a substantive role in theory appraisal because of their institutional independence. In contrast to practicing economists, philosophers are free to discuss problems of interpretation or fundamental difficulties of research programmes, as their livelihood does not depend on the good impressions of their economic models. This does not mean that philosophers are forced to take a critical attitude against all scientific theorising. On the contrary, as I have shown throughout chapter 4, philosophical reasoning can help to understand the methodological status of economic theories and thereby lead to better defences than the ones practitioners tend to give. In addition to their institutional independence, philosophers of science are trained to carefully weigh arguments about methodological questions, and this can enhance their ability to stimulate a methodological discussion; in contrast, economists are mainly trained to build models within a given theoretical school.

In the last section I refute some counterarguments that may be raised against my pragmatic framework for theory evaluation.

Finally, here is a list of the main theses of my work, providing the whole structure of this book at a glance:

1. Traditional empiristic methodologies are ill-suited for giving prescriptions to economists as well as assessing the quality of economic theories.
2. Postmodern relativism is irrefutable, but irrelevant for theory evaluation.
3. A pragmatic approach to theory evaluation that is radically problem oriented can legitimately give local guidelines for theory evaluation by way of a means-ends analysis. It is not possible to achieve more than instrumental rationality in theory evaluation, as there is no objective point of view. This does not lead to an atheoretical instrumentalism, as it respects the crucial role of theory for long-run progress and explanation.
4. Philosophers can play a special role in theory evaluation because of their institutional independence, which gives them time to study history and methods and enables them to critically discuss matters that practitioners must defend rather dogmatically if they want to be part of their scientific community.

2. EMPIRICISM AND FALSIFICATIONISM

In this section I will first give a short philosophical overview of the development of logical positivism and falsificationism and then switch quickly to their application by several methodologists who have used these concepts for criticising economics. Logical positivism or verificationism suggests itself as a natural starting point, as it was the groundbreaking fundament of modern philosophy of science. Falsificationism can also best be understood against this backdrop, since Popper explicitly designed it to overcome the epistemic problems attached to logical positivism. As history has shown, Popper succeeded in this: falsificationism (and not verificationism) is now the benchmark from which one is expected to distinguish one's own position. That is not to say that falsificationism is without problems or generally accepted, but it is still an important position in economic methodology. As the concept is widely known, a brief introduction to the main philosophical ideas will suffice.

The specific applications to economics are of greater interest here and will thus be outlined in more detail: I will summarise the positions of four critics that have stressed the need for economics to become more empirical, starting with the classic work of Terence W. Hutchison and ending with a recent position by Scott Moss. The empiricist position in economic methodology is still the most influential one, even if its classic form is today mostly considered obsolete.

The most important and therefore longest subsection discusses problems of the empiricist criticism outlined at the beginning of this chapter. This analysis uses well-known concepts from philosophy of science and shows why criticising standard empiricist claims is the first step towards a new methodology. At the end of this section two case studies illustrate the shortcomings of the empiricist methodologies in economics with some palpable examples. The first case study is a longitudinal overview of modern economic-growth theory, and the second concentrates on the current quarrel between rational-choice and experimental economists. Both case studies show the relation between theoretical and empirical work in economics and thereby give hints as to why some of the demands of the empirical methodologists are ill-argued.

2.1. From Logical Positivism to Falsificationism

It is widely accepted that modern philosophy of science started in the 1920s with logical positivism. The so-called Vienna Circle, including members such as Carl Gustav Hempel, Moritz Schlick, Otto Neurath and Rudolf Carnap, was the driving force behind the programme of logical positivism. In the crazy times between the two world wars this programme can be seen as an attempt to establish (or defend) conceptual clarity and academic standards against some of the fashionable nonsense of that time.

Inspired by the early Wittgenstein and the work of Bertrand Russell, it was a basic assumption of the Vienna Circle that synthetic judgements a priori are impossible, which means that we cannot know facts about the world by merely thinking about it, without digging into any empirical details. As a result, the main negative task of the Vienna Circle was to criticize metaphysics as meaningless and finally abolish it from philosophy and science. What remained for philosophy was the logical analysis of language and the somewhat utopian aim of building a unified science.

Obviously, if metaphysics was to be criticized as meaningless, a criterion of meaning was needed.[1] Logical positivism chose verifiability as the necessary criterion for a statement to be meaningful. In his seminal essay »Überwindung der Metaphysik durch logische Analyse der Sprache«, Rudolf Carnap illustrates with much clarity how the criterion of verifiability may be applied in order to exterminate meaningless metaphysics.[2] He classifies meaningless statements into two classes:

1. Meaningless by semantics and
2. Meaningless by syntax.

A statement is meaningless by semantics if it contains predicates that are in no way reducible to basic empirical statements – the so-called »protocol statements«.[3] Carnap gives the example of the fictional predicate »babig« which is supposed to have no empirical content at all. Thus, we have no way to detect if something is »babig« or not. For Carnap it follows that such a predicate is meaningless and hence should be exterminated. Carnap claims that there are many predicates in metaphysics which are meaningless[4] and that there is no way out of this, since metaphysics tries to accomplish exactly

[1] Note that strict meaninglessness is different from »nonsense«, i. e. statements that are simply wrong (all fish can fly) or practically irrelevant (the average height of persons whose name ends with the letter »h« is 1.78 m).

[2] There is an ongoing discussion about whether Carnap was indeed a verificationist. See e. g. Creath (1982), Richardson (1998) or Mormann (2000). The question may be interesting, but it is of no importance for my work, as Carnap's position can be interpreted as a prototype of a verificationist and is a traditional starting point for any form of verificationism.

[3] See Carnap (1931), p. 222. Those protocol statements were later often referred to as »sense data«.

[4] It does not need to be the case that those predicates are newly invented words. Usual words can lose their original empirical meaning without gaining a new one. See Carnap (1931), p. 224-225.

what is, in his opinion, impossible: to ascertain non-empirical facts about the world.

The case of a statement's being meaninglessness by syntax is more complex. A statement can be meaningless even if it contains only meaningful terms, i. e. empirical terms in the sense explained above. This is the case if the words are combined in a way that makes no sense. There are two cases distinguished by Carnap:

1. Errors in natural language syntax (»Cesar is and«)
2. Errors in logical syntax (»Cesar is a prime number«).

To sum up somewhat simplifying, a meaningful sentence must contain only meaningful (i. e. verifiable) elements that are combined in the correct way.

There is no need to dig further into the details of logical positivism here. In the end, the failure of the logical positivist project became clear even to its adherents: The harsh distinction between meaningful science on the one hand and meaningless metaphysics on the other was impossible to draw when it became clear how much of scientific thinking relied on »metaphysical« concepts that are not directly verifiable. It is today the standard view that we may learn the most from the Vienna Circle's programme by understanding in detail why its approach failed.

Karl Popper was an early critic of the logical-positivist programme, and he tackled with a slightly different central question: He did not want to demarcate meaningful sentences from meaningless ones but tried to distinguish science from pseudo-science and metaphysics.[5] Popper criticized the Vienna Circle's criterion of verifiability and the idea of constructing knowledge from the bottom up out of basic empirical statements, mainly because of the problem of induction: Out of a finite number of observations it is impossible to *logically* derive the kind of general laws which are typical for science. Popper presented a radical criticism of the various attempts made to justify an inductive way of gaining general knowledge – and this criticism has not been refuted until today.

According to Popper, all attempts to justify induction end up in an infinite regress, because any »induction principle« must justify itself via induction again. Put more concretely: Let's assume the induction principle states that after 1,000 successful tests of a scientific law we can accept the law as true. Such an induction principle cannot be true a priori, as that would be a *synthetic* judgement a priori – and apriorism is ruled out by Popper, just as it was by the Vienna Circle.[6] If it is true *a posteriori*, this is the same as saying it is

[5] In this section I present only a small fraction of Popper's early ideas. As with the section on Carnap, this part does not aim at a truthful reproduction of a philosopher's work but is rather used for introducing important concepts.

[6] See Popper (1934), p. XXIV, p. 5 et al.

true by induction. Therefore, we need another induction principle to justify the first one – and so on, and so on. This proves that there cannot be an induction principle that does its job, for it can neither be true a priori nor justified empirically, as this leads to an infinite regress.[7]

The same kind of reasoning can be applied even when we try to assign only probabilities to the validity of a scientific law: If an assumed induction principle would state that a law is »true with the probability of 0.9« after 1,000 positive observations, there is still the need for another induction-principle to defend this claim. Popper therefore rejected induction as a method of justifying the validity of scientific laws.

In fact, he also rejected the idea that one could verify (in the sense of an unalienable truth) *anything* by empirical observation. For Popper the fallibility of all knowledge is a basic assumption; thus we cannot »verify« anything.[8] Popper's famous »solution« of the induction problem is more a kind of circumvention: There is no way to gain true knowledge out of empirical observation, but true knowledge is not what science is all about! The scientific project is much better characterised as a process of creating bold hypotheses and then trying to refute them. The origin of these hypotheses is quite irrelevant and leaves room for induction or any other metaphysical way of gaining inspiration for their discovery. But such methods can never provide a *justification* for scientific theories. Popper's analysis of the problem of induction is what turned the tables upside down, by establishing falsifiability instead of verifiability as necessary condition for a hypothesis to be »scientific«. The sufficient condition for a hypothesis to count as scientific law claims that the hypothesis must have survived many tests in different conditions – i. e. it could not be falsified even if every attempt was made to do just that.[9] Popper is fully aware that he is making a decision by defining science in this way; to him, the methodological rules are conventions of the scientific game.[10] The rules Popper sets out are tailored to maximise the falsifiability of theories, and therefore he forbids auxiliary assumptions that are made to save a theory from falsification; auxiliary assumptions must always increase the falsifiability of a theory.[11]

Upshot:

Popper's approach is extremely relevant for economic methodology. It is likely that no economist had ever characterised his profession as construct-

7 See Popper (1934), p. 200.

8 An introduction of the given length may be an insult to Popperians, but as *economic* methodology is in focus many interesting problems will be dealt with in the following subsections about falsificationist economic methodology.

9 The logical subtleties of verification and falsification are of minor importance here. Hempel (1965) gives an excellent overview.

10 See Popper (1959), p. 32.

11 See Popper (1959), p. 61 et sqq.

ing a system of knowledge that is based on sense data, as logical positivism suggested. Therefore, the Popperian reconstruction of science as an ongoing test of bold hypotheses was easier to accept for most economists than was logical positivism. However, as stated earlier, economists never have not taken falsificationism seriously in their daily work, and changing this seems to have been the main impetus for many to become convinced empiricists in economic methodology. The next subsections present a reconstruction of important positions that argue for more empirical work in economics and which were clearly inspired by both logical positivism and Popper's works.[12]

2.2. Criticising Economics for its Lack of Empirical Content

It is a common criticism that neoclassical economics should take empirical findings more seriously. While early critics such as the economist Terence W. Hutichson concentrated on demanding an increase in falsifiability and falsification of economic theories, the need to directly integrate empirical findings into the foundations of economic theories has been increasingly stressed by succeeding economic methodologists. The methodological positions of Hutchison, Hans Albert, Mark Blaug and Scott Moss are used as illustrative examples for empiricist methodologies.

2.2.1. Economics Must take Falsification More Seriously

Shortly after Popper had written the »Logic of Scientific Discovery«, Terence W. Hutchison claimed the basic postulates of economic theory to be unfalsifiable and thus tautological and practically irrelevant. His work »Significance and Basic Postulates of Economic Theory« was an early plea for more empirical work in economics. In his later works he didn't fundamentally change that position, but still argued against »empty formalism«.

The main impetus of Hutchison's early work is to criticise apriorism and the connected believe that it is possible to gain insight into real world (economic) relations by means of pure theory, i. e. analytical models and deduction.[13] As those relations of pure theory are not falsifiable, they do not for-

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[12] Henceforth I will sometimes *not* distinguish explicitly between empiricist and falsificationist views, as falsificationism *is* an empiricist position.

[13] As pseudo-sciences where flourishing in the first half of the 20th century, it was an important task for Hutchison to distinguish the »real« sciences from pseudo-science. Even if that demarcation is far from being finally settled (and most probably never will) it does not seem to be so pressing nowadays.

bid »any conceivable occurrence«[14] and are therefore devoid of empirical content.

Hutchison consequently attacks the view that holds the social sciences to be different *in principle* from the natural sciences, because they are concerned with »understanding the essence« of things. Similar to Popper, but without referring to him directly, Hutchison proposed falsifiability or simply testability as a criterion of demarcation between science and non-science.[15] In his early work there is much more optimism concerning the feasibility of clearly demarcating metaphysics from empirical sciences than in his newer works – nevertheless, Hutchison generally stuck to his empiricist position, and was even called »ultra-empiricist« by Fritz Machlup. [16] At the same time, Hutchison pragmatically rejects Quine's famous criticism of the analytic-synthetic distinction[17], saying that its acceptance would lead to less clarity and a weakening of academic standards.[18]

Hutchison believes the primary task of economics to be the providing of policy-relevant predictions, which in his opinion the empty formulas of pure theory cannot deliver. When arguing in favour of prediction, Hutchison is always aware of the special difficulties in predicting human behaviour.[19] Therefore he only asks for »predictions which, on the average, are slightly but significantly less inaccurate and unreliable than would be forthcoming without their input of systematic, more or less disciplined economic knowledge.«[20] This is reminiscent of von Hayek's »pattern prediction«[21], but Hutchison is not very clear in his assessment of von Hayek, which he mentions only very rarely. Concerning the means for obtaining better prediction, Hutchison clearly prefers econometric and statistical tools to standard economic theory.[22]

If economists accept prediction as one of their aims, they usually restrict themselves to conditional predictions by incorporating ceteris paribus clauses into their theories. Hutchison argues against this, because ceteris paribus clauses always lessen the falsifiability of a theory and hence its empirical content. He takes the law of consumer demand as an example, which he formulates like this: »If the price at which a good is sold rises, ceteris paribus the amount of the demanded good declines.«[23] According to Hutchison, the cet-

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14 Hutchison (1938), p. 161.
15 See Hutchison (1938), p. 9.
16 See Machlup (1955), p. 8 (Reference from Hart (2003), p. 357).
17 See section 2.3.1.4 and 3.1.1.1.
18 See Hutchison (1992), p. 63. Lacking clarity is the main critique he levels at the »antipositivists« like Deidre McCloskey.
19 See e.g. Hutchison (1977), p. 8–12.
20 Hutchison (1992), p. 81.
21 See Hayek (1974).
22 See Hutchison (1992), p. 86.
23 See Hutchison (1938), p. 41.

eris paribus clause can be interpreted in at least two ways: Either it is a vague generalisation and encourages ad hoc immunising or it makes the statement purely analytical. In the latter case, one *determines* whether the ceteris paribus clause is true by checking if the proposed law was confirmed. In contrast to the natural sciences, economics requires one to make ceteris paribus *assumptions* that one knows to be untrue. In the natural sciences one can perform experiments and control the boundary conditions, so the ceteris paribus clause can be legitimately said to have empirical content. In economics, a ceteris paribus clause means at best that the proposition one has made is true »in many cases«[24]. Hutchison acknowledges the need for ceteris paribus clauses, but demands more precision when formulating them: Instead of simply asserting that a condition holds ceteris paribus, economists should define exactly the interactions they are expecting from disturbing factors.

Apart from his fundamental methodological criticisms, Hutchison provides a more sociological critique of economics as well. In his book *Knowledge and Ignorance in Economics* he gives a detailed account of the devaluation debate in Great Britain.[25] Hutchison comes to the conclusion that economists tend to »overplay their hands«[26] and are wildly overoptimistic when assuming consensus among their colleagues.

Hutchison remains convinced that empirical testability remains the hallmark of good science and fiercely attacks any attempts to undermine this standard.[27] He does not believe in the self-regulation of the »marketplace of ideas« and maintains that markets can go wrong.[28] Thus, he thinks it the task of methodologists to criticise and finally correct the going astray of economics.

Hans Albert's work can be seen as an elaboration of Hutchison's main ideas. Albert established the term »model-platonism« in the early 1960s.[29] This refers to an alleged tendency in economics to construct tautologic models and by this to evade empirical checks of economic theory. In order to draw a precise and clear picture of Albert's criticism, I shall briefly sum up two of his examples.

Hans Albert analyses the empirical content of the law of falling demand[30], which claims that the demanded amount of a good can be described – cet-

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24 See Hutchison (1938), p. 44.

25 See Hutchison (1977), p. 98 et sqq.

26 See Hutchison (1992), p. 43.

27 As far as other economic methodologists are concerned, Hutchison believes that there is a trend towards »extreme permissiveness« that uses the term »positivism« merely as a dustbin for all different sorts of methodological sins. For a passionate pamphlet, see Hutchison (1992), p. 49 et sqq.

28 See Hutchison (1992), p. 92.

29 See Albert (1963), p. 331 et sqq. In Albert (1998), p. 182 et sqq. there is an updated version of this discussion.

30 In the following I will use »law of demand« as a synonym.

eris paribus – as a monotonously falling function of its price. The catch in this formulation is the ceteris paribus clause, as it can be used to immunize the law completely from empirical testing. According to Albert, the ceteris paribus clause is not a mere precautionary measure but a central part of the law of demand. The following list summarises three cases that Albert uses to suggest the problems that may arise from the employment of ceteris paribus clauses in the law of demand, sorted by an increasing amount of factors to be kept constant.

1. *Constant preferences*: Hans Albert rejects the assumption of constant preferences, because this would allow for »explaining« or rather excusing every divergence from the law of demand by a spontaneous change in customers' preferences (i. e. a deviation from the assumption of constant preferences). When analysing open systems such as economic ones it is hard – if not impossible – to control for the stability of the elements under examination. According to Albert, this leads a fortiori to the empirical emptiness of the laws in question, which makes them practically inapplicable.[31]
2. *Weak ceteris paribus clause:* There are several ways for understanding a ceteris paribus clause. The general remark »the rest being equal« does not precisely specify *which* factors are to be kept constant. This leaves room for the ad hoc invention of any kind of disturbing factors which can made responsible for failing to confirm the law of demand in a specific experimental setting.
3. *Strong ceteris paribus clause:* In this context, »strong« means that everything except the price of the examined good remains constant.[32] If the ceteris paribus clause is interpreted in this way, the falling demand is no longer an empirical question but rather an analytical truth: If the consumption of all of the other goods, the saving rate and the money supply all remain constant, the consumption of the examined good is forced to fall, because there is simply no money left to maintain it at its former level. In this case, the law of demand becomes a logical truth and loses the information it is supposed to carry.

Albert claims that all three interpretations of the ceteris paribus clause lead to the immunisation of economic models against empirical counter-evidence.

The second of Albert's examples I will present refers to the economic theory of growth. According to Albert, there is a misunderstanding about the status of the central equations in economic growth theory: Growth models which claim to explain the conditions of balanced growth actually *define* the

[31] See Albert (1959), p. 379.
[32] In Albert (1965) there is a detailed discussion concerning differently strong ceteris paribus-clauses.

very conditions of what is to be understood as balanced growth.[33] Because of this, the *conditions* of growth are identical to the *concept* of growth and therefore can tell us nothing about how to bring it about. Albert calls theories like this model »platonic«, as they do not put forward hypotheses that have empirical content.

Albert underlines his reproach of model-platonism using the given examples and, of course, some others. In his view, economics is often concerned with purely formal deductions based on assumptions that are chosen at will and defended by vague plausibility considerations, instead of creating empirically meaningful theories that can be tested. Empty rational-choice economics runs the risk of building up a system that resembles an »astronomy of the movement of goods«[34] and ignores the actually interesting features of the economy – which is why Albert calls it a »logic of planning«, [35] stressing the normative component of rational choice.

Albert admits, however, that while statements in economics are not always completely empty, often falsification is made impossible due to practical restrictions. The case of the weak ceteris paribus clause in the law of demand (see above) may be an example of this.

For Albert, the genesis of a model does not play a role in its appraisal. Methodological keywords such as »simplifying to the essential« belong to the context of discovery and cannot be used for justifying models.[36] Albert's position is similar to Hutchison's plea against any way of trying to understand the economic world out of a priori reasoning. For him, the creation of empirically testable hypotheses is necessary but lacking in mainstream economics.

Mark Blaug is a more recent defender of a falsificationist position in economics. He is an outspoken Popperian, but as an economist he concentrates more on the practical relevance of methodology than the more subtle philosophical issues. Blaug defines himself as an »aggressive« methodologist, i. e. one who aims to change economics instead of defending it.[37] Along with

[33] See Albert (1963), p. 349. Here and in Albert (1957), p. 385 et sqq. Albert seems to refer to Harrod-Domar models. A more detailed discussion can be found in section 2.4.1.

[34] Albert (1963), p. 361. (Translation by S.D.).

[35] Albert (1961), p. 45. (Translation by S.D.).

[36] See Albert (1959), p. 375. Strangely though, one page later Albert argues in favour of »empirical fundaments« as a criterion for good models. For further discussion, see section 2.3.1.4. The distinction between context of discovery and context of justification has itself been under attack for some time; see Hoyningen-Huene (1987). While there are ambiguities, I will continue to use the concept for pragmatic reasons. A similar case can be made for attacks on the fact/value split. While there are no pure value-free facts of the matter, it is nonetheless helpful and feasible to separate facts from values, as the concepts can be grasped even without the existence of »pure« cases.

[37] See Blaug (1990), p. 3.

Hutchison and Albert, he harshly criticises purely formal models that have no reference to the real world at all. The main point of Blaug's work is to urge economists to try more seriously to formulate truly discriminating tests. Blaug is well aware of the difficulties in doing this, but sees these as obstacles to overcome rather than reasons to refrain from a falsificationist methodology. (In order to avoid repeating arguments too much, the following section on Blaug is shorter than the preceding ones on Popper, Hutchinson, and Albert.)

Mark Blaug argues consistently against the reluctance of economists to »produce theories that yield unambiguously refutable implications«[38] and their unwillingness to confront predictions with empirical data. In accordance with Terence Hutchison, Blaug is highly critical of the formalist revolution and claims that mathematical calculations serve mainly as an entry barrier for new scientists, because they are hard to learn and relatively easy to check for consistency.[39] Blaug employs many detailed case studies to show the doubtful connection between highly abstract theories and reality. He does not, however, accuse economic theories of being completely empirically empty. His point is rather that most models are much too simple and because of this cannot represent complicated facts. Instead of concentrating on better predictions, economists have engaged in a technical discourse of possibility theorems and mathematical proofs that do not promote progress (in the sense of a better understanding of real-world economic processes).

Blaug criticises (among others) growth theory, the theory of consumer demand, the marginal productivity theory of wages or the Heckscher-Ohlin theorem and, of course, general equilibrium theory for lacking empirical adequacy.[40] According to Blaug, all these and many other »advances« of economic theory have done little to improve the predictive power of economics but mostly solve problems economists created for themselves.

On the other hand, Blaug acknowledges that there is empirical research going on in economics, but he condemns it as »playing tennis with the net down« because it is aimed at confirming the truth of economic theory instead of challenging it.[41] Of course, economists do not officially admit such a methodology and claim to be testing their hypotheses, which is why Mark Blaug speaks of »innocuous falsificationism«. Blaug sees the only hope for better economics in taking falsification seriously and claims that new econometric methods are needed to achieve this aim.

Blaug's falsificationist position has been harshly attacked by many methodologists as unattainable in the social sciences for reasons such as the impossibility of pursuing controlled experiments with real economies or the

[38] Blaug (1980), p. 238.
[39] See Blaug (1998).
[40] See Blaug (1980), p. 239.
[41] See Blaug (1980), p. 241.

openness and interdependence of economic systems, which makes the isolated observation of causal relationships extremely difficult. Blaug answers these attacks with the request to »try harder« and aims to show by means of historical examples that falsification *is* possible and fruitful in economics, but happens too rarely and, more importantly, is not systematically pursued.[42]

2.2.2. Empirical Findings Must be Integrated Into the Foundations of Economics

In addition to their arguments for taking falsification more seriously, many methodologists claim that the theoretical fundament of economics must be enriched by empirical research about human behaviour and step away from short-term maximisation of income as the only goal of its agents.

Hans Albert comes up with two proposals for better economic research: The introduction of sociological research and the systematic consideration of institutional settings. He is convinced that there are no separate economic problems:

> There is […] no economic problem […] about which it is safe to say that it could be solved without recourse to social factors not yet considered.[43]

Albert argues that there is no »theoretical autonomy« of economics that can be attributed to its problems. In short: There is no such a thing as »pure economics«.[44] As economics is concerned with exchange-acts of real persons, Albert demands to integrate factors other than simple income-maximisation into economic modelling and proposes social norms and institutions as promising candidates.[45]

Translated into modern terms, Albert pleads for better foundations in economics by making it an empirical science of human behaviour[46] instead of a science of empirically empty rational-choice calculations.[47]

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42 See Blaug (1980), p. xv.
43 Albert (1963), p. 360. (Translation by S.D.).
44 See Albert (1960), p. 480.
45 See Albert (1963), p. 360. In Albert (1998) he still sticks to that position.
46 See Albert (1959), p. 380.
47 Albert has not changed his position fundamentally in his more recent publications, but there are some interesting changes concerning the points he stresses most. In some publications he acknowledges the role of economic theory as method of *explaining* social processes, and sometimes even seems to plead for an economisation of sociology rather than for a sociologisation of economics. See e. g. Albert (1978), p. 60. Albert clearly welcomes institutional economics, but he still feels the need for filling the »cognitive deficit« in economics. See Albert (1984), p. 59 et sqq. and Albert

One last fraction of methodologists deserves to be mentioned shortly in this section about empiricist methodologies: The advocates of the so-called KIDS (Keep It Descriptive, Stupid!) approach for social simulations, who oppose the KISS (Keep It Simple, Stupid!) community. Simulating social phenomena is a relatively new branch of economic research that raises several highly interesting methodological issues to which I will come back later. At this point I shall only briefly sketch out the position of Scott Moss as a researcher in the simulation field, who demands more empirical work when designing economic simulations.

Scott Moss has been arguing for better micro-foundations and against general equilibrium theorising in economics since the early 1980s.[48] By this he not only hopes to achieve better predictions but sticks to the idea that social science should offer theories for *explaining* social processes.[49] He is convinced that simulations should be based on a descriptively correct empirical basis in order to get useful results. By reconstructing some major scientific success stories, he tries to deliver evidence that good science is always based on empirical work, and comes to the following conclusion:

> One is unlikely to get to the thin pinnacle without the laborious work of also building up a broad base that is more directly related to observation and evidence – there is no 'magic' short cut (such as assuming people act collectively as if they were rational […] to obtain useful abstract social theory.[50]

In contrast to what the adherents of the KISS approach preach, simplicity is no end in itself for Moss. He puts forward several arguments against simplicity as a criterion for good science:[51]

1. Simplicity can be useful when the task to be performed is known in advance. E. g. if the task is building a machine that serves a specified aim, it is sensible to build it as simply as possible. When scientific discovery is at issue we face a completely different situation, because we have to explore the outcome and cannot simplify in advance.
2. Computing power has increased in such a tremendous way that there is no technical need for simplification in simulations anymore.
3. We are inclined to believe that the world is constituted on simple principles, but there is no a priori reason for the truth of this belief.

(1985), p. 60 et sqq. Interestingly, he does not mention experimental economics even in his latest publications on the topic. See Albert (2002).

48 See Moss (1981).

49 See Moss/Edmonds (2005), p. 2.

50 Moss/Edmonds (2005), p. 4.

51 See Moss/Edmonds (2005), p. 1 et sqq.

4. The fact that complex structures can emerge out of simple, basic principles is not a sufficient proof that the found principles are causally responsible for the complexities of the world.

For Moss, simplification is only acceptable *afterwards*, i. e. when the relevant factors have been determined. His position includes the demand for »theory-free« agents: According to Moss, modellers should not in any way assume behaviour for agents that is not based on *prior empirical observation* or does not correspond to what experts or participants of the respective field would accept as an appropriate description of their behaviour.

2.2.3. Summary

The foregoing section presented the common criticism that neoclassical economics needs to take empirical findings more seriously. Where Hutichson and Blaug concentrate on the demand for increasing falsifiability and falsification of economic theories, the claim for directly integrating empirical findings into the foundations of economic theories has been most notably stressed by Albert and Moss.

2.3. Problems of Empiricism and Falsificationism

Now that the empiricist criticism has been laid out in some detail, it is time to check its validity. In the following sections I discuss arguments that can be used for defending neoclassical economics against the empiricist charges. The aim of this chapter is two-fold: In the subsections under 2.3.1, I present philosophical positions that all deal with difficulties of defining »unproblematic background knowledge« or a solid ground for research. The arguments presented here can be used for explaining and justifying the fact, that neoclassical theory is still alive – despite the empiricist methodologists' criticism presented above. The subsections under 2.3.2 critically discuss the appropriateness of empiricist criteria for theory appraisal.

2.3.1. Problems with the Definition of »Unproblematic Background Knowledge«

All the positions considered here outline the difficulties of empirically testing economic theories or basing them on empirical data. A common point of these positions is what is often called the »theory ladenness« of observation, which expresses the view that there is no such thing as pure observation – that all observation is necessarily affected by some theory. Stated otherwise, this means that that testing and observing can only be meaningful when some beliefs are fixed as »unproblematic background knowledge« and thus are excluded from testing. While even the empiricist methodologists are probably aware of theory-ladenness, their normative prescriptions seem to largely ignore it (which may be the reason that some of the empiricists' attacks seem to be beside the point). Hence, the arguments presented in this section can be used to justify the fact that many economists have ignored the empiricist's criticism presented above.

The following positions all deal with theory-ladenness in some way and are ordered by increasing radicalness: A rather introductory note is Popper's quite vague acceptance of protecting the rationality principle against falsification (section 2.3.1.1). I then continue with a presentation of the views of Thomas S. Kuhn, who further developed the concept of theory-ladenness to show how it can lead to a complete separation of paradigms, which may explain why the empiricist claims were constantly rejected (section 2.3.1.2). In an exception to the ordering of this section, Imre Lakatos' methodology did not further radicalise Kuhn's views but rather tried to give a more rational justification for the rigidities that Kuhn observed. Lakatos' views were openly welcomed by economists and have helped to justify their rejection of empiricist demands (section 2.3.1.3). Finally, I present a synopsis of holism and coherentism as the concepts which most radically integrate theory-ladenness at their theoretical basis and by this point to theoretical weaknesses in the empiricist claims presented above.

2.3.1.1. Popper on the Rationality Principle

It may be surprising that Karl Popper is the first protagonist to be mentioned in this section, for he is usually seen as the inventor and defender of falsificationism. However, his ideas concerning the rationality principle demonstrate some of the central problems of naïve falsificationism and hint to possible answers that are discussed in detail in the following sections.

As the founder of critical rationalism, Karl Popper had a much more relaxed view on the rational-choice approach than his student Hans Albert has. In his essay »The Rationality Principle«[52], Popper argues that the assumption

[52] See Popper (1967).

that people decide rationally can be useful even if it is unfalsifiable or already falsified.[53] He asserts that the social sciences are not so much concerned with precisely predicting single behaviour but with the explanations »in principle« of typical situations. This is why we do not need to know what is exactly going on »in the psyche« of all participating individuals; it is enough to assume that people decide appropriately, given their respective aims and knowledge. Popper acknowledges that such a formulation of the rationality principle is »nearly empty«.[54] Nonetheless, he calls it useful as a methodological principle, because it encourages focusing on the analysis of modelling of situations and allows for ignoring the inconvenient details of human behaviour.[55] This is the reason why models can never be accurate representations; according to Popper, they are necessarily oversimplifications in some sense.[56] This is a counter-position to empiricist methodologists, who demand integrating behavioural observations directly into the behavioural foundations of economics in order to make it more realistic (as e. g. Hans Albert claimed). Popper, in contrast to Hans Albert, recommends changing the specification of the model (the *situation*), and not the underlying rationality assumption, if predictions contradict empirical data. He argues this to be the more fruitful approach, since models are more interesting, more informative and easier to test than the rationality principle.[57] Popper's view is best interpreted as seeing the rationality principle as a »metaphysical« assumption for the social sciences just as the causality assumption is one for the natural sciences.[58] By this interpretation, one can avoid the alleged inconsistency to his falsificationist methodology that requires falsifiability for theoretical laws, because the rationality principle is now seen not as a theoretical law but rather a heuristic for observing and explaining the social world.

[53] Popper declares the rationality principle to be false in a strict sense, because humans are irrational in some cases. In general, however, he considers it to be a good approximation for human behaviour.

[54] There is quite an extensive discussion about the meaning of the rationality principle being »nearly empty«. See for example Nadeau (1993), Lagueux (1993), Salazar (2000) and Kirchgaessner (2004). I tend to read Popper as saying the rationality principle is untestable, but its applicability shows that human behaviour is at least not completely chaotic.

[55] For a similar standpoint see e. g. Suchanek (1993). Referring to Popper, Latsis (1972) coined the term »situational determinism«.

[56] See Popper (1967), p. 361.

[57] Some authors see this as a contradiction to falsificationism, which they presume to demand changes of the *theory* in the case of empirical contradictions. (See Köllmann (2001), p. 8-9.) Popper's treatment of the rationality principle shows that he does not demand instantaneous falsification in the light of counterevidence, as this will immediately kill many fruitful approaches.

[58] For the role of »metaphysical assumptions» in Popper's falsificationism see Popper (1934), p. 13. Note that Popper does not consider metaphysics to be nonsense but rather sees it as valuable in guiding research.

Upshot:
Popper accepts the purposeful ignorance of empirical facts in order to protect the rationality assumption as a basis for providing scientific explanations. Because he does not offer much argumentative support for his view, however, it remains nothing more than a value judgement. Thus, the advocates of empiricism could still argue that the acceptance of the rationality principle is a sin rather than a virtue.

2.3.1.2. Arguments from Kuhn's Methodology
Thomas Kuhn is probably the most important critic of empiricist methodologies and therefore deserves a detailed introduction. His ideas deliver the key for understanding why empiricist demands for changing mainstream economics theory have had little success. This does not mean that Kuhn offers rational reasons for this – instead, he stresses the irrational aspects of science. Offering a more descriptive analysis, Kuhn states several difficulties in Popper's falsificationist programme. The most important features of Kuhn's ideas will be outlined in the following section.[59]

The term »paradigm« is surely the most important notion in the work of Thomas Kuhn.[60] Nonetheless, it is not easy to define. Margaret Masterman even distinguishes twenty-one different meanings of »paradigm«,[61] clustered into three categories:[62]

1. Paradigms as metaphysical worldviews: Ideas that fundamentally affect the way we perceive the world and think about it.
2. Sociological paradigms: Accepted scientific results, institutions and methodological decisions.
3. Constructed paradigms: Textbooks, classical works, measuring instruments etc. that influence scholars and thus determine the way a specific science is practised.

From this it becomes clear that paradigms are different from theories, because they are a kind of background conviction or a set of methodological rules *for* theories.[63] Speaking in Kuhnian terms, one might say that all science is »paradigm-laden«. Paradigms cannot be defined in a precise and formal

[59] As much of this section is a summary of Kuhn's *The Structure of Scientific Revolutions,* I will refer to exact pages only sparingly. For the sake of readability I will also skip the usual qualifications and present Kuhn's position as if it were mine.
[60] The Kuhnian term »paradigm shift« has today become a frequently abused buzzword.
[61] See Masterman (1965), p. 61 et sqq.
[62] In his postscript to the new edition of *The Structure of Scientific Revolutions*, Kuhn proposed substituting »paradigm« with the term »disciplinary matrix«: See Kuhn (1962), p. 194 et sqq. I will stick to »paradigm«, as this is the much more common term.
[63] The rationality principle is a good example, because it is the underlying principle of many methodological sets of rules and the guideline for the majority of economic

way, and as a result there are difficulties with comparing them on a purely rational basis. This is because the arguments in favour of one paradigm must always refer to the paradigm itself, as there is no neutral »meta-paradigm«.[64] This problem is labelled »the incommensurability of paradigms«.[65] Partly because of this property, paradigms are, even in mature sciences, not easy to describe. They resemble religious beliefs that enforce themselves by being applied in daily routines.[66]

Despite being difficult to define, paradigms are essential for science: Without paradigms, science could not exist, as without any rules there can be no games. But how is a paradigm established? This happens through a kind of evolutionary process: In the pre-paradigmatic phase of a science, random facts are collected without applying any theory (as far as that is possible) to them. Therefore all facts have a priori the same relevance. Different pre-paradigmatic schools interpret the facts differently and enter into competition.[67] If one school seems more successful than another in explaining nature consistently, it will attract more members, and the other pre-paradigmatic schools will lose influence. A paradigm gradually emerges from the remaining school. At this point normal science can start developing precise and »esoteric« theories. A paradigm is especially attractive for scientists when it is sufficiently unprecedented and open-ended, so that a lot of research can be done. To sum up, a paradigm is a set of beliefs that determines the normal way of doing science: What counts as scientific in a specific branch is a result of past and currently established ways of doing science. [68]

Only when a paradigm is established can »normal science« begin. Normal science is science under a paradigm, meaning that the basic assumptions of the paradigm are no longer questioned.[69] In normal science it is fairly clear what counts as a problem and what a correct solution would look like. This makes science as a professional business possible: Very subtle and specific problems can be addressed in specialized journals, in which agreement to key ideas is assumed. Those who are sceptical are simply excluded from the scien-

theories. Note that some theories, such as Newton's mechanics, became exemplars of paradigms.

64 See Kuhn (1965), p. 258 et sqq.

65 See von Dietze (2001), p. 47 et sqq. for an inspiring discussion of this problem.

66 See Kuhn (1962), p. 58 et sqq. A good description of this feature is »tacit knowledge«.

67 In this phase of research, the process seems well described by the Popperian term »critical guessing«.

68 See Kuhn (1962), p. 25 et sqq. for the whole subchapter.

69 This is probably why philosophy never really reached scientific status: There is no common paradigm. When a paradigm does emerge in a specific philosophical field, this field is often no longer regarded as philosophy but has become a science. Thus, philosophy is doomed to stay in the pre-paradigmatic phase of competing schools.

tific community and are thus forced to turn to philosophy, do historical research or give up being scientists altogether.[70]

Normal science can be well characterized as a puzzle-solving activity: It is a game with rules, one in which the solution of a problem can always be roughly anticipated. To put it clearly: Science is, for Thomas Kuhn, not a critical quest for truth but rather a dogmatic operation serving the respective paradigm. There is a lot of mopping up to be done, in Kuhn's words. The puzzle-solving, mopping-up work of normal science can be categorized as follows:

1. The gathering of facts, their interpretation and the postulation of relations between them so that they fit into the paradigm. Through this the scope of the paradigm is constantly expanding, and the paradigm may be used in fields that are far removed from its original purpose.[71] As more fitting facts are observed, the better theories under the paradigm become empirically confirmed.
2. The confirmation of predictions of the paradigm. As a paradigm is different from a theory, it does not predict anything by itself; predictions are made by those theories that are compatible with the paradigm. Normal science runs well when many paradigm-compatible theories can be found which create valid predictions. If nature can somehow be forced into the paradigm, this delivers ongoing proof that the current paradigm is right and fruitful.
3. Further specification of the paradigm. This is mainly the work of methodologists who try to define what should count as good science and why. They remain inside the paradigm but make it more precise.

So if the results are mainly obvious, why do science at all? The challenge in science is always *how* to prove the paradigm; the *ways* found to do this can be very interesting and even surprising. In general, falsification is not the aim of science. Single hypotheses may well be discarded, but anomalies that suggest the rejection of the whole paradigm are mostly ignored, or the research that led to them is regarded as a failure. Obviously, normal science has several defects:[72]

- Normal science leads to a »one-track mind«. Only problems that fall into the leading paradigm are seen, while other – possibly fruitful – observations and theories are generally neglected.

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70 See Kuhn (1962), p. 33.
71 An example is Becker (1978) or Wible (1997), Wible (1998).
72 See Kuhn (1962), p. 38.

- There is much intolerance against new thoughts or theories which do not fit into the paradigm. Normal science does not search out novelty.[73]

But Thomas Kuhn sees advantages to normal science, too:[74]

- Detailed and precise research can only be possible through relying on some kind of standard that does not need to be questioned. Seemingly esoteric research into very specific problems is the actual strength of the scientific method. Simplification and a focused worldview are needed for fruitful science.[75]

- Only when doing normal science can the boundary of a paradigm be detected. Scientists have to know the outcome of an experiment in advance in order to detect if something has gone wrong. The paradigm is pushed to its ultimate limits. But at some point, anomalies become numerous and the paradigm is not able to keep its promise of success anymore. A crisis of science comes into being.

If paradigms are so rigid and normal science does not search for novelty, how can a paradigm ever change? It will not change, as long as normal science is successful. But because normal science is so detailed, at some point nature will not fit into the paradigm anymore. Sometimes small refinements of the paradigm can help, but in other cases they do not.[76] Anomalies are part of the daily work in normal science, and usually they are regarded as puzzles to solve. But eventually it becomes harder and harder to invent theories that match the empirical data under the leading paradigm. Science then enters into crisis: the paradigm now cannot keep its promise and loses adherents. And the game starts all over again: The field is now open for new ideas, genius and intuition.[77] At some point one of these new ideas will lead to a new

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73 In saying this, Kuhn states the exact opposite of Popper. See Popper (1965).

74 See Kuhn (1962), p. 38-39. It is possible to interpret Kuhn's methodology as an essentially economic one, because he carefully weighs the costs and benefits of standards in science. See section 7.1 and Deichsel (2007).

75 A classic example is a geographic map enabling orientation if and only if it delivers a *simplified* picture of reality. See Robinson (1962), p. 33.

76 As an example, planetary movement under the geocentric paradigm was first depicted using circles. When loops in planetary movement were observed, they were explained by epicycles without rejecting the geocentric paradigm. The maturity of the paradigm also determines how easily that paradigm can be adjusted.

77 The Kuhnian term for this is »extraordinary science«. Extraordinary science can also happen by chance without prior crisis. See Kuhn Kuhn (1962), p. 96.

paradigm. A revolution has taken place.[78] By causing this process, normal science is *generating* novelty without *searching* for it.

The shift from one paradigm to another can be a lengthy process. First of all, there is, of course, always resistance against changing the original paradigm. Second, it can be difficult to view the same data from a different perspective. Kuhn even argues that such a gestalt-shift does not happen with individual persons:[79] There must be a group that sees the world differently and attracts new members. Thus the old paradigm slowly dies out.[80] That does not mean it becomes useless: It has solved a lot of problems and created solutions applicable in everyday situations – and these will endure even if a new paradigm comes to reign.[81]

One important aspect of paradigm-shift is that it cannot be enforced or fully rationally explained. Anomalies are a necessary but not a sufficient reason for the rejection of a paradigm, because they always occur. A new paradigm usually has even more counterevidence than an old one. So what has to be compared is the fruitfulness of the paradigms in question, and this question cannot be answered by reason alone. Hence we can only properly speak of progress when discussing normal science, for between two paradigms there is no common truth-criterion. Kuhn is in fact critical of any objective truth-criterion. Nonetheless, paradigm choice is not purely irrational. Kuhn provides classic criteria like precision, simplicity, generality and fruitfulness as guidelines – but even by means of these criteria one cannot choose a paradigm on a purely rational basis.[82]

For Kuhn, one may conclude, falsification is a destructive process, so much so that it should only be applied when there is little hope that it can be avoided. But even then, »falsification« of a paradigm will only happen (in the form of a revolution) when a promising alternative is at hand. For Kuhn, it is the small steps of normal science that constitute progress rather than the quasi-falsifying gestalt-shift of paradigm change.[83]

Upshot:

The Kuhnian view explains why the basis-empiricist criticism outlined in section 2.2 was not successful in changing economics: The integration of empirical knowledge about human behaviour would mean a paradigm change, as it would lead to a completely different type of science that could not even be

[78] The classical example for a scientific revolution is the paradigm-shift from Newtonian to relativistic physics.

[79] See Kuhn (1962), p. 98.

[80] See Kuhn (1962), p. 162.

[81] See Kuhn (1962), p. 39. The shift from Newtonian to relativistic physics is an example again.

[82] See Kuhn (1962), p. 156 et sqq.

[83] There is still controversy about whether a Kuhnian can acknowledge progress between paradigms. There are passages in Kuhn's work that say so, but they seem inconsistent with the general thrust of his work. See Klee (1997), p. 149.

compared to neoclassical economics. The claims of the empiricist methodologists simply lay outside the dominating rational-choice paradigm and were hence ignored by the majority of scientists working under that paradigm. If one accepts Kuhn's stance on incommensurability, it is impossible to decide which paradigm is the better one.[84] For Kuhn and his followers, progress is only possible *within* a paradigm - and even there it cannot be defined in a straightforward manner.[85] Unfortunately, normative methodological discussions always deal with the question whether a ruling paradigm needs to change. Kuhn's approach can explain the persistence of neoclassical theory as well as why empiricist criticism of it is likely to be pointless, as long as one takes the problem of incommensurability seriously.[86] What Kuhn cannot offer is an evaluation of paradigms. Imre Lakatos is a philosopher who has further developed Kuhn's ideas and tries not only to explain persistence but to justify it as well. His methodology will be presented in the next section.

2.3.1.2. Arguments from Lakatos' Methodology

Lakatos's methodology can be interpreted as an integration of Kuhn's objections into Popper's falsificationism.[87] By now it should be quite clear why the suggestions of the empiricist methodologists were blocked when it came to changing economics. Lakatos provides additional arguments explaining why falsifying the core of economic theory by empirical observation may not be as easy as some empiricist methodologists tend to think. Here is a summary of his main points:

1. Direct observation is impossible. In order to test a theory we need instruments or statistical know-how. Scientific observations are thus dependent on other theories.[88]
2. Even if we could clearly distinguish between theory and observation, there would be no way to gain certain knowledge out of observation in compliance with the assumptions of falsificationism, where certain

84 According to Kuhn, one cannot test paradigms. This is particularly true of the rational-choice paradigm, as any conduct whatsoever can be reconstructed as rational. Due to this speciality the neoclassical paradigm cannot detect anomalies, and hence it is hard to see how the process of crisis, revolution and normal science could ever emerge in economics.

85 Kuhn suggests discussing progress criteria in a way similar to how we discuss ethical values. He asserts that rational discourse is still possible even if there are no clear-cut criteria. See Kuhn (1977), p. 111 et sqq.

86 Even without incommensurability, sociological reasons may prevent scientists from adopting external methodological advice. See the insightful study of Mackie (1998) that deals with sociological forces which led to a canonisation of economic theory.

87 See Backhouse (1997), p. 88 et sqq. and Lakatos (1970), p. 173.

88 See Lakatos (1970), p. 96-97.

knowledge is only possible within the realm of logic.[89] Hence, certain falsification is impossible.

3. Even if we had theory-free observation and certain knowledge from observation, it would not follow that a theory that runs into empirical difficulties is definitely falsified, because there is always room to alter an auxiliary hypothesis or postulate some disturbing interferences instead of questioning the theory.[90]

After stating the difficulties of falsification, Lakatos developed his own way to get out of them: Simplifying a bit, the Lakatosian term »scientific research programme« can be regarded as a watered-down version of the (quite radical) Kuhnian notion of a paradigm. Lakatos does not focus on incommensurability or the irrationality of paradigm choice but instead gives arguments for the rationality of ignoring empirical counter-evidence to theories. This is one reason why the Lakatosian approach is often labled the »rational reconstruction« of theory-dynamics. Lakatos accepts many of Kuhn's findings on the descriptive level, but in contrast to Kuhn, he tries to defend them normatively as rational. According to Lakatos, scientific research programmes consist in a »hard core« of basic convictions that are never changed and a set of assumptions that can be altered when empirical findings threaten the hard core, the so-called »protective belt«.[91] Even critics of Lakatos admit the fruitfulness of this distinction for understanding the structure of economics,[92] and Lakatosian-inspired analysis of economic research programmes has been a major trend in economic methodology.[93]

Following Lakatos, a research programme is called *progressive* if and only if it is able to generate hypotheses that are at least partly empirically confirmed[94] and can be used for the prediction of novel facts[95] without losing

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89 See section 2.1 and Lakatos (1970), p. 97-98.
90 See Lakatos (1970), p. 98-100.
91 See Lakatos (1970), p. 133..
92 See Hands (2001), p. 296
93 See Latsis (1972) for the first work of this tradition; see Wong (1978) for another well-known work. Hands (2001), p. 287 gives an impressive overview of this field.
94 Note that by this definition it is quite easy for a research programme to be »progressive,« because only partial confirmation is demanded – and *partial* confirmation can be found for nearly anything. See footnote 102 in this chapter. Lakatos himself notes problems with the so-called tacking-paradox (See Lakatos (1970), p. 128.): According to his criterion, research programmes would become progressive simply by tacking any partially confirmed statement to a theory of the research programme. In total, Lakatos' methodology is well suited for analysing the structure of research programmes, but it is now considered rather weak at appraising them.
95 Note that novel facts do not necessarily lie in the future; they only have to be novel in the sense that they are different from the input data used and different from what a theory was designed to predict. See Lakatos (1970), p. 114 footnote 99.

the well-established achievements of old theories[96]. Older theories are only said to be falsified if such hypotheses are found, which is why Lakatos's view is called »constructive falsificationism«: Falsification happens only when a better alternative is at hand.[97]

The textbook *Neue Institutionenökonomik* from Erlei/Leschke/Sauerland gives an elaborate definition of neoclassical (and institutional) economics in terms of a Lakatosian research programme. As it is a good and recent example of how Lakatos' methodology can be applied to economics, I will translate large sections of it here. The authors define the hard core of (mainstream) economics through four propositions:[98]

- HC1: The individual orients its action – while paying attention to relevant restrictions – on its preferences and is the only source of values (principle of individuality).

- HC2: The individual evaluates its options for action by means of a cost-benefit calculus and decides for its relative advantage.

- HC3: Social systems can be seen as decision units. However, all properties that are assumed for social systems (groups, societies, firms, households or other organisations) must be ultimately compatible to the properties and incentive structures of the individuals constituting the social system under consideration.

- HC4: Economics does not make assertions about the behaviour of single individuals. This is the domain of other sciences (e. g. psychology). Statements deduced from economic theory are always based on representative behaviour.

After having defined the hard core of economics as shown above, the authors characterise the protective belt by the following eight statements:[99]

- PB1: In every point in time t the preferences of individuals are given and constant.

[96] Note the contrast to Kuhn in this point – Lakatos requires full commensurability.

[97] Lakatos mentions, however, that it is not possible to decide ex ante if a research programme remains progressive in the future which shows again why Lakatos' framework delivers only weak criteria for appraising theories. See Lakatos (1970), p. 133.

[98] See Erlei/Leschke/Sauerland (1999), p. 51-52. (Translation by S.D.).

[99] See Erlei/Leschke/Sauerland (1999), p. 52 (Translation by S.D.). Only PB7 and PB8 are specific points that distinguish institutional economics from the mainstream.

- PB2: The individual usually has only limited knowledge about objective facts.
- PB3: Knowledge and skills of individuals can change over time.
- PB4: Scarcity is universal and therefore (different sorts of opportunity-) cost restrictions emerge.
- PB5: Market goods and collective goods as scarce goods create utility.
- PB6: Every voluntary exchange – considered in isolation – leads to an increase in net utility for the partners of exchange, respectively making none of them worse off.
- PB7: The equivalent of voluntary exchange on the market, regarding the choice of institutional arrangements, is the consensus principle as special variant of the Pareto principle.
- PB8: The functioning of the market mechanism and the quality of the provision of collective goods depends on the design of institutions.

This formulation of an economic research programme shows, for example, that the assumption of constant preferences is part of the protective belt of neoclassical economic research. If empiricist methodologists demand the integration of theories about preference-change into the hard core, this request is outside the original research programme.[100]

Following Lakatos, science is no longer about creating falsifiable hypotheses and rejecting them when they contradict empirical findings. Rather, the hard core of a research programme is deliberately fixed[101] with the aim of predicting novel facts in the sense explained above. Lakatos concentrates much more than Popper on the dynamics of science and is therefore much less demanding about features of single theories.[102]

[100] At this point, I argue of course on a purely descriptive basis. Normative reasons follow in later sections.

[101] Lakatos notes that the judgement of knowledge as problematic or unproblematic is always subjective. See Lakatos (1970), p. 23 et sqq. The reason is of course the problem of induction, which makes it impossible to gain secure knowledge about the future out of observation in the past.

[102] Feyerabend remarks that this deprives Lakatos of any normative bite. See Feyerabend (1965). Lakatos even allows for inconsistent theories: see Lakatos (1970), p. 176. This is one reason why Lakatos' methodology is not well-suited for theory appraisal. See footnote 94 in this chapter.

Upshot:

Lakatos's methodology does not show that the demand for more empirical work in economics is inherently wrong. But Lakatos' work delivers arguments explaining that a call for more falsification or empirical adequateness needs to be substantiated with good reasons why such a move would indeed lead to a progressive problem-shift.[103] From a Lakatosian perspective, the fruitfulness of research programmes (their »progressivity«) is what counts and not the empirical status of single components of a theory. So if neoclassical economics has domains in which it can be successfully applied and no superior alternative is available, it does not matter if the behavioural assumptions are descriptively accurate or if the theoretical core is unfalsifiable. For Lakatos, progress in science does not happen by following methodological prescriptions of strict falsification or empirical research but by inventing and pursuing fruitful research programmes. Empirical work in economics could become such a research programme, but as of now it has not constructively falsified neoclassical economics. This conclusion shows again that Lakatos' methodology is not well suited to decide strictly between competing research programmes – however (in addition to Kuhn's descriptive analysis) it gives reasons why critics of a research programme must do more than pointing to empirical shortcomings of single components.

2.3.1.4. Holism and Coherentism

As we have seen, Lakatos's methodology can give a good description of the structural composition of economics and by this deliver reasons for the rejection of some empirical counterevidence.

Digging a bit deeper into epistemological issues, the concept of holism shows additional, fundamental problems of empiricism.[104] Rather than discussing problems of applicability or counter-arguments against the application of empiricist claims (as in previous sections), this section will raise fundamental internal difficulties of empiricist positions. It is the last subsection dealing with the problem that for empiricist claims to make sense, some knowledge has to be fixed as unproblematic by convention.

Quine's paper »Two Dogmas of Empiricism« is famous for having shown quite convincingly what its title promises and therefore becoming

103 Taking Lakatos' criteria for progress literally (see above), it is highly probable that more empirical research would lead to a progressive problem-shift (if one manages not to contradict basic findings of the rational-choice approach), because the predictions need only be *partially* confirmed. This leads to the paradoxical result that two competing research programmes can be progressive problem-shifts to each other.

104 Note that Lakatos was aware of Quine's holism. The presentation of the two positions has been separated here for the sake of clarity.

a major threat to the classic empiricist programme. Here is the main line of argument:[105]

Empiricism is based on two dogmatic assumptions which are unjustifiable. The first dogma consists in the clear separability between analytic (uninformative of the world) statements and synthetic (empirically informative) ones. The second dogma, according to Quine, is reductionism, i. e. the belief that each meaningful statement must be reducible to a logical construction that consists only of terms which refer directly to experience.

Quine argues that the problems with the analytic-synthetic distinction rest on difficulties with clarifying the meaning of »analytic«. Without a precise understanding of synonymy there is no way of attributing »analytic« to anything else than tautological truths like »all unmarried men are unmarried«. The difficulties start when we use synonyms and change the sentence to »all bachelors are unmarried«. In order to clarify why this sentence is analytic, we need an understanding of synonymy that does *not* refer to analyticity, and Quine aims to show by means of several examples why this is impossible.[106]

The second dogma, reductionism, falls apart together with the analytic-synthetic dichotomy: If we cannot define »analytic« in a non-circular way, it does not make sense to separate meaning into factual and linguistic components. As a result, the idea of reducing the meaning of single sentences to empirical observations becomes absurd. Quine's fundamental critique led to holism, which completely rejects the analytic-synthetic distinction.

Pierre Duhem and Quine are among the earliest and best-known representatives of holism in philosophy of science. Their position is often roughly summed up as the »Duhem-Quine thesis«,[107] which states that it is impossible to test single components of a theory, because in every test it is always the whole theoretical body or even the totality of our beliefs that is at stake.[108]

A short formalization can clarify this point. The naïve falsificationist view can be stated as follows: Whenever a theory T falsely predicts an event E, this falsifies the theory. (Formally: $(T \rightarrow E; \sim E \vdash \sim T)$). The holistic view acknowledges that that there are many components of a theory that can be the reason for error, such as laws (L), auxiliary hypotheses (H), boundary conditions (C) or background convictions (B). If a prediction is wrong, the whole bunch of components is falsified: $((L\&H\&C\&B) \rightarrow E; \sim E \vdash \sim(L\&H\&C\&B))$. An exam-

105 See Quine (1951) for the original source and e. g. Hylton (2002) or Nimtz (2003) for stimulating recent discussions.

106 The exact course of argumentation is too long to be repeated here. Of course there are opposing views to Quine. See e. g. Carnap (1955), Grice/Strawson (1956) for an intensive discussion. For some newer reflections on the feasibility of the reductionist programme, see Beckermann (2001), chapter 12.

107 See Duhem (1906) for Duhem's classic article and Quine (1951) for Quine's part.

108 Duhem is refering only to hypotheses in physics, Quine to the whole framework of our knowledge. See Gillies (1993), p. 98-116 for a detailed comparison of the Duhem and Quine theses.

ple from economics is useful here to illustrate this point: Let's say a group of economists predicts the GDP to grow 3% and instead it falls 3%. The wrongful prediction gives no indication whatsoever regarding the source of error; usually, in this case, economists would defend themselves by claiming that the boundary conditions have changed from what they initially assumed.

Holism tells us that when a wrongful prediction is made, we cannot know if the laws (i.e. the theoretical core) are the source of the error or if we falsely assumed boundary conditions to be constant. Hence we are not forced to give up the whole theory but can tinker with different boundary conditions or auxiliary hypotheses. Quine goes as far as including a change of classical logic in the realm of possible alterations, because he is convicted that there is no knowledge a priori, not even knowledge about logic.[109] If we accept that empirical findings cannot determine theory choice, does this mean there is no guideline at all how to react to counter-evidence? Quine's own proposals are a bit vague, but supposedly this is the nature of such matters, because we cannot and should not expect precise methodological recommendations on a global level.[110] Quine proposes to change theories in such a way that[111]

- the totality of our knowledge is only minimally affected and
- if possible is simplified.

The general impact of Quine's position is the move from a foundationalist to a coherentist conception of knowledge, where theories are not seen as »representing« some data, but the focus is only on the fit between data and theories.

This means that any system of knowledge draws its plausibility only from the mutual support of its sentences and is not relying on anything external as a result of Quine's general rejection of the possibility of a sound empirical fundament.[112] From this perspective, the demand for more empirical work on the microfoundations that Hutchison, Albert and Moss are uttering is shortsighted, because there is no independent way of knowing whether the microfoundations are correct: The quality of the microfoundations cannot be judged separatly from the outcomes of the (macro-)theory; it is only the coherence of the whole theory that can be judged.

109 See Quine (1951), p. 21. See Quine/Ullian (1970) for a discussion of some more criteria such as generality and refutability.
110 See especially section 3 for an elaboration of this point.
111 See Quine (1951), p. 25.
112 This leads to a certain circularity in the justification of theories. Gerhard Vollmer often speaks of virtuous circles compared to vicious ones. From this point of view, the right question was not, if the structure of neoclassical theory is circular, but if the circle is a virtuous or a vicious one. See Vollmer (1992), p. 159.

Empiricist methodologists could defend their criticisms as propositions for a new economic theory that fits better into the totality of our knowledge. But if this were the case, they should not criticise neoclassical theory for lacking empirical validity but instead check if a more empirical economics would be more coherent.[113] When arguing for more empirical work, the critics seem to have had the outdated DN-model[114] of explanation in mind. This model rests on the assumption that true scientific statements can be created only by correct deduction from true observational statements. This conviction may lead to the demand for more empirical work on the basis of economics. However, with this assumption of a clear separability of starting conditions (observational statements) from outcomes, the DN-model is not capable of modelling the complex, holistic structure of scientific theories that makes such a separation often impossible.

Upshot:

The Duhem-Quine thesis and the resulting holistic conception suggest that the methodological discourse should not recommend increased empirical research on the fundamentals but rather encourage fitting the results of theories into our system of knowledge and increasing their contribution to solving problems.[115] From this perspective, there is no general justification for more empirical research at the basis of economics.

2.3.2. Problems with the Evaluation of Theories

In the preceding sections I presented different approaches that deal with theory ladenness as an obstacle to achieving better economic theory merely by intensifying empirical research or trying to falsify more thoroughly. In this section I shall set such considerations aside and will concentrate on other problems that arise in theory appraisal when empiricist criteria are applied.

2.3.2.1. Practical Problems with Empiricism and Falsification in Economics

Firstly and most notably, there are many practical problems with data acquisition and interpretation in the social sciences. To call them »practical problems« is not to say they are less severe. Quite the contrary: In many cases these problems are the very source of subsequent methodological debates.

[113] For an evaluation of a more empirical economics see section 2.4.2.2. Because the experimental results are quite heterogeneous it is prima facie quite improbable that more coherence can be achieved by means of experimental economics.

[114] »DN-model« is the abbreviation of »deductive-nomological model« and is alternatively called Hempel-Oppenheim Scheme. See Hempel/Oppenheim (1948).

[115] See Schröder (2003), p. 235-237 for a discussion employing examples from economics.

I will list the most important difficulties with gathering data in economics here:[116]

1. It is impossible to pursue experiments in macroeconomics, therefore all empirical work that addresses macro-questions has to rely on statistical analysis.
2. Economics is strongly connected with the behaviour of human beings and therefore its »laws« cannot be as robust as laws in the natural sciences can be. Since humans are involved, experiments can be controlled to a far lesser degree in the social sciences than they can in the natural sciences.
3. Economics deals with complex and highly interdependent systems, therefore the »quest for causality« is very hard to pursue, as the ceteris paribus conditions are in fact never met and single ceteris paribus laws do not help very much for understanding an economy. The complexity and interdependence of economic systems make precise unconditioned predictions impossible.

Upshot:
It is safe to say that general problems with pursuing experiments, problems of controlling experiments properly and the complexity of economic systems are major obstacles for empirical results to be as precise in economics as they can be in some natural sciences.[117] That said, we can turn to the more philosophical arguments against the call for more empirical work in economics.

2.3.2.2. Truth, Verisimilitude and Progress – Semantic Problems
When comparing scientific theories or models it seems at first natural to state that the truer theory is the better one. But what does »truer« actually mean, if we accept that we can never reach absolute truth? Karl Popper introduced the concept of verisimilitude in order to be able to compare the truthlikeness of theories:

Given that the truth-content and the falsity-content of two theories t_1 and t_2 are comparable, t_2 is said to be nearer to the truth if and only if either:

[116] For a good overview of obstacles to falsification (not only »practical« ones) in economics see Caldwell (1982), p. 238 et sqq.
[117] Of course other social sciences have similar problems. Remarkably, sociologists or psychologists already deal with »economic questions« in a way that resembles the demands of the empiricst methodologists. One might say the demands have actually already been met – just in sciences other than economics. This fact hints at the conclusion that empiricists‘ demands do not lead to a clearly superior way of dealing with economic questions.

(a) The truth-content but not the falsity-content of t_2 exceeds that of t_1, or
(b) The falsity-content of t_1, but not its truth-content, exceeds that of t_2.[118]

This method of computing verisimilitude was purposefully designed to show that a theory which has been already falsified can still be better than another false theory. In a way this provides an escape from Poppers radical anti-inductivist stance, as good experimental corroboration can now be seen as an indication of high verisimilitude.

This view has several problems, though. In the 1970s a number of papers criticised technical defects in Popper's definition of verisimilitude that emerge when two theories are compared that are known to be false.[119] Miller and Tychý proved that a false theory t_2, which exceeds another false theory t_1 in content, is always excessive in both falsity-content *and* truth-content over t_2. In such cases, Popper's conditions can never be met. This is a particularly important criticism, as Popper designed his definition explicitly to deal with false theories:

> Ultimately, the idea of verisimilitude is most important in cases where we know that we have to work with theories which are at best approximations – that is to say, theories of which we know that they cannot be true. (This is often the case in the social sciences). In these cases we can still speak of better or worse approximations to the truth (and we therefore do not need to interpret these cases in an instrumentalist sense).[120]

After accepting the technical criticisms mentioned above, Popper eventually retreated to the notion that verisimilitude is better characterised as heuristic concept and does not necessarily need to be defined formally.[121]

There are other difficulties with the concept of verisimilitude, though. Firstly, it remains somehow dubious what is actually meant by »truth-content« by Popper, because truth exists for him only as a regulative idea that is never attainable. There seems to be only one way out of this: We take as truth-content of a theory the predictions of the theory that have not yet been falsified, i. e. the predictions that have been confirmed. The truth-content defined in this sense is then the same as the »degree of corroboration«. Seen like this, claiming an increase in truth-content would be equivalent to demanding that economists must take falsification more seriously and build theories that are more successful in severe empirical tests (see section 2.2.1).

118 Popper (1963), p. 341 (Translation by S. D.).
119 See Miller (1974) and Tichy (1974). Niiniluoto (1998) provides a survey of the discussion that followed.
120 Popper (1953), Popper (1963), p. 343. (Translation by S. D.).
121 See Popper (1972), p. 59.

But this seems a contradiction to the many assertions of Popper saying that corroboration is not to be confused with truth in any way. The radical anti-inductivism of Popper is not only an obstacle for talking about true statements or theories – it makes comparison of theories by truth-content impossible.[122] A main point of falsificationism is that it discovers falsity and not truth, so Popper's early motivation for developing falsificationism (avoiding any form of inductive reasoning) is undermined by the whole project of assigning different degrees of verisimilitude to theories.

Even if we neglect these problems for the moment, it is still doubtful whether a comparison of theories employing the notion of truth in the above sense can be helpful at all. As we have seen, Thomas Kuhn offers arguments for the incommensurability of paradigms. These arguments can be extended to theories: At least in economics most theories are designed to solve a specific problem or to clarify only a certain part of the economic sphere. Therefore, direct comparison of truth-content is usually not possible. How can we say that aggregate-demand theory has more truth-content or falsity-content than e.g. human-capital theory? There is simply no common baseline. Direct comparison without any component of choice is only possible with data that can be exhaustively described on a one-dimensional scale. Certainly most scientific theories are not that simple.

Popper's concept of verisimilitude delivers an unambiguous outcome only when the truth-content and the falsity-content of a theory are entirely clear, i.e. if and only if from two theories that aim to explain the same phenomena and are equal in logical content, both have been tested on the same instances and one theory succeeds in predicting facts correctly in more cases than the other. This raises the question whether increasing truth-likeness is the right aim at all.

Due to the famous paper on economic methodology by Milton Friedman[123] and especially Lawrence Boland's[124] interpretation of it, most economists are roughly familiar with the conception of instrumentalism. This view sees science not as a quest for truth but as a useful tool for solving problems.[125] Here, the question of whether a theory is a correct deduction based on true premises, as presupposed in the DN-model of scientific explanation, is *not* what theory appraisal is all about. For instrumentalists, it does

122 See Grünbaum (1976).
123 See Friedman (1953).
124 See Boland (1979).
125 I take »instrumentalism« as the overarching concept that is defined by the refusal to speak about the truth-status of scientific theories or their elements. It includes more specific brands such as pragmatism, which defines science as the quest to solve problems.

not make sense to speak about »true« premises.[126] Now, if the usefulness of theories for doing a specific job is what counts and not the ontological relation between reality and the elements of a theory, the discussion about verisimilitude becomes pointless – and so does the demand to increase a theory's »truth-content«.

The instrumentalist view is certainly appealing in some respects. For example, it provides reasons against empirical work on the basis of economic theory: In this view, it does not really matter whether or not humans *really* behave rationally. Instrumentalism leaves the option for »as-if assumptions« viz., the assumption that people (on average, not individually) behave as if they were rationally deliberating.[127] Such as-if assumptions are appraised not by their empirical adequacy or truth but by their contribution to the creation of theories that help to solve specific problems while staying as simple as possible. Note that scientific explanation is still possible but loses its universal aspect, because an explanation is now only appraised in respect to a certain problem.

An example may serve to clarify this point: Neoclassical economic theory can explain why prices are higher if markets have a monopolistic structure (compared to polypolistic markets). The theory states that it is rational for company owners to raise prices in monopolistic markets, because this maximises their returns – whereas in polypolistic markets it does not, because customers will buy the cheaper products of competitors. Here we have a rational-choice explanation for the observed behaviour. But there is no claim that this explanation firmly rests on empirically observed premises; it is simply an *adequate* explanation in relation to the problem in hand.[128] The specific status of rational-choice explanations will be clarified in more detail in section 2.4.2.

Upshot:

The mechanical increase of »truthlikeness« is not a convincing methodological recipe for economics. Instrumentalism offers an alternative view, which leads to good arguments against empiricist methodologies by showing why a (seemingly) truer (in the sense of being more empirically adequate) theory

126 Interestingly, Hempel (as one of the creators of the DN-Model) notices that it is impossible to check the truth of single premises of a theory directly. See Hempel (1965), p. 112. It is difficult to see the use of the DN-Model once this concession is made.

127 Note that the use of as-if assumptions is possible under a realist view of science, too. This is the case when as-if assumptions are used for pragmatic reasons and the formula »as-if« expresses epistemic uncertainty, but the usefulness of questions concerning their truth is not denied in principle. See Mäki (1998b).

128 This is the point of Bas van Fraasen's theory of explanation. Van Fraasen takes into consideration that the same facts can demand for different explanations with respect to the problems we want to solve. See van Fraassen (1980), p. 143 et sqq. See also section 4.1.4.1.

is not automatically the better one.[129] Instrumentalism focuses on problem solving and therefore rejects problem-independent assessments of theories. This is different from the approach adopted by the empiricist methodologists as outlined in section 2.2. All those authors make *general* demands for how economic theory has to change and do not allow for the possibility that different problems need different solutions. This is especially true for Scott Moss' position, as he wants to build a new, more empirical economics from the bottom up.

Mark Blaug's view could be made consistent with instrumentalism if he merely demanded more severe testing to establish how helpful a theory is in solving a given problem. But as he demands a general testing of all consequences, and even components of theories, this interpretation is not possible.

Hans Albert's position is situated somewhat strangely between those of Moss and Blaug, as he inconsistently argues that the way of discovering the basic components of a theory does not matter all if the theory is severely tested, yet only some pages later he demands an empirical foundation for economics.[130] Apart from this inconsistency, Albert does not refer to specific problems but proposes a *general* cure for economics. As the arguments in this chapter showed, there are internal problems of such general argumentation, of which the debate about verisimilitude is only a special case.

2.3.2.3. Diagnosis of Progress – Pragmatic Problems

In addition to instrumentalist arguments, there are other pragmatic reasons why more empirical research or more falsification should not be defended dogmatically. Even if we could be sure that a more empirical economics would lead to better predictions, less empirical neoclassical economics could still be defended. This can be done if the instrumentalist view on science is pushed a bit farther: We stick to the notion that science is not a quest for truth but a quest for solving problems. If science is seen in this way, economic reasoning can step in: If scientific theories are means for solving problems, we can perform something like a cost-benefit analysis when comparing theories. This is one main point of what is done in the so-called economics of scientific knowledge (ESK), which will be discussed in more detail in section 4.1.3. Here it is merely used as a possible counter-argument against the empiricist methodologies.

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129 Note that instrumentalism does not dismiss empirical work in general, nor does it recommend any specific as-if assumption. It just leaves the question open and recommends seeking the answer in the usefulness of the results of a theory. It's all about the *right* theory, not the most descriptively correct theory.

130 Albert (1963), p. 375-376. The claim for an empirical basis is a contradiction to Hans Albert's epistemological programme of critical rationalism, which harshly rejects induction as a means of justification.

From the perspective of the economics of scientific knowledge, the usage of a theory or model is seen as a cost factor for solving problems. Of course, scientific theories do not solve problems directly but are rather the basic tools for creating more concrete tools that will then solve the problems.[131] Seen this way, a theory that would traditionally be called an advancement to the established standard because it predicts more precisely is not necessarily so if economic problem-orientation replaces empirical success as a criterion: If the problem to solve is simple enough, the most advanced theories can be overly complex. Even if all scientists agree that Einstein's theory of relativity predicts more precisely than Newtonian physics, there still is plenty of use for the old, simple theory. The same may hold for the relation between neoclassical economics and approaches based on empirical research.[132] From an economic point of view it is always the relation of input to output or cost to benefit that is decisive. There are no better solutions in an absolute sense, but only more economic means for achieving given ends. Note that this view can encourage fundamental research, even if in some cases it seems improbable that fundamental research directly leads to outcomes that help solving practical problems, in a similar vein as it is economically advisable to include high-risk stocks in a portfolio.[133] Fundamental research is of high risk, but in rare cases the reward can be very high.

Upshot:

The empirical methodologists discussed above seem to completely ignore the economic aspects of scientific research. For them, comparing theories is all about the output without regard to the input – or even worse, they see the increased empirical adequacy of the input-side (the assumptions) as an end in itself. When the aim of science is not exclusively the growth of knowledge but the efficiency of promoting it as well, it is hard to reject theories across the board for their empirical shortcomings. The aim of »explaining much by little« does not seem to be an absurd demand, which is why economic arguments in philosophy of science cannot be as easily neglected as one might think at first sight.

2.3.2.4. Problems with the Appraisal of Models

The empiricist methodologists are uncompromisingly arguing for more empirical adequateness – be it directly via using an empirically grounded fundament or indirectly via more serious testing. We have seen that instrumentalist and economic perspectives on science can deliver counter-arguments to this view. Now we consider arguments in favour of unrealistically simple and even unfalsifiable models. Such positions can be found in the scientific com-

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131 As an example, think of the relation between propositional logic, building a computer and using it to solve the problem of red-eye removal in photos.

132 Section 2.4.2 includes a discussion of case studies.

133 See Lütge (2001), p. 62 footnote 82.

munity that performs computer simulation of social dynamics.[134] In contrast to Scott Moss's methodological position, most economists in this field take KISS (keep it simple, stupid!)[135] as their methodological catchword. They see the complexities of descriptively more correct models not merely as disadvantageous because of their higher »costs« but because they doubt the basic premise that we can learn more in the social sciences if we head towards more descriptive adequacy right from the beginning.[136] Advocates of the KISS approach claim that we have no other chance than to simplify radically if we want to understand at least some features of complex systems like economics.

Thomas Schelling's models of segregation are a good example to illustrate this.[137] In these models black and white stones are distributed on a checkerboard, symbolizing the black and white inhabitants of a (north-American) city. Now a certain threshold-ratio of stones in the neighbourhood that have a different colour from the stone under consideration is defined. If this threshold-ratio is reached, the stone under consideration is said »to feel uncomfortable« and moves away from its original position. The astonishing result of such a simplistic model was that even if the individual threshold-ratio requires only 30% of stones in the neighbourhood having the same colour, complete segregation of the colours is the result after a few rounds of moving stones.

Such basic effects can get easily be lost sight of if a much more accurate and less schematic model is applied. The main argument of the KISS advocates points to the core of the idea of modelling: simplification is necessary for understanding. We make models in order to *reduce* the complexity of the real world, not to mirror it. Of course, good models do not *neglect* the complexities of the systems they try to represent, but striving towards high accuracy in every aspect means nothing less than the rejection of theorising – which can result in a mere collection of facts that may be descriptively highly accurate but rarely help in explaining matters.[138] This is an argument for why the call for more descriptive accuracy on the basis of economics cannot be sustained as per-se argument. The appropriate level of abstraction depends

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134 For a more detailed discussion see Pyka/Deichsel (2009). See section 5.1.4 for a case study.

135 See e. g. Axelrod (1997), p. 5.

136 KISS advocates do *not* oppose the idea of adding more empirical details in later versions of a model. A nice example for this can be found in the literature about the evolution of cooperation that started with a simple tit-for-tat model, which was thereafter refined in various respects. See Ball (2004), chapter 17-18 for an overview.

137 See Schelling (1971) for the now-classic article.

138 The term »explanation« is itself under philosophical discussion. The covering-law model of explanation is generally considered outdated due to several difficulties. I do not wish to enter into this philosophical discussion here, but refer to the abovementioned pragmatic approach to explanation of Bas van Fraassen, which will be discussed in more detail in section 4.1.4.1.

on the aim of the model. The understanding of fundamental mechanisms is the aim, the KISS method still seems the approach of choice. Highly complex models may (sometimes) accurately generate output, but they do not enable scientists to understand how it comes about, because complex models often develop their own life and produce artefacts that makes them difficult to interpret. A famous example is Sugarscape, which does not even aim to reproduce a real society, but is more like a sandbox toolkit for social scientists. In an elaborate version the Sugarscape world becomes unpredictable and can lead to greatly differing outcomes at the macro-level, even when started with the same initial conditions.[139]

The neoclassical standard model of supply and demand is probably better suited for analysing basic market structures than a model that tries to capture human behaviour with psychological methods. In many cases, however, approaches different from neoclassical theory might be much more useful. If institutional settings are at issue, some standard neoclassical models can hardly be used to make a point at all, because perfect competition is usually assumed. But the point here is not defending or attacking neoclassical theory but the plausibility of an uncompromising methodological call for less-abstract models. It seems this criticism would lose its thrust if it would account more systematically for the problems that are actually addressed by neoclassical models.[140]

All this suggests why it is not useful to talk about the »right« level of abstraction from a global point of view, and to consider that »everything depends on the problem«. This claim leads to the obligation to specify in advance the domain of a model. This can help precluding over-interpretations of what may be little more than an artefact of the formal specialities of a model.

Even if it is accepted that high descriptive accuracy is not applicable to abstract models, the outcomes of models must be hooked somehow to the empirical data. Take Schelling's segregation model again as an example: It is tough to falsify the results of that simulation, since it is based on so many intentionally wrong assumptions: People do not live on checkerboards and their decision to move houses is most certainly influenced by far more (and more complex) reasons than what is assumed in the model. Hence it would not be very astonishing if the simulation predicted behaviour that is completely remote from what we see in actual North-American cities, e. g. no segregation at all. When simulating social phenomena with the KISS approach, one is bound to a confirmationist methodology, developing simple models

[139] See Epstein/Axtell (1996), p. 92 footnote 32.

[140] Many neoclassical models can be criticized for lacking honesty when it comes to the problems they are dealing with. See section 5.1. Looking more carefully at the possible misfit between the claims of a theory and their fulfilment seems a promising task for future methodological research.

that reproduce stylised facts of the real world and offer a possible partial explanation for them.[141] Critical testing steps in at another point, namely when confirmation is already achieved. Only then it is possible to test the robustness of the model by changing parameters like board-size, form of neighbourhood, simultaneity of moving and the like. If the model survives such tests, further testing of the basic assumptions is not possible, as asking people if they have certain racial-thresholds in mind when choosing houses does probably not lead to honest answers. Such restrictions hold for a large part of economic modelling, where often the postulation of »invisible hand mechanisms« is the goal. In these cases it goes against the very point of the models to ask for empirical grounding of the basic assumptions.

Upshot:

If one accepts simulations following the KISS principle as scientifically useful, neither falsificationist (like Blaug) nor basis-empiricist (like Moss) convictions can be kept up. These methodologists cannot deal with the explanatory status of KISS models, which merely aim at offering *possible* mechanisms for social dynamics. KISS models are on the one hand decidedly simple, and therefore far away from empirical adequacy, and on the other hand committed to reproduce stylised facts, which is adverse to any falsificationist methodology.[142]

2.3.2.5. Difficulties with Assessing the Usefulness of A Priori Reasoning

The empiricist methodologies are unsuited not only for appraising abstract models but for the evaluation of a priori reasoning as well. These days it is widely accepted in science theory that there is no such a thing as synthetic knowledge a priori, i. e. there is no way of gaining knowledge about the world independently from empirical research.[143] This is the main reason why the methodologists presented in section 2.2 are arguing for more empirical and less formal (mathematical) research. I shall not try to take on the heroic task of defending a true aprioristic methodology here. But even if I do not believe that it is possible to gain knowledge about the world by purely formal deductions, there may be other justifications for them. If these justifications are sufficiently convincing, this would be a substantial counter-argument to the empiricist point of view that uncompromisingly asks for more empirical research.

In fact, it seems quite reasonable to tone down the wholesale condemnation of »a-priori speculation« a bit. Not everything that lacks empirical content is without any new meaning: If neoclassical economics is seen as an empty formal system, the theorems that can be proven in that system of course add nothing to that system; they are tautologies. Yet they may tell

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141 This line of argument will be filled with more detail in section 4.1.4.3.

142 See Arnold (2007).

143 A famous contrary position is held by Ludwig von Mises. See Mises (1933).

us something which we did not know before. Rejecting this view means rejecting the whole task of logics and mathematics and adopting the view that we need no theorems at all, because strictly speaking it's all in the axioms – which would surely be an absurd position. Economics could be considered nothing else than economically interpretable mathematics and justified as such. And indeed, this seems to be a characterisation of economics that many economists would agree upon.[144]

The practical usefulness of formal deduction can go further than just exploring theorems from a set of axioms. The famous impossibility theorems of social-choice theory can be used to show which properties of electoral procedures cannot be implemented together at the same time. Here the logical impossibility has real practical relevance – and it does not matter how strict the rationality assumptions underlying the deductions are, because the more strict those assumptions are, the more generally applicable the impossibility theorems become.[145]

There is a third way of justifying »empty theorizing«: Mathematic theories can provide an extremely useful structure for analysing data. Fisher's equation of exchange is a good example for this: It is empirically completely empty, but surely provides a nice framework for collecting and sorting data.[146]

Upshot:

There are many ways to justify purely formal work, which shows that the empiricist methodologists of economics are unjustifiably dogmatic in their attacks against it.

It must be emphasised that the reasons put forward here cannot be used to defend the view that we can learn something about the economy by looking at empirically empty equations.[147] It is certainly true that we need empirical work for that, but it would be dishonest to claim that such work is not performed at all.[148] As I will show in the succeeding section, there are already many researchers who are trying to overcome the empirical shortcomings of rational-choice models or are working on the integration of institutional settings with neoclassical economics.

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144 See Blaug (1998) and Rosenberg (1992).

145 See Kelly (1988), p. 60.

146 See Morgan (1999), p. 359 et sqq. and Hausman (1992b), p. 26.

147 It is a different question if we can learn *normatively* from such models.

148 Of course there is a lot of purely theoretical work in economics which cannot be tested at all. But again, the question of a disproportion between empirical and theoretical works cannot be decided a priori by methodological speculation. Where do the empiricist methodologists get their knowledge about the right way to do science? If they want to be consistent, they need empirical research for this as well. In section 4.1.4.1 I will present Larry Laudan's view that incorporates the above demand.

2.4. Progress in Economics – What Role Does Empiricism Play?

In the preceding sections we have observed some problems of the empiricist positions outlined in section 2.2. Now, given the passionate pleas against »empty formalism« in economics on the one hand and the difficulties to establish a truly empiricist methodology on the other, it is time to have a look at some case studies and see how the relation between theoretical works and empirical research manifests itself in the actual course of economic research in time. The first case study gives a longitudinal overview of economic-growth theory and its main line of progress during the last sixty years. The second case study presents two contrasting viewpoints on the rationality assumption in economics. In both case studies I will not give a full-blown description but will restrict myself to the level of detail that is necessary for a methodological discussion.

2.4.1. Case Study Macroeconomics: Growth Theory

Economic growth is a central topic for macroeconomic research. Today it is common to take the so-called Harrod-Domar models as a starting point for modern growth theory.[149] In these models investments are seen as the driving force for economic growth, as they generate additional demand via multiplying effects. Of course, investments increase the capital stock as well, which enlarges the productive capacity of an economy. The whole trick of Harrod-Domar models is computing the rate at which income and consumption must grow for the capital stock to be working at full capacity, i. e. all produced goods can be sold at the market.[150] As one can easily see, there is no *explanation* for growth in these models; they merely define equilibrium conditions in a purely formal way.[151]

The next step in growth theory began with two seminal papers by Robert Solow.[152] Technically speaking[153], his model uses a Cobb-Douglas production function with labour and capital as production factors, plus an external neutral technology variable. The assumption of perfect competition is used for

[149] See Harrod (1939) and Domar (1946) for the classic articles. Due to the structural similarities of both concepts, the term »Harrod-Domar models« was established. Note, however, that the causal relations between investments and growth are different in the two papers.

[150] For a short formal demonstration, see section 7.2.

[151] This is exactly Albert's claim; see Albert (1957), p. 387.

[152] See Solow (1956) and Solow (1957).

[153] Readers which are uneasy with economic terminology may skip this paragraph; the essential point is summarised afterwards.

estimating the production elasticity of the factors labour and capital: At perfect competition, standard microeconomic theory predicts a payment to the factors that is equal to their marginal productivity. Given that, one can use the proportion of interest rate to wage rate in order to estimate the production elasticity of the factors. Now, if the production elasticity of each factor plus the amount of economic growth in total and the growth of the factors labour and capital is known, the technology variable can be computed as residual.[154] By doing exactly that, Solow has shown that the *residual* is responsible for 87.5% of economic growth.[155]

Speaking less technically, Solow's theory tries to explain growth in output by growth of the factors labour and capital, both having decreasing returns, which is the reason why the residual in Solow's model – which is called »technical knowledge« – is the decisive factor for economic growth in the long run.

As one can easily see, Solow's theory does not have much empirical content either, as the biggest part of economic growth is »explained« by a residual factor. On the other hand, it clearly contradicts the Harrod-Domar hypothesis of the strong relation between investment and growth, because in the Solow model there are *decreasing* returns to capital growth.

If one assumes technical knowledge to be a public good, the Solow model is the basis for the so-called convergence hypothesis, which says that less-developed economies will tend to grow relatively faster than economies that have already accumulated a high level of capital because of the decreasing returns on capital in the Cobb-Douglas production function. Even if we accept that the Solow model does not deliver an explanation for economic growth, it cannot be denied that its basic structure is still present in much of the recent advances in growth theory that have tried to dig deeper into the mystery of the »Solow residual«.

The so-called »New Growth Theory« was the next big thing in growth theory.[156] Here, human capital is considered a part of the factor of capital, which has increasing (instead of decreasing) marginal gains – thus making growth in the long run possible. Human capital is now seen as the main source for technical progress. Additionally, »spillover-effects« are said to boost growth: Technical advances are rarely only useful for the aim they were originally designed for, but often they can be used in circumstances the inventors did not think of. If this happens, knowledge can »spill« across industry sectors and countries and thereby may cause productivity gains that were not intended by the inventor, resulting in additional economic growth.[157] Those spillover-

154 For a brief formal demonstration see section 7.3.

155 See Solow (1957), p. 320.

156 See Romer (1986) for a classic paper See Arnold (1995) for a good overview of subsequent developments.

157 Some recent publications are critical of the spillover claim when it is used too generally. See Pyka/Gilbert/Ahrweiler (2009).

effects lead on the other hand to an underinvestment into research and development of private firms, because they make technical knowledge partly a public good. Still, the crucial element that makes New Growth Theory different from its predecessors is the integration of positive feedbacks that help escaping the decreasing returns of capital as postulated in Solow's basic model. (Those positive feedbacks were initially subjected to heavy criticism, because they can make economies inherently unstable.)

A further argument often put forward in New Growth Theory stresses the differing preconditions across countries for the usage of specific technologies. This argument is headed against the assumption of technical know-how to be commonly usable knowledge and allows for different levels of technical progress in different countries. It offers an explanation for why the convergence hypothesis does not hold generally: poorer economies might not *be able* to use the newest technical knowledge, because they don't have the prerequisites to use it.

The most recent research in growth theory lifts the discussion about the applicability of knowledge in different countries to an even higher level. The approach is best characterised by the catchwords »social infrastructure« or »new institutional growth theory« and not only tries to explain the economic performance of a country by the amount of its capital, labour and technical knowledge but also takes the formal and informal *institutional* environment into account.[158] Empirical studies confirm the hypothesis that institutions are impressively important for growth.[159] Additionally, econometric studies show that Solow's convergence hypothesis holds for countries with similar quality of institutions.[160] The question of where »good institutions« that create economic growth come from is tentatively answered in some recent studies, but it cannot be said (yet) that this really is the next big step in growth theory.[161]

2.4.1.1. Discussion

The development of growth theory is a nice example for discussing empiricist methodologies. Hutchison, Albert, Blaug and Moss would all agree that Harrod-Domar models are to be dismissed. So would economists today. But what does this tell us? After all, Harrod-Domar models historically led to the Solow model, and the basic structure of this model still provides the background for recent developments that incorporate empirical research on institutions. This shows that it is not really helpful to »reject« certain models from a methodological point of view when there is no alternative offered: Solow's early model was almost empirically empty but nonetheless provided

158 See Jones (2002) for an excellent introduction.
159 See e.g. Leschke (2003).
160 See e.g. Knack (1996).
161 See e.g. Acemoglu/James (2006) and a discussion in section 5.1.2.

a good structural basis for further research, which added empirical content by developing and testing theories about new factors and preconditions for economic growth. It is important to note that this did not happen due to the empiricist criticism of economics, which was largely ignored by economists. Rather, the growth theorists noted by themselves that Solow's basic model was not the end of the story and that more detailed research on the residual called »technical knowledge« seemed the most fruitful way to proceed. As shown above, they added empirical content to the Solow residual, but they did not do it in the way the empiricist methodologists suggested: they neither changed the basic behavioural assumptions nor tried to falsify old theories by severe tests. Rather, they devised new theories and searched for empirical confirmation.

A similar development has been going on in other fields of economics as well. Mark Blaug presents several cases where economic models were adjusted to empirical findings.[162] A good example for progress in this sense can be found in the debate about the Phillips curve. In its best-known form the Phillips curve establishes a fixed relation between unemployment and inflation.[163] Due to theoretical criticism by Friedman and Phelps and its confirmation during the times of stagflation in the seventies, the classic Philips curve is now considered falsified, at least for long-run relationships.[164] But similar to growth theory, the Philips curve provided a useful basis for further research, as the observed short-run relationship between unemployment and inflation called for theoretical clarification. This led to models of the labour market that integrate variables such as power, information asymmetries, market rigidities, institutional variables, prospect formation and many more. However, the rationality postulate as theoretical foundation was only touched very rarely, and the old concept of the Philips curve provided the backdrop for much of the later research. This is similar to the development in growth theory in which the early Solow model still provides the basis for recent developments.

Upshot:

We can learn at minimum three things from this case study:

1. »Empty« models *can* provide a useful structure and analytical instruments for further research; therefore it is often not helpful to reject them for their lack of empirical content.
2. Economists *have* been digging into the empirical details and changed their models thereafter. At least *some* former proposals are now considered to be falsified due to empirical research.

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162 See Blaug (1990), p. 5.
163 See Samuelson/Solow (1960).
164 See Phelps (1967) and Friedman (1968).

3. In macroeconomics there seems to be no tendency to refrain from the rationality assumption, even if better descriptive accuracy is the aim.

These conclusions show that, on the one hand, accepting the basis-empiricist methodologies could have been harmful to economics, because this would have stopped the invention of models that now form the basis of whole research programmes. On the other hand, one can see that economics is not as hopelessly non-empirical as some critics describe it. Rather, by concentrating on the old, basic models the critics seem to forget later developments that, even if they are still based on the old framework, add empirical content in several ways.

The question of whether or not the reluctance of most economists to abandon the rationality assumption was a good move will be dealt with in the next section.

2.4.2. Case Study Microeconomics: The Status of the Rationality Assumption in Economics

The usage of the rationality assumption is an important issue when tackling the question of how much attention economists pay to empirical observation. It is important to note that the rationality assumption can be understood in two fundamentally different ways: normative and descriptive. Both cases need further clarification of what is exactly meant by »rationality«, as there is a wide range of possible interpretations. In economics, all interpretations have in common that they define rationality as the capacity to choose the best means for a given end, which is often abbreviated as means-ends rationality.[165] The stereotypical picture of the economic man concentrates on the short-term maximisation of money. However, I am not aware of any economist who would defend this view of the economic man, neither as a normative nor as a descriptive model.[166] If the rationality principle is interpreted *normatively* we cannot run into *empirical* problems, because what *is* has no bearing whatsoever on what *ought to be*.[167] This is why the normative inter-

165 This view of rationality manifests itself most precisely in rational choice models that work with an explicit micro-calculus.

166 Note that a strictly *short-term* maximising economic man isn't even able to make investments. It seems this view of the economic man is nothing more than a straw man used by its critics.

167 Of course it may be the case that a rationality criterion is *normatively* ill-suited for empirical reasons – e. g. if it poses demands that are unachievable by human beings. This is, however, a discourse that is completely remote to the evaluation of economic models.

pretation can be ignored in further lines of argument, because we are discussing theory evaluation of empirical scientific theories.[168]

Hence, the relevant quarrels take place on the descriptive side, where two opposed schools of thought are each claiming to have the superior concept. The first school tries to find rational explications for *any kind* of behaviour and so pushes rational choice to its very limits by accepting it to be an empirically empty concept. The second school tries to establish basic behavioural principles by means of systematic and experimental observation of human conduct. The two opposed schools of thought are hence the perfect candidates for a case study that tries to clarify the relationship between theory and empirical research in economics.

2.4.2.1. The Rational Choice Approach

Gary Becker became famous for assuming rational behaviour in circumstances where no one before him thought it could make sense. His work is a good example of how research can be based on an interpretation of the rationality principle that does not forbid any observable behaviour.[169] Particularly interesting examples are Becker's models of the family, where he offers rational reconstructions of marriage, childbirth and even the education of children.[170] In these models, families are represented as a kind of little factory – a multiperson unit producing basic goods such as meals, health, skills, children, self-esteem and others out of market goods plus time, skills and knowledge of its members.[171] As there is a huge literature about Becker's approach available[172] I shall outline only one model and omit the technical details.

In this model, Becker explicitly concentrates on how parents form the preferences of their children – which is quite an unusual thing for an econo-

168 Note, however, that a big share of work in economics is normative in nature, e. g. reform proposals that are based on libertarian convictions. James Buchanan justifies the usage of the economic man in his constitutional economics as a helpful worst-case scenario and thereby escapes criticism of its empirical shortcomings. See e. g. Brennan/Buchanan (1985), p. 69.

169 Becker's own terminology may be sometimes misleading in this respect. In the famous paper of Stigler and Becker »De Gustibus Non Est Disputandum« he states explicitly that humans act according to the principle of rational utility maximisation. See Stigler/Becker (1977), p. 76. This suggests the rationality principle to be observable by watching the actions of people, which is surely not what Becker intends to say.

170 A rather short sketch of the model must suffice here. After all, the basic idea and not the mind-boggling details are of interest for the question at issue. A detailed exploration can be found in Becker (1991); a short introduction can be found in Becker (1992).

171 See e. g. Becker (1992), p. 38 et sqq.

172 See Pies (1998) for a detailed discussion.

mist to do, as preferences are normally seen as given.[173] The basic idea behind Becker's model is pure common-sense thinking: The model builds on the idea that preferences are shaped in the early years of our life. Thus, parents have the power to influence the preferences of their children and will rationally try to teach them love and a sense of duty towards the parents, if parents think they may need support from their children later. If parents succeed in this, they can establish a very efficient solution to the problem of non-binding agreements in that domain.

With his model Becker offers an explanation for a part of preference formation and additionally gives an argument that makes it easier to accommodate the altruistic behaviour of parents with the assumption of rational utility maximisation.[174] If parents believe in the bonds to their children, they will invest more into their education and spend less money on pension schemes. On the other hand, social security systems have big effects on family structure, as they fundamentally change the incentives to invest in the relationship to one's children (and their education, health etc.).

Instead of going into further details, I will add a characterisation of Becker's general approach. Becker himself stresses more than once that it is the *approach* that distinguishes economics from other sciences and that there simply is no *scope* to which economic analysis can be restricted.[175] For Gary Becker, economic analysis is equivalent to untangling the costs that guide aggregated human behaviour in specific contexts.[176] By this, Becker offers a purely methodological justification of the rational-choice approach: He tries to reconstruct changes in aggregate rates as rational reaction to changes in restrictions (and not in preferences[177]) as they are perceived by the individual. This does *not* include the claim that real persons are rational or that their preferences do not change. Becker is merely committed to Popper's view that protecting the rationality assumption from falsification is more fruitful for theoretical learning in the social sciences and that falsifying it would be easy, but pointless.[178] Becker's approach is imperialist in the sense that he deals with many problems that were traditionally dealt with in sociology, social-

[173] In that sense, this approach is a turning away from Stigler/Becker (1977), too. See Pollak (2003), p. 11. For an overview of the described model, see Becker (1992), p. 49.

[174] Interestingly, the model leaves room for »real« altruism, which is specifically included by the variable »a«. See Becker (1992), p. 53.

[175] See e. g. Becker (1978), S. 3.

[176] See Pies (1998), p. 16 et sqq.. Note that in contrast to some critical views (See Lawson (2004a)), this definition does *not* make economics a »theory of everything« because, firstly, it is highly dependent on other sciences for making adequate guesses about non-monetary costs; and, secondly, it can neither deal with all questions that concern individual behaviour nor deal with normative issues or structural questions.

[177] Note that the preferences of children in the model described above are actually restrictions imposed on the children by their parents.

[178] For the best known manifesto of this view, see Stigler/Becker (1977).

psychology or law, but it is *not* imperialist in the sense that it aims at occupying these fields of research: As noted above, for Becker there are no natural limits of scope for sciences, but sciences are rather *defined* by their methods.[179]

To sum up, one could say that for Becker the status of the rationality assumption in the social sciences is equivalent to the status of the causality assumption in the natural sciences. It is not an empirical sentence, but rather the basic assumption needed to talk about laws and build models that can help understanding social dynamics.[180]

I shall leave it at this small draft of Gary Becker's work, since it is sufficient to see the basic line of argument he is putting forward and how he is using shadow-prices, implicit market-mechanisms and the consideration of psychological factors in order to apply economic theory to new areas of research.[181] In the last twenty years it became clear that Becker's imperialist use of the rational-choice approach has become an impressive success in the sense that it has attracted a critical mass of followers.[182]

2.4.2.1.1 Discussion

At first sight Gary Becker's approach seems to be the exact opposite of what the empiricist methodologists are demanding, as it seems not to make economics more empirical but rather protect it from all empirical disturbances. A more careful look reveals that this is not true: Becker in fact fulfils a central claim of Hans Albert by his extension of economics to other subjects: He does *not* do »pure« economics, because he integrates factors other than goods and prices in his analysis; more precisely, he *extends* the concept of goods and prices, which allows him to apply it to non-monetary factors as well. All choice-problems can now be reconstructed as cost-benefit problems. With his economic imperialism, Gary Becker at the same time *confirms* and *contradicts* Albert's assertion that pure economics is to be rejected. He contradicts this thesis in that he turns every analysed situation into a purely economic one by converting it into a cost-benefit problem. This makes the core of economic theory empirically empty in Popper's sense, as it no longer forbids any kind of behaviour.[183] But this is not true for specific models (as Becker pro-

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179 See Becker (1978), p. 15. Consequently, Becker firmly encourages other sciences to be imperialist in his sense as well, as this increases competition.

180 See Kirchgaessner (2004), p. 5.

181 Gary Becker analyses marriage, crime and even drug addiction as rational decision problems (among others). See Becker (1978).

182 Books like *Freakonomics* are clearly inspired by Becker's approach and have become popular bestsellers in recent times. See Levitt/Dubner (2005). Vromen (2009) gives a list of seven books of a similar nature, which proves the impact of Becker's approach. Even a sub-discipline of sociology is now using rational-choice models. See Hedström/Stern (2008).

183 As discussed in section 2.3.1.1 Popper allows for unfalsifiable or »metaphysical« assumptions.

vides them), which can predict outcomes very clearly and unambiguously (and possibly falsely) – when the postulated restrictions under which the economic agents act in certain situations are precisely specified. Contrary to the empiricist criticism, the success of available models using this methodology shows that the fruitfulness of a research programme does not depend on its empirical basis.[184]

If »pure« economics is considered to deal exclusively with market goods and their prices, Gary Becker confirms the thesis that pure economics would be poor economics. For Becker, economics cannot be a pure »astronomy of goods« but has to include factors other than monetary ones – such as time constraints or social norms – even if one aims at analysing classic economic situations.[185]

When discussing the appropriateness of rational-choice reasoning it is important to notice the status of this kind of modelling. Gary Becker does not promote the thesis that his models can yield predictions for single individuals, nor does he claim that individuals can calculate in a perfectly rational way or would verbalise their reasons for acting in a way that corresponds to rational-choice models.[186] The models merely refer to representative individuals that serve for explaining changes in macro-rates. This is an important restriction, as the aggregate nature of those models only allows for analysing averages as talking about »representative individuals« – neglecting to consider how macro-rates come about and what the distribution behind them looks like.[187]

The fact that rational-choice models refer only to changes in macro-rates and do not claim to represent the way actual humans decide gives an important hint as to why empiricist criteria are difficult to apply to rational-choice models: The problems with the appraisal of models as discussed in section 2.3.2.4, with the example of Thomas Schelling's segregation models, arise immediately.

Upshot:
Becker's models show how we can make sense of aggregate human behaviour using the rational-choice paradigm, even if the assumptions underlying some models may seem counterintuitive at first sight.[188] If one takes current psychological research as a standard, Becker's models are easily falsified on an individual basis. But compared to most other neoclassical models, Becker's

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184 I will discuss views that refuse to see any success in mainstream economic models later, in section 4.2.1.3.

185 See Becker (1992), p. 38.

186 See Becker (1978), p. 4.

187 If the data does not follow a normal distribution, but e.g. has two peaks, analyses using average data may be beside the point.

188 This, of course, leads to the fact that it is possible to derive absurd consequences from such models. See e.g. Bergmann (1995) or Rogeberg (2004).

models are closer to psychological research, as they extend the scope of rational choice to non-monetary factors that were previously only rarely taken into consideration. By this, additional factors can be integrated into the models even if they still contradict current psychological knowledge in some respects. Such contradictions can be justified by the fact that every model is made to clarify one specific problem and cannot be applied outside this domain. Becker's claim is not to be empirically accurate in every sense but to find systematic driving forces of specific situations by analysing how rational persons would react to them. Becker's position is here very similar to Popper's concerning the rationality principle:[189] Taken literally it is false, but it is a very useful paradigm – and there seems to be no alternative of similar scope in the social sciences.[190]

2.4.2.2. The Experimental Approach

The experimental approach became widely accepted as a part of economic research in 2002 (if not earlier), when Vernon Smith and Daniel Kahnemann won the Nobel prizes in economics.[191] This approach was first introduced the 1950s, when Herbert Simon came up with the concept of »bounded rationality«, which models human beings not as maximisers but as satisficers. The response from neoclassical thinkers was, of course, to reinterpret Simon's satisficing as maximising under the computational restrictions of the human brain.[192]

Much of experimental economics is concerned with falsifying (or confirming![193]) the assumption of the economic man as a rational short-term income maximiser.[194] Additionally, experimental economists deal with the question of how humans really behave when confronted with several standardised choice situations and which motives drive them. These are the same questions that the empiricist methodologists in section 2.2 were concerned with. In order to make clear how experiments can be conducted in economics, it is convenient here to outline a game that has been the basis for many experiments in economics: the so-called ultimatum game.

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189 See Popper (1967), p. 365.

190 See Becker (1992), p. 52. I shall pursue and intensify the discussion of whether or not he is justified in this in section 4.1.4.2.

191 Still, in 1998 Mark Blaug remarked on the difficulties experimental economics struggled with. See Blaug (1998). A good textbook introduction to experimental economics is Kagel/Roth (1995). A nice comparison between standard economics and experimental economics can be found in Schoefer (2005).

192 See Simon (1955) and Simon (1987) for classic papers.

193 In recent times the criticism has arisen that much of the research in experimental economics is merely an attempt to induce the expected rational behaviour under laboratory conditions. See Güth/Kliemt (2003).

194 Note that in this section I am necessarily referring to a non-tautological view of the economic man or »rationality«.

In this game, Player 1 has the task of dividing a given amount of money between himself and Player 2. Player 2's only option is to accept the offer or reject it. If he dismisses the offer, neither player gets the money. Now, according to standard rational-choice theory – which focuses on short-term income maximisation – Player 2 will accept every offer that is greater than zero, because he gets an advantage from doing so. As Player 1 is able to anticipate such a reasoning, it seems rational for him to offer only the tiniest possible fraction to Player 2, because this maximises his expected income from the game.[195]

Experimental results showed, however, that real human behaviour differs substantially from what standard rational-choice theory predicts: Player 1 claims on average only two thirds for himself[196] and Player 2 rejects offers lower than 20% of the total sum with a probability of 0.5.[197] Results like these often lead to the conclusion that humans are not egoistic maximisers but have a preference for fair distributions. Several other games produced results that can be interpreted in a similar way.[198] In every experiment some participants clearly diverged from what rational-choice theory would predict, but a large part of the actions observed still could be interpreted as maximising the monetary output.[199] It is important to stress the heterogeneity of the observed behaviour here, which makes it hard to derive general conclusions about human behaviour from such experiments.[200]

Experimental economists offer various different explanations for behaviour that contradicts monetary maximisation, but most of them can be summarised as follows:[201]

- Fairness/altruism: Humans do not maximise their own utility exclusively but have genuine preferences for fair distributions or the well-being of others. Especially reciprocal behaviour often occurs in the experiments and is seen as a key to the resolution of social dilemmas.[202]

- Intrinsic motivation: Material incentives are not the only sources of motivation for humans. Sometimes an act is performed »for its own

195 Since this is played as a one-shot game, there is no reason to invest into future cooperation.
196 See Güth/Schmittberger/Schwarze (1982), p. 375 et sqq.
197 See Fehr/Schmidt (2001), p. 5.
198 See Schoefer (2005) section 4.3.1.2.
199 See Schoefer (2005), p. 42.
200 Parts of evolutionary economics focus on the heterogeneity of human behaviour as a necessary condition for evolutionary processes to start. However, those theories do not look for a general theory of human behaviour but focus on the effects of diversity on the results of evolutionary processes. See Nelson/Winter (2002) for an overview.
201 See Frey/Benz (2001), p. 17 et sqq.
202 See e.g Bowles/Gintis (2002) and Falk (2003).

reasons« if something is thought to be »right« completely separate from utility calculations.

Even if this short introduction neglects a large number of experiments and interpretations, it should suffice for grasping the way experimental economists do their research – which enables us to begin a methodological discussion.[203]

2.4.2.2.1 Discussion

It is easy to see that experimental economists are moving in the opposite direction from Gary Becker and his notorious defence of rational choice: Instead of protecting the concept of the economic man from falsification by making it a tautological, heuristic principle, experimental economists try to confute it with their experiments.[204] This leads to models that differ from the standard models e.g. by introducing fairness or intrinsic motivation as additional preference. If such models are interpreted as confutation of the rational-choice approach, this view ignores the Beckerian interpretation of the rationality principle as a methodological rule and not an empirical statement. In fact, Becker's models (and those of his followers) are eager to integrate new factors that influence human behaviour or offer explanations that provide egoistic reconstructions for what seems altruistic at first sight.[205]

A constructive refutation that respects the rational-choice methodology would consist in establishing a different behavioural assumption of similar generality. However, up to now experimental economics has mostly analysed human behaviour in certain precisely defined situations – and even then it has not discovered universally valid behavioural patterns but rather quite heterogeneous conduct. This shows once again how difficult it is to take empir-

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203 Note that experimental economics is not necessarily restricted to testing the microfoundations of human behaviour. Bruno S. Frey deals, for example, with macro-effects and constitutional economics in experimental settings. See e.g. Frey (1997). Note also that experimental economics has reached a level of sophistication which makes a delineation of neuro-psychology and economics increasingly harder. »Neuro-economics« is all the rage in experimental economics. For an example, see Knoch, et al. (2006). For my methodological discussion, the differences between neuro-economics and normal experimental economics are irrelevant. Be aware that Don Ross sees no contradiction between neoclassical economics and neuro-economics, because he deals with economics of the human brain as biological system. See e.g. Ross (2008b).

204 The underlying cause for the methodological quarrels between the two schools may be a confusion that results in seeing the rationality principle as a rationality *hypothesis*. See Vanberg (2002).

205 See Schoefer (2005) chapter 5 for a good summary of rational reconstruction.

ical studies as a basis for building theories.[206] It shows as well that it is problematic to call such theories »more realistic«.

The fundamental difference between the two schools of thought lies in the way they wish to explain the economic world. Experimental economists are interested in finding out what actually motivates humans, whereas Gary Becker and the rational-choice school try to understand various macro-phenomena »in principle« by developing models that keep the homo-oeconomicus structure.

The works of experimental economists are important and badly needed for showing when and how human behaviour diverges from standard assumptions. However, it remains doubtful whether the introduction of additional *endogenous* incentives like fairness is fruitful for economic research. Gary Becker argues in a famous paper that we learn more when preferences are considered constant and we take variables such as fairness as *restrictions*.[207] This is, however, more a simple twisting of words, as it does not really matter if additional factors are modelled as restrictions or as preferences as long as they can be measured and interpersonally compared and are constant over some time.[208] If those restrictions are not met, postulating changes in preferences or restrictions is completely ad hoc and does not explain anything.

If experimental economists could *explain the success* of the classic economic models *and* surpass them in some respects, we could clearly speak of progress. Experimental economics is undoubtedly of high value, as it can deliver badly needed data for economic research. However, if its main goal is gathering socio-psychological data, it is only fair to ask in what sense experimental economics can be legitimately called economics and whether it is not instead a branch of social-psychology.[209] If experimental economists really try to build up a new science of economics out of the behaviour they observe, then »this

206 Even if someone were to come up with an alternative general model, the usage of the economic man could still be justified as calculated pessimism when discussing normative questions. See Brennan/Buchanan (1985), p. 69. As noted above, because their focus is on constitutional analysis they argue it is more helpful to model human beings as egoistic utility-maximisers than to assume benevolence. If all humans were benevolent by nature, we would not need laws and constitutions. But a small group of egoists can force the majority to behave egoistically as well, if only as defence.

207 See Stigler/Becker (1977), p. 76.

208 See Schröder (2007), p. 56-57.

209 See Güth/Kliemt/Napel (2003), p. 4. Of course psychological research can be of high importance to economics, but that is not the question here. The question is whether or not experimental economics can be rightfully called »economics«. The astonishing proximity of psychology to experimental economics can be seen in Knoch, et al. (2006). In the definition of the hard core of economics psychological questions are explicitly ruled out. See section 2.3.1.3.. Experimental economics is therefore a different research programme – and as such to be embraced, of course, at least from a pluralist point of view.

methodology is naively reductionist and illegitimately assumes that economics should not do what all successful science does, namely, model abstract aspects of its target phenomena instead of would-be complete and fully ecologically situated facsimiles of them.«[210]

Upshot:

The methodological analysis of this case study has shown that a claim for more empirical work is naïve when it is seen as a way to overcome rational-choice theory. If it aims at refuting the version of the economic man that concentrates merely on short-term income maximisation it has clearly won the war. But the question is, was there ever really an enemy to begin with – that is, has any economist ever really held this view? To be sure, using Becker's tautological interpretation of rational-choice theory, experimental results can be *integrated* into neoclassical reasoning and are no refutation of it. The empiricist methodologists seem to argue against a straw man when they oppose rational-choice models as empirically inadequate, because the basic rational-choice assumption is intentionally empirically empty and concrete models may be empirically inadequate in some results – but if they are, economists are eager to offer refined versions. So experimental economics is fine for calibrating and improving neoclassical economics, but it is neither a substitute for nor a refutation of it.

2.5. Conclusions in Between

This discussion of empirical methodologies and their problems should have made some points clear. Most importantly, I hope to have shown why it is not a useful methodological position to *generally* demand more falsifiability or more empirical research on the *foundations* of economics.

As the case studies showed, there *has* been (constructive) falsification and empirical progress in economics, and to claim more of the same is a moot point, as this presupposes knowledge about the optimal degree of empirical work. Of course it may be the case that economists are engaging too much in the formal development of highly abstract models, but this can be judged only from a historically distinct perspective, as there is a lot of applied research in economics as well.[211]

If there is a need to fight against the dominance of purely formal reasoning, experimental economics is on the right track and offers a much more serious attack against the rational-choice school than does purely methodological criticism, because its results force neoclassical economists to think about

210 Ross (2008c), p. 473. See Schröder (2008), p. 229 for a similar argument.

211 Don Ross argues that many philosophers and methodologists tend to overlook the vast amount of empirical work being done in economics. See Ross (2008a), p. 308-309. See also Fitzenberger (2009).

the status and the range of their findings when they want to use their rational-choice models. So even if experimental economics cannot falsify the rational choice approach in toto, it can indeed refute single models or show the limits of their predictive power.

In the foregoing section I gave an account of various arguments that demonstrate problems for empiricist methodologies, of which the first bunch concentrated on the difficulty of finding a solid basis for falsification and inductive reasoning. Popper realised the advantages of protecting the rationality principle from falsification, a position which eventually led to a problem-oriented conception of holism and coherentism. The second bunch of arguments showed the limits of empirical methodologies when it comes to assessing the quality of economic models.

Of course these arguments are not the end of the discussion. Methodological positions other than the ones presented at the beginning of this chapter arose, trying to offer a solution for the obstacles faced by empirical methodologies; I will deal with them in the next section.

3. POSTMODERN REACTIONS

We have seen that the empiristic methodologies face several difficulties that cause them to be poorly suited for the evaluation of economic theories. Whereas the former section only intended to outline the problems of empiricist methodologies, I will now take the next step and present postmodern economic methodologies that have their basis in a philosophical school of thought which doubts many epistemological principles that are presupposed in empiristic methodology as well as in common-sense thinking. An important underlying idea was already presented in section 2.3.1.4 and is well characterised by the term »holism«, i. e. the rejection of the idea that anything can be justified by reducing it to some »fundamental« empirical elements that are beyond reasonable doubt. As the philosophical movement is of high importance for economic methodology, I will first present the development of *philosophical* ideas and then show the implications they had on *economic* methodology. The latter will be done through an exposition of Bruce Caldwell's and Deirdre McCloskey's methodologies, which are prominent examples for methodological thinking that is clearly inspired by postmodern currents in philosophy.

Following the exposition I will discuss whether postmodern methodology offers a way out of the problems we noted with empiricist positions and if it can be helpful for theory appraisal in economics.

3.1. The Relativistic Challenge

The commonality of the following positions is their relativistic attitude. Relativism in its broadest sense is the thesis that there are no absolute judgements – that all judgements can be valid only relative to some external standard. Critics say this would lead to complete arbitrariness, as there are of course no absolute meta-standards for judging the standards. The detailed discussion of the consequences of relativism for philosophy of science and particularly for theory evaluation in economics follows in the next chapters.

All of the positions discussed below argue against the very notion of a somehow directly given external data and by this are principally opposed to a naïve form of foundationalist empiricism, thus opening the door for relativism.

3.1.1. The Philosophical Background for Postmodern Economic Methodology

I have selected three famous philosophers here in order to set the stage for postmodern economic methodology. It is important to note that only the last one, Paul Feyerabend, is usually called a »postmodern« philosopher. The other two, Willard Van Orman Quine and Nelson Goodman, are commonly regarded as analytic philosophers, but as I shall show, they developed crucial arguments for postmodern thought. As a precise definition of »postmodern« is not available, I shall define postmodern philosophy as the faction of philosophy that is opposed to talking about an externally given reality or a foundationalist philosophy and rather endorses a far-going relativist attitude.

I will start by introducing Quine, the classic author who inspired the epistemological turn away from reductionism; he will be followed by Goodman, an even more radical relativist; and finally, I will end with the anarchist philosophy of science of Paul Feyerabend, in order to establish a direct connection to *methodological* questions.[1] Thomas Kuhn is surely of greatest importance and might be well called the father of postmodern methodology, but I will skip him here, because his methodology has already been laid out in detail in section 2.3.1.2 as a rather descriptive criticism of empiricism and less a sketch of new epistemological ideas. As we are aiming only for an understanding of the philosophical movements that influenced economic methodology, a short overview of the respective positions will suffice.

Any discussion of epistemology has to start a fortiori with a reference to Kant and his Copernican revolution in this field. Since the days of his *Critique of Pure Reason* it is a common cliché that we cannot know anything about how things *really* are. In Kantian terms, the »thing-in-itself is unknowable«. Accepting this, epistemology is reduced to the task of analysing the conditions of our capabilities for experiencing the world and is then more appropriately called transcendental philosophy. Kant attacked the simple empiristic view of the mind as a »tabula rasa« that becomes subsequently filled with experience data and in this way gains knowledge about the external world.

We have seen in section 2.1 how Rudolf Carnap and the Vienna Circle tried to rehash classic empiricism by making use of the tools of modern logic. It was up to Willard Van Orman Quine to play the role of Kant again, attack some of the fundamental aspects of this philosophical school and switch

1 Of course there are many philosophers who would fit in this line, most notably Ludwig Wittgenstein and his change from *Tractatus* to *Philosophical Investigations* (See Wittgenstein (1921) and Wittgenstein (1953)), but a choice had to be made, and the discussion of additional positions would not have added much that would be useful for discussing economics.

the main focus in epistemology to the detailed study of how we perceive the world.

3.1.1.1 Willard Van Orman Quine

Even if it is usually very hard to specify the beginning of a new era in philosophy, Quine's »two dogmas of empiricism« clearly marks such a turning point. As the destructive force to empiricism of this paper has already been roughly illustrated in section 2.3.1.4 I will concentrate more on the parts of Quine's philosophy that influenced later postmodernist, relativistic thinking in philosophy of science.[2] The most important aspect in this regard is surely Quine's holistic worldview, which is most clearly presented in his book *The Web of Belief*:

> It is not the contemplated hypothesis alone that does the implying, but rather that hypothesis and a supporting chorus of background beliefs. Nor is it usually a simple observation that is implied, but rather a conditional prediction that if a certain step is taken the observation will ensue.[3]

Using the example of the simple sentence »Water boils at 100° C«, Quine shows how many background convictions are present even in a seemingly simple case.[4] As a result of this – which is most important for postmodern methodology – we can always readjust an incorrect theory by making changes that are drastic enough to make the theory compatible with the data. Such changes can include even the rules of language or logic, as for Quine there is no knowledge a priori, which would be sacrosanct. Of course, this has led to many objections, including a famous one stemming from Adolf Grünbaum:

> … if someone were to put forward the false empirical hypothesis H that 'Ordinary Buttermilk is highly toxic to humans', this hypothesis could be saved from refutation in the face of observed wholesomemess of ordinary buttermilk by making the following 'drastic enough' adjustment of our system: changing the rules of English usage so that the intension of the term 'ordinary buttermilk' is that of the term 'arsenic' in the customary usage.[5]

If this was all there was to say about theory adjustment, Quine's thesis would be rather trivial, as everything could be saved from falsity by changing the meaning of words. But obviously it is not as easy as that. Even if Quine argues that logic and language may change, as they are (like everything else) ul-

[2] Quine's works had even more impact on the philosophy of language, but this field is too far away from my topic to be discussed here separately.
[3] Quine/Ullian (1970), p. 103.
[4] Quine/Ullian (1970), p. 103 et sqq.
[5] Grünbaum (1962), p. 20.

timately empirically justified[6], that does not mean they should be easily altered, because they are the very fundaments of our knowledge. Quine's idea is to repair a theory in such a way that other fields of established knowledge are only minimally affected by this change, or as he puts it, »not to rock the boat more than need be«.[7] But still, systems of knowledge very different from the present scientific one are indeed conceivable (even if hard to imagine), and if they would make no use of classical logic at all, they could not be accused for that per se. This is a very liberal, one might even say relativistic view, compared to traditional empiristic positions and clearly influenced postmodern economic methodologists.

Another important point in Quine's position is the empirical *underdetermination* of theories, which means that the observable data is never sufficient for selecting a single theory as the right one. There are always other candidates available that fit to the same data equally well. In his major work *Word and Object*, Quine shows that even logically incompatible theories can lead to the same empirically observable conclusions. Quine explains this by means of his famous example of a field linguist trying to learn a completely unknown language just by observing how it is used.[8] In the example, every time one of the natives sees a rabbit, she utters the word »gavagai«. According to Quine, the field linguist is not justified to conclude that »gavagai« means »rabbit«. It is perfectly consistent with the observed facts to conclude that »gavagai« means in fact *not* »rabbit« but »part of a rabbit«, »state of a rabbit«, »I like rabbits« or anything else that is consistent. There is no guarantee that a finite set of observations can identify the correct meaning of »gavagai«.

All this means that traditional empiristic criteria can provide much less guidance in theory appraisal than empiricists claimed, and this increases the space for radically different theories and methodologies. Like many of the postmodern philosophers after him, Quine argued strongly against the very idea of a »first philosophy«, an epistemology that tries to find firm grounds for our knowledge and argues for a radically modest ontology that culminates in the slogan »to be is to be the value of a bounded variable«[9]. As we will see in later sections, Quine's arguments can be taken as a basis for pluralist or relativistic methodologies. Quine himself, however, took a different path and argued for the naturalisation of epistemology, meaning that the natural sciences – and particularly psychology – should crowd out philosophy from this domain.[10] In Quine's holistic thinking, this can be justified by their rep-

6 This is not to be confused with a foundationalist empirical justification, as we are still speaking in holistic terms. In such a framework logic and language are justified via the working of the whole system they are the basis of.

7 Quine (1990), p. 15.

8 See Quine (1960), p. 29 et sqq. for the locus classicus of this example.

9 See Quine (1948) for the source of what later reduced to this slogan.

10 See Quine (1969), p. 75.

utation in our world: The knowledge of the natural sciences is the role model for what we accept as knowledge[11], which is of course a purely pragmatic reason. However, this is a circular justification: We should trust in the natural sciences for finding out the truth, because we trust their results most. But a circularity of this kind cannot be avoided in a holistic system, because by definition of holism there is no fixed foundation for any sentence; rather, the system gains its credibility by the way the parts of our web of belief mutually support each other.

In principle, again, there is no difference between metaphysics and science for Quine, as only the consistency of a system counts and a recourse to fixed foundations is no legitimate justification.[12]

Accepting this, it was no big step to reach even more relativist views that reject Quine's preference for the natural sciences. Nelson Goodman presents a particularly attractive alternative and will be discussed in the next section.[13]

3.1.1.2. Nelson Goodman

Nelson Goodman takes many of Quine's arguments for granted but nonetheless draws radically different conclusions, particularly concerning the status of scientific research. Quine initiated the raising of doubts about the fundamental differences between science and other forms of knowledge, but he ended up recommending scientific methods even for philosophical questions; Goodman openly advises a far-reaching relativism. This is another important step towards postmodern methodological positions.

Let me start by describing the basic features of Goodman's framework in order to give some substance to the preceding assertions. Goodman has made many extraordinary claims, such as 'worlds are rather made than found' and 'truths conflict with each other'. The basis for these relativist convictions can be found in his first major book, *The Structure of Appearance*[14], although his more recent work *Ways of Worldmaking*[15] expresses many of his thoughts in a less technical way. Goodman denies that any property of a system is intrinsically basic – an important difference to the empiricist methodologists, who take observables to be the most basic properties. Goodman, however, sees the case of empiricism only as a methodological *decision* to take sense-data as the primitives of a system. This is by no way an error in itself, as Goodman be-

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11 At least, it undoubtedly fits better than philosophical a priori speculation.

12 See Quine (1951), p1.

13 Richard Rorty could have been selected on equal grounds, but Goodman seems best suited for presenting an interim position in *philosophy of science* between Quine's naturalism and the even more relativist methodology of Paul Feyerabend. Rorty's point is primarily a destructive one, attacking the very possibility of any epistemology and reasoning about what »ultimately« may turn out as true.

14 See Goodman (1951).

15 See Goodman (1978).

lieves that any primitives can be chosen to construct a system, as there are no things that are *really* basic – the question of whether something is »basic« or not can only be answered relative to a system. But without any reference system we cannot express anything, Goodman says.[16] Goodman harshly attacks the epistemic realist's hope that there can be such a thing as »the one true system« which allows us to see the world as it *really* is.[17] The fact that there can be conflicting systems that are equally acceptable does not mean that our criteria of acceptability are too weak. They are just different for different purposes. Consequently, science is not a privileged way of exploring the world but just one system among many that are equally valid.

From this, it follows that »systems« and »worldviews« or simply »worlds« are made rather than found, in Goodman's view.[18] This is why even opposing views can be true, when they refer to different systems. One of the simplest examples for this is surely the view that the earth is resting fixed (mostly true in cartographical systems) which is opposed to the view that it rotates (true for current astronomy). But often there is no overlapping truth behind different truths in completely different systems, such as arts and physics. Such systems can neither be reduced to a »deeper« system nor translated into each other. Yet they can be correct versions of the world for their own purposes. Does that mean that anything is possible? In a way yes, everything is *possible*. But it does not follow that every version of the world is equally well suited for the same task, which is ultimately the reason for developing complicated theories (or making worlds) at all.[19] Goodman insists that there are rigorous restraints for worldmaking, such as consistency, coherence, respect for applicability etc.[20]

As foundationalist justifications are not possible in Goodman's relativistic view, the quality of systems can only be justified by a process called reflective equilibrium, i. e. the idea of using bidirectional adjustment between general principles and specific results with the hope of arriving at an acceptable compromise between the two.[21]

Even if Goodman's relativism is quite far-reaching, he still subscribes to some principles of rightness and does not imply that *all* systems are equally valid for every task. Furthermore, he suggests preferring better »entrenched« predicates to less »entrenched« ones in his discussion of the problem of in-

16 See Goodman (1978), p. 18.
17 He frequently discusses optical illusions in order to support this claim. See e. g. Goodman (1978), chapter V.
18 The philosophical term for this is »constructivism» – the world is not a given but is rather formed by the way we perceive, talk, reason etc. In that sense one could say »to grasp something is to make it«.
19 See Goodman (1978), p. 36.
20 See Goodman (1978), p. 149 et sqq.
21 Even if the term »reflective equilibrium« is usually associated with John Rawls, it was coined much earlier by Nelson Goodman. See Goodman (1955), p. 85.

duction, which shows a certain amount of methodological conservatism.[22] This conservatism is overcome through the work of the next protagonist in our series of philosophers of science, Paul Feyerabend.

3.1.1.3. Paul Feyerabend

Paul Feyerabend is surely the most glamorous person in the history of philosophy of science. During his lifetime he was constantly attacked for both his radical positions and his unorthodox behaviour in academia, but he nonetheless achieved important and highly paid university positions, and his work has become increasingly accepted as serious philosophy rather than mere provocation. His position is one of the most far-reaching relativistic conceptions imaginable; his central theme fights against any kind of fixed rules or standards.

Feyerabend started his career as an adept of the Popperian school of critical rationalism, but soon concentrated on the difficulties of separating theory from observation, which led him to adopt the demand for theoretical pluralism quite early.[23] In 1969 Feyerabend argued directly against the idea that any good science must be linked to observation data,[24] and very soon his position radicalised into a general criticism of the dominance of reason as normative concept. For Feyerabend there are no sacrosanct standards at all, and science is not a privileged way to gain knowledge. In his first book *Against Method*[25] Feyerabend aims to show that great progress in science very often is accompanied by complete and intentional ignorance of any so called »scientific method«, from which he concludes that dogmatically following accepted methods does not lead to progress. His attacks concentrate on the seemingly undogmatic method of critical rationalism, arguing it is self-contradictory to establish (dogmatically) a set of rules when aiming at an open and undogmatic process of science.[26]

According to Feyerabend, the only way to get out of this paradox is to accept »anything goes« as methodological principle. This does not mean that no method can be chosen or argued for, but quite the opposite: Any method can be argued for by its own standard, but it cannot be imposed on others or generalised. Neither does »anything goes« entail the suggestion to do whatever one likes in science – it just means that no method is sacrosanct.[27] Feyerabend called his position »methodological anarchism« or dadaism. Under-

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22 See Goodman (1955), p. 121. For a discussion see e. g. Kahane (1965).
23 See Feyerabend (1960) and Feyerabend (1963).
24 See Feyerabend (1969).
25 See Feyerabend (1975).
26 This line of thought can be found in many works of Feyerabend because his fight against the Popperian tradition (and Popper personally) is a key issue in Feyerabend's work. See e. g. Feyerabend (1987), p. 184 et sqq.
27 See Hoyningen-Huene (2002), p. 36-37.

standably, such a position deprives the methodologist from the possibility of claiming his thoughts to be anything more than a personal opinion. Of course this had an impact on Feyerabend's writings themselves, which become less structured and more poetic in later years. In which sense is Feyerabend an even more radical relativist[28] than Goodman? Where Goodman still accepts consistency as an important criterion, Feyerabend argues that even the »minimal criterion« of consistency can be a hindrance to scientific progress:

> … it has emerged that science is always full of lacunae and contradictions, that ignorance, pigheadedness, reliance on prejudice, lying, far from impeding the forward march of knowledge are essential presuppositions of it and that the traditional virtue of precision, consistency 'honesty', respect for facts, maximum knowledge under given circumstances, if practised with determination, may bring it to a standstill.[29]

In a similar vein, he attacks the commonly demanded conservatism in theory dynamics, i. e. the demand that a new theory should include the successful parts of a predecessor, or that more entrenched predicates are to be preferred to less entrenched ones. Why should a new theory be assessed on the standards of the old one, Feyerabend asks, seeing the demand for »conservative« progress as an unfair and unjustified advantage to established knowledge.[30]

Now, after a short summary, let's see how the described post-positivist movements in philosophy of science made their way to economic methodology.

3.1.1.4. Summing up

The chosen authors share the common conviction that there are no firm grounds upon which knowledge can be built.[31] All the discussed approaches are anti-foundationalist in this sense and by this are opposed in principle to

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[28] Feyerabend later often protested against this label and talks of a »so-called relativism« because in later years he realized that »relativism« is in fact a methodological programme, something which he tried to avoid endorsing under all circumstances, as he believed that no one should give any kind of advice to others. See Feyerabend (1991), p. 116 et sqq.

[29] Feyerabend (1975), p. 260.

[30] See Feyerabend (1975), chapter 3.

[31] It is of course an arbitrary decision which contribution to take as criticism of naïve empiricism or falsificationism and which one as a constructive contribution to postmodernist thought. This is why Quine appears in both sections. A similar case could be made for the Lakatosian methodology of scientific research programmes, but Lakatos's work is usually seen as a further development of falsificationism rather than a work of postmodernism, since it very much stresses the rationality of the research process.

methodologies that take empirical observation as their »gold standard«. Popperian falsificationism is different from this, because even if it does not directly demand an empirical fundament for theories to qualify as scientific, it needs the conventional acceptance of stable background conditions in order to be able to specify the falsifying conditions of a theory. With his consequent holism and his radical conclusion that even logic is finally founded in empirical observations, Quine's naturalistic defence of the scientific method was different from classic empiricism. This opened the door to genuinely relativistic positions like those of Nelson Goodman and, most radically, Paul Feyerabend. With these alternatives in hand, some economic methodologists were eager to leave the empiricist or falsificationist position behind and build their views on the new currents of philosophy.

3.1.2. Postmodernism in Economic Methodology

This section sketches out some of the major movements in postmodern economic methodology that were inspired by the philosophical movements presented above.

3.1.2.1. Methodological Pluralism

In the early 1980s the number of economic methodologists began to grow rapidly, and what before was a field for some rare specialists became a vivid scientific community.[32] One reason for this may be the rising awareness that methodology does not necessarily consist of »rational reconstruction« of scientific research programmes or normative reasoning about the empirical content of economic theories. After the philosophical critiques of the notion of one overarching concept of rationality and the connected idea that there could be only one universally justifiable set of rules for the proper conduct of science trickled through to economic methodologists, they started working in a much more descriptive and historical way than before. Many used a Lakatosian framework to clarify the underlying structure of economic schools of thought by reconstructing their methodological »hard core« and »protective belt«, but left out the component of appraisal, which is difficult to carry out with Lakatos' philosophy.[33] In his book *Beyond Positivism* Bruce Caldwell was among the first to state explicitly the fall of the so-called »Received View« of empiricism and falsificationism. He called his alternative to the old rule-

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32 E. g. many of the now classic books were published in these times. See Blaug (1980), Boland (1982), Caldwell (1982) and McCloskey (1985).

33 See Hands (2001), p. 287-288 for an impressive overview of Lakatosian works in economic methodology.

based and monistic methodologies »methodological pluralism«.[34] I will skip most of the anti-empiricist part of his book because many of the arguments have been dealt with already in the above sections with a more direct connection to economics. A short summary helps nonetheless for understanding both Caldwell's and many other methodological positions. Caldwell arrives at his methodological pluralism by sketching out the problems of various attempts to define criteria for theory choice:[35]

1. Confirmationism: This conception, started by the Vienna Circle, promised to deliver clear-cut distinctions between good and bad science but got into problems by Popper's criticism of inductive logic and several paradoxes of confirmation.
2. Falsificationism: In order to overcome the problem of induction by falsifying wrong theories, a reliable set of falsifiers is needed. The empirical basis of falsificationism is seemingly based on facts, but Popper admits that it is a matter of convention which facts are regarded as basic.
3. Paul Feyerabend's position is an attack against any rule-based methodology and prescription and argues that no theory whatsoever should be excluded. So he is against any definition of criteria for theory choice but argues in favour of the maximal proliferation of theories, which culminates in his anti-prescriptive slogan »anything goes«.
4. Thomas Kuhn proposed to view some standard criteria (accuracy, consistency, scope, simplicity and fruitfulness) as norms and values of the scientific game and let the players discuss them for themselves. It is doubtable if such a position can be really regarded prescriptive in any substantive sense, as Kuhn requests nothing more from practising scientists than to find their criteria themselves.
5. Lakatos's methodology of scientific research programmes promises to combine the best of both worlds in being descriptively accurate and offering criteria for deciding whether a programme is progressive or degenerative. But looking more closely at Lakatos's proposals, it becomes clear that there is no clear-cut rule at all for deciding this. A degenerative programme can turn progressive at any time, and we can only guess whether a certain field of research has come to an end. By stressing the end of (Popperian) instant rationality without being able to define time restrictions for a slower version, Lakatos's methodology fails to give proper advice for theory choice.[36]

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[34] Of course there are now many pluralists in economic methodology, but for reasons of brevity and clarity I will stick to Caldwell in this section. For an overview of pluralism in economics see Salanti/Screpanti (1997).

[35] Quoted from Caldwell (1982), p. 221 et sqq.

[36] See Feyerabend (1965) for an early criticism of Lakatos along these lines.

Caldwell concludes from this that »no robust prescriptive algorithm of choice has been discovered by the […] philosophers«[37] and continues by underscoring the high importance of descriptive work for economic methodology. In order to support his thesis of the unachievability of a single prescriptive method, Caldwell discusses problems of application of various criteria for theory choice in a confirmationist framework and the difficulties of falsifying economic theories due to various uncheckable initial conditions, the absence of general laws and problems of data representation. In a later publication, Caldwell briefly presents five problems of monistic methodologies:[38]

1. A single set of immutable standards for science is a chimera.
2. Therefore, the claim that any set of (bad) standards is better than no standards at all is wrong.
3. Monism fails to appreciate the richness of science.
4. Monists try to tell scientists how to proceed correctly; this has led to a lot of resentment towards methodology in general.
5. Monistic methodologies cannot keep the promise of providing defence against totalitarian standards; those are the duties of the »citizen-scientist«, not the methodologist.

This discussion leads Caldwell to his proposal of »methodological pluralism« and the recommendation for methodologists to withdraw from the search for a universal method for science. Caldwell considers other tasks to be more fruitful and lists those:

> […] to foster an understanding of the scientific process among the members of hisprofession; to systematize jargon; to rationally reconstruct the methodologicalcontent of various research programms; to promote an environment in which bothnovelty and criticism can operate freely.[39]

Caldwell later characterised the idea behind his pluralism as »understanding what economics is all about and […] by doing so improv[ing] it.«[40] He recommends starting a methodological analysis that is done by both the methodologists and the practitioners of a specific field. When reconstructing a research programme, assuming a specific framework of analysis is unavoidable – which is why it must be explicitly stated.[41] The next step consists of the critical assessment of the methodological core previously reconstructed.

37 Caldwell (1982), p. 228.
38 See Caldwell (1984), p. 237 et sqq.
39 Caldwell (1982), p. 245.
40 Caldwell (1984), p. 234.
41 Strangely, he speaks later of methodological pluralism as the »attempt to practice value-free evaluations«. See Caldwell (1984), p. 241.

The genuine task of methodology for Caldwell seems to be meta-methodology, which gives overviews of research programmes and puts their methodological hard cores under critical scrutiny. Caldwell refrains from giving any methodological principles which could guide this criticism, but insists that that any programme should be criticised *on its own terms*.[42] He openly admits the relativism intrinsic to his position and in principle subscribes to Feyerabend's slogan »anything goes« in methodological terms.[43] However, Caldwell's rhetoric is less provocative than Feyerabend's, and he tries to conciliate his readers by stating that methodological pluralism does not necessarily lead to methodological anarchism in economics, as Caldwell assumes some substantive theoretical contributions to be taken seriously by *any* methodologist.[44]

He sees a possible threat to his position in the lack of points for criticism if everyone were to become a methodological pluralist, but argues that a consistent pluralist cannot recommend pluralism for everyone, as this would amount to recommending a »single universalist nonmethodology«[45]. Caldwell pragmatically justifies his methodological pluralism as useful for methodological discussions and offers neither an attack to theories of truth nor a connection to one of them, leaving this task to philosophers.[46] This is different with Deirdre McCloskey, another important postmodern methodologist of economics, who explicitly argues against the notion of Truth (with a capital T!) and will be dealt with in the next section.

3.1.2.2. The Rhetorical Turn in Economic Methodology

The so-called rhetorical analysis of economics has become one of the most influential strands in economic methodology. In cases like this it is usually not easy to find a definitive starting point, but here it was clearly Deirdre McCloskey, who started the debate with the publication of »The Rhetoric of Economics« in the *Journal of Economic Literature* in 1983 and the following book of the same name.[47]

To avoid confusion it is important to note that the word »rhetoric« is used here in the broadest sense and is attributed to any kind of disciplined conversation. The aim of rhetorical analysis is *not* unmasking the »mere rhetoric« of economics in contrast to its substance but discovering how economists ac-

42 See Caldwell (1982), p. 248-249. Later he allows the methodological pluralist to apply any criteria he likes. See Caldwell (1984), p. 239.

43 See e. g. Caldwell (1984), p. 242.

44 See Caldwell (1982), p. 251.

45 Caldwell (1984), p. 241. He does not offer arguments for the alleged inconsistency, but merely expresses his hope that the methodological discussion will continue.

46 See Caldwell (1984), p. 242-243. He accepts that this deprives him of the possibility to speak of progress.

47 See McCloskey (1983) and McCloskey (1985). Even if there are many others using the rhetorical approach I will stick to McCloskey for reasons of clarity and brevity.

tually persuade each other about the rightness of their ideas. This does not imply that they are using cheap tricks; according to McCloskey, rhetoric is rather unavoidable, as all conversations are necessarily rhetorical.[48] She is convinced that »economists do not follow the laws of enquiry their methodologies lay down«[49] but thinks that this would be a good thing to do. Many of her publications and books begin with a fundamental criticism of »modernist« (as she calls it) methodology and traditional epistemology in general. As a basis for this criticism McCloskey lists the ten commandments of modernist economic science as she sees them:[50]

1. Prediction and control is the point of science.
2. Only the observable implications (or predictions) of a theory matter to its truth.
3. Observability entails objective, reproducible experiments; mere questionnaires interrogating human subjects are useless, because humans might lie.
4. If and only if an experimental implication of a theory proves false is the theory proved false.
5. Objectivity is to be treasured; subjective »observation« (introspection) is not scientific knowledge, because the objective and the subjective cannot be linked.
6. Kelvin's Dictum: »When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind« [...]
7. Introspection, metaphysical belief, aesthetics, and the like may well figure in the discovery of a hypothesis but cannot figure in its justification; justifications are timeless, and the surrounding community of science is irrelevant to their truth.
8. It is the business of methodology to demarcate scientific reasoning from non-scientific, positive from normative.
9. A scientific explanation of an event brings the event under a covering law.
10. Scientists – for instance economic scientists – ought not to have anything to say as scientists about the oughts of value, whether morality or art.

Any philosopher of science will immediately recognise that this list was not made to represent the recent level of philosophical research but is rather a reconstruction of the modernist convictions of mainstream economists. However, McCloskey attacks not only these modernist convictions but *any* kind

48 See e.g McCloskey (1994), p. 48. This leads to McCloskey's rejection of any difference between style and substance.
49 Quoted from McCloskey (1983), p. 482.
50 Quoted from McCloskey (1985), p. 7-8.

of rule-based methodology. Her arguments rely heavily on what has already been outlined in section 3.1.1, which is why a short summary shall suffice here. McCloskey draws a fundamental difference between »small m methodologies« that give practical advice for everyday's problems and »capital M Methodologies« that claim to give universal prescriptions for *the* scientific Method. For McCloskey, all »capital M Methodologies« have failed to give us certain guidelines for good scientific research and how to separate it from useless speculation. One argument she gives is the self-defeating character of such Methodologies: If they were to take their prescriptive (and often empiricist) standards seriously, they themselves would have to be the first to go.[51] McCloskey goes on to cite a great number of philosophers who are opposed to positivism in order to show that this kind of methodology is obsolete in philosophy. As far as economics goes, McCloskey shows – by referring with approval to Kuhn, Feyerabend, Duhem and others – that falsification has itself been falsified by the history of science: Neither the Keynsian revolution nor its monetarist counterpart would have been possible had the protagonists followed a modernist methodology as sketched out above.[52] McCloskey draws additional support against modernist methodologies from the »if you're so smart, why aren't you rich« paradox, from which she concludes that prediction is impossible in economics.[53] The paradox states that if prediction *were* possible at a significant level, economists would use their knowledge to make money and would not publish it for others to use.

From this »refutation« of old Methodological(!) convictions McCloskey proceeds by stating that any attempt to save the prescriptive force is deemed to fail. She attacks any kind of speaking about the Truth (with capital T) or what »ultimately« will turn out as true by means of two main arguments: First, we can never know anything about an ultimate truth; and second, we don't even need to know ultimate truth for our practical work. This is not to deny the relevance of standards for scientific work, which McCloskey underlines by citing Stanley Fish: »[T]here is no 'standard or set of standards that operates independently of the institutional circumstances… which is not the same thing as saying there is no standard'«[54]. Scientists must have a methodology, but no Methodology.[55] Putting forward hypothetical criteria or »some future tests« is nothing but covering up matters and is not helpful in any way – for McCloskey, the very idea of Truth or a unified Methodology »is a fifth wheel, inoperative except that it occasionally comes loose and hits a

51 See McCloskey (1985), p. 12.
52 See McCloskey (1985), p. 17-18.
53 See McCloskey (1983), p. 487-488.
54 See Fish (1989), p. 164 cited from McCloskey (1994), p. 243. McCloskey also says that »anything goes« does not go *in argument* and refers to Feyerabend on this. See McCloskey (1994), p. 186.
55 See e. g. McCloskey (1994), p. 184.

bystander«[56]. McCloskey compares well-meaning Methodologists to bureaucrats who try to be helpful but ultimately impose useless rules on a system which would do better without them.[57]

In spite of all these arguments against traditional methodology, McCloskey tries to stress that she is not speaking in favour of »irrationalism«, in the sense that anything whatsoever is equally valid for economics. She defends herself at length against the common reproach of »tu quoque« against relativistic thinkers, which she sarcastically sums up: »You, oh relativist, in asserting the truth of relativism must acknowledge a standard of truth. Gotcha.«[58] McCloskey dismisses this argument by the »'rhertorician's tu quoque«, arguing that philosopher's attack is a rhetorical device that presupposes a safe metalinguistic level and by this begs the question. The relativist position can become non-self-contradictory easily, if it avoids assuming truth for itself.

On the other hand, it is often maintained (against relativistic positions) that a lack of a common methodology would lead to chaos and bad science – i.e. McCloskey's attacks against Methodology are wrong for the *results* that their acceptance would lead to. McCloskey rejects this argument against her »irrationalism« as a strange, authoritarian one and thinks it »odd to hear intellectuals raising alarm against intellectual anarchy«.[59] In her opinion the only useful effect of looking for a clear demarcation between science and non-science could be the protection of scientific freedom in absolutist countries.

After this philosophical characterisation of McCloskey's work, let's finally turn to the actual course of action of the rhetorical analysis of economics: It is quite plainly the attempt to discern patterns and strategies by which economists *persuade* each other. There are several approaches that can be lumped together under the flag of »rhetorical« analysis in the way it is intended here. McCloskey started with a method that takes the term »rhetorical« quite literally: In her first publication she used tools of literary analysis on classic economic texts.[60] Her result was, as could be expected, that the use of rhetorical devices is widely spread in economics, supporting her claim that the image of rational discussion without irrational rhetorical ornaments does not happen and, additionally, is not possible.[61] More concretely, McCloskey lists the following rhetorical devices used in the classics of economics she analysed:[62]

56 McCloskey (1985), p. 47.
57 See Mäki (1995), p. 1311.
58 McCloskey (1994), p. 200.
59 See McCloskey (1985), p. 39.
60 In McCloskey (1983) she analyses only two pages from Samuelson's *Foundations* (see Samuelson (1947)) as a »proof of method«.
61 McCloskey maintains that even the acceptance of what counts as a proof in mathematics is based on the persuasion of the community and can change over time. See McCloskey (1994), p. xv, where she refers to Lakatos (1976), p. 29 footnote 1.
62 See McCloskey (1983), p. 500 et sqq.

- Demonstration of mathematical virtuosity as evidence of virtue.
- Several appeals to authority.
- Several appeals to the relaxation of assumptions.
- Several appeals to hypothetical toy economies.
- One explicit appeal to analogy and use of metaphorical language.

The usage of rhetorical devices is a clear departure from the official modernist methodology, but McCloskey argues that there are good reasons for this, because intelligent persuasion is always based on such devices. Many useful notions that are not testable had to be eliminated from economics if the prescriptions of the modernist methodology (as McCloskey depicts it) were taken seriously. Examples are Fisher's untestable equation of exchange (MV=PT) or, more generally, the notion that the economy is basically competitive. These are not empirical propositions but »simply an invitation to look at it this way«,[63] which promises to deliver highly illuminating insights. McCloskey further analyses common phrases of persuasion such as »it is natural to assume« and the metaphorical character of talking about supply-and-demand curves or the notion of »equilibrium«. But rhetorical analysis is not restricted to finding rhetorical devices in economic texts. In newer publications McCloskey counts philosophical argumentation theory and Kuhnian sociology of science as rhetorical approaches. Even if I put aside any detailed report of a specific rhetorical analysis, the point of it seems fairly clear: It illuminates the way economists persuade each other and makes explicit diverse strategies of persuasion that economists were only dimly aware of before McCloskey's work. For example, McCloskey is convinced that it was not due to its statistical tests but through »the sheer bulk of the book – the richness and intelligence of its arguments«[64] that Friedman and Schwartz convinced their readers in their book *A Monetary History of the United States, 1867 – 1960*[65]. But still, one could ask what the *value* of McCloskey's insights is.

McCloskey points out several advantages that could be gained by the study of the rhetoric of economics:[66]

- Better writing, by knowing more about rhetorics.
- Better teaching, by knowing how to express the tacitness of economic concepts.
- Better capability to explain economic concepts to non-economists.
- Better science by knowing the restrictions of economics and not recurring to the seeming objectiveness of some methods and therefore being able to discuss papers more honestly and in a less bitter way, as values

63 McCloskey (1983), p. 501.
64 McCloskey (1983), p. 498.
65 See Friedman/Schwartz (1963).
66 See McCloskey (1983), p. 512.

that where before dismissed as »unscientific« can now enter the discussion.

It is easy to see that the main point of the rhetoric of economics is a therapeutic one. After being confronted with their use of rhetorical devices, economists are supposed to be able to discuss less dogmatically and more honestly about their respective positions. The methodology of the rhetorical turn is a completely descriptive one.[67]

3.2. Criticisms of Postmodernism

At first sight, the views of postmodernist methodologists may seem persuading, but surely this is not enough to lead us to accept them. A critical discussion is needed. As in the preceding chapters, I shall start with arguments that concentrate more generally on epistemology and philosophy of science and continue with a discussion that refers directly to McCloskey and Caldwell as protagonists of postmodern economic methodology. Throughout this section I will not merely present the arguments but will evaluate their validity for the methodological questions under discussion.

3.2.1. Some Counter-positions from Philosophy

3.2.1.1. Paul Boghossian – in Defence of Knowledge

Paul Boghossian is one of the best-known enemies of postmodernist thought and has recently written a book summing up his arguments against postmodernist relativism[68] which is, after the preceding introduction to postmodernist thought, a good basis for assessing the validity of either side's positions. Primarily, Boghossian tries to show that there are inescapable inconsistencies in relativistic positions entailing the claim of the possibility of »equally valid« but contradictory views of the world.

Boghossian concedes that there can be equally valid but different ways of knowing the world as long as they do not contradict each other: An active baseball player's knowledge of the game is different from that of a sports journalist, but this causes no problem whatsoever. Boghossian employs a different example to point out a primary problem of relativism: According to the most widely accepted scientific knowledge, humans first entered America from Asia. By contrast, some Cheyenne River Sioux believe that humans

[67] See Caldwell/Coats (1984), p. 576.

[68] See Boghossian (2006). For an article that can be considered a »light version« of the book see Boghossian (2001).

in America have descended from »Buffalo people« and hence did *not* come from Asia.[69] Here we have two beliefs that cannot *both* be valid at the same time. For Boghossian this is the first hint that epistemic relativism is not a tenable position, as he takes the law of Non-Contradiction (~(P&~P)) for sacrosanct.[70]

After having set the stage by the above example, he develops more advanced attacks against relativism by arguing against constructivism as the philosophical position that often stands behind relativism, which states that there are no *objective* criteria to distinguish true knowledge from mere belief. More precisely, Boghossian argues against the full description dependence of facts the way Goodman adopts it. Recall that Goodman's constructivism is based on the idea that our worlds are composed of objects that are constructed by us via categorizing and grouping primitives, but that there are no primitives that can legitimately claim to be the only basic ones. Our reasons for categorising them as »individuals« (see section 3.1.1.2) are purely pragmatic, and the resulting worlds cannot claim to have any resemblance to the way things are in and of themselves – for there is no way to know how things are in and of themselves. The following passage presents Boghossian's main arguments against this concept in short form and evaluates their force:

> If our concepts are cutting lines into some basic wordly dough and thus imbuing it with a structure it would otherwise not possess, doesn't there have to be some worldly dough for them to work on, and mustn't the basic properties of that dough be determined independently of all of this fact-constituting activity?[71]

Admittedly this sounds like a good argument, but in fact the only thing it challenges is pure idealism. Goodman and the constructivists can easily escape such a critique by admitting the existence of »something« without specifying it any further. So this argument of Boghossian does not destroy the main constructivist conviction that what we talk of as facts are highly constructed items.

Boghossian's next argument in his attack against constructivism refers to the problem of backward causation.[72] While we may easily talk about objects such as electrons, giraffes and mountains, the existence of these objects antedates our own. Boghossian argues that we cannot meaningfully be said to construct our own past, i. e. giraffes cannot be objects constructed by us. The problem of this attack (and of Boghossian's approach in general) is the presumption of some facts that a constructivist or relativist would deny. Such a

69 See Boghossian (2006), p. 1.
70 See Boghossian (2006), p. 40.
71 Boghossian (2006), p. 35.
72 See Boghossian (2006), p. 38.

twist allows for criticism that seems quite striking but in reality attacks a straw man. Boghossian seems to assume that constructivists would claim that our talking about the genesis of giraffes makes them appear in a literal sense in a time before we could talk. This is surely absurd, but it does not refute constructivism, because constructivism does not entail such a view. Constructivism merely states that our knowledge, our facts and *our perception* of the past are all dependent on ourselves; there are no »common-sense« facts, nor are there given facts about the past. I know of no version of social constructivism that entails the view that we could change »the real past« by backward causation if we were to construct the world in a different way. The past we have depends on us, but there is no backward causation of brute facts involved.

The following argument by Boghossian contains a similar error: he states that it is an intrinsic aspect of many concepts (such as mountains, electrons and giraffes) that they were *not* constructed by us.[73] Clearly mountains are not supposed to be built by humans. But as far as I know, no one claims that. It is not self-contradictory for constructivism that there exist concepts that were *not consciously* constructed. If that were the case, the whole project of constructivism must have seemed absurd from the beginning, as many of our concepts are generally thought to refer to things independent from ourselves. When talking about *epistemic* questions I find it rather natural to go beyond the standard idea of facts that are out there and we perceive them as they are. Epistemology is about the way we come to know the world, and I fail to see the advantage of a theory stating that our worldview roughly – but somehow justly – corresponds to the real world.

Getting back to the earlier example of the two opposing views concerning the origins of Native Americans, Boghossian attacks the relativist view that both could be true at the same time, as there are no absolute (sometimes called »global«) facts of the form »p«, but only relativistic (sometimes called »local«) ones of the form »according to a theory T (or worldview W), we accept p«. Boghossian rejects the traditional anti-relativistic argument that calls relativism self-refuting, by stating that if relativism were true, it could not consistently claim to be more than a report about what relativists find agreeable to say, because true relativism must be applied to itself.[74] Instead, he puts forward a slightly different argument and tries to show relativism to end up in an infinite regress. Here is the essence of his argument: If relativism allows only local truth, it follows that for every local fact there is a never-ending chain of »according to a theory we accept, there is a theory we accept and according to this latter theory «p« is true.«[75] Hence, there is no absolute justification for relativism. But do relativists need to claim that it is an absolute fact that there are no absolute facts? The way I interpret the postmodernists

[73] See Boghossian (2006), p. 39.
[74] See Boghossian (2006), p. 52-54.
[75] See Boghossian (2006), p. 56.

is different: They hold that there are no facts, only opinions or worldviews. But if that is nothing more than an opinion (and not a fact!) as well, we easily return to a coherent position. The »traditional argument« against relativism can be used by relativists as a defence.[76]

Upshot:

Of course my responses to Boghossian's arguments do not show that constructivism is correct. Rather, they show that constructivism is hard to refute. Boghossian's attempt of refutation seems futile, as the weaknesses of the opposite view – namely epistemic realism and versions of the correspondence theory of truth – are banned into the footnotes and said to be of low importance for the argument.[77] The main problem with Boghossian's work is his attempt to establish a fundamental difference between truth and beliefs about truth without being able to offer a viable criterion for truth. Boghossian's failure to refute constructivism/relativism suggests that a reconciliation between the two frontiers is highly unlikely and that a shift of focus would probably be the best thing to deal with the controversy.[78]

It is not the purpose of my work to deliver a complete review of Boghossian's arguments, which is why the given examples shall suffice here.[79] In order to gain a balanced view on the criticisms of postmodernism, it seems more promising to have a look at other opponents.

3.2.1.2. Daniel Dennett – Scienticism

Daniel Dennett is another well-known enemy of postmodernist thought, even if he did not publish his major works on that subject. In two (rather polemic) publications, Dennett explicitly attacks postmodernism and tries to defend »the scientific method« against it.[80] Daniel Dennett argues that postmodern science theory (e. g. McCloskey's rhetorical analysis of economics) is wrong in assigning equal validity to scientific discoveries and any other kind of »narrative« and moreover that it is even harmful to science.[81] Dennett locates the danger of postmodern multiculturalism in equating the scientific method with Western imperialism and thus seeing science as oppressive and neglecting the liberating effects it can bring along. For Dennett, if scientists are to be held responsible for what they say, postmodernism is utterly use-

[76] See McCloskey (1994), p. 201.
[77] See e. g. Boghossian (2006), p. 128, footnote 17.
[78] I shall propose such a shift at the end of this section.
[79] Even if my short refutations seem to suggest otherwise, I highly appreciate Boghossian's work for the clearness of his arguments. His precise and simple style gives an excellent example of good, honest academic work that helps in clearing up many misunderstandings, but on the other hand (and sadly for Boghossian) makes it easy to detect his own errors or unsupported presuppositions.
[80] See Dennett (1998) and Dennett (2001).
[81] See Dennett (1998).

less because of its denial of any fundamental standards – standards in which Dennett firmly believes:

> […] this attitude passes as a sophisticated appreciation of the futility of proof andthe relativity of all knowledge claims. In fact this opinion, far from being sophisti cated, is the height of sheltered naiveté, made possible only by flatfooted ignoranceof the proven methods of scientific truth-seeking and their power. Like many another naif, these thinkers, reflecting on the manifest inability of their methods oftruth-seeking to achieve stable and valuable results, innocently generalize fromtheir own cases and conclude that nobody else knows how to discover the trutheither.[82]

But there is more to Dennett's arguments than just contradiction. He states that the postmodernist attack on »Truth« (global truth) has no bearing at all on whether *specific statements* are true or false (local truth). In science it makes perfect sense to ask whether a theory (an observation, etc.) is true or not. Dennett calls this a »vegetarian concept of truth« (in accordance with Rorty), but he denies that acknowledging this step can be used for a relativisation of the »vegetarian truths«. His claim is supported by an evolutionary argument: All the life forms we know of are, consciously or not, heading towards truth, because it is useful for their survival. Even the human capacity of doubt and reflection is helpful for this aim. Culture, science and communication are, according to Dennett, all to be seen as tools that help us in coming closer to truth and increasing our fitness. The whole point of life and its development is truth, and »we have created a technology of truth: science.«[83] Science is the most advanced way of finding truth and avoiding error, and controversial as it may often be, its results are, after some years of fighting, the most uncontroversial facts we have.

Dennett concedes that science is not neutral but driven by our values and interests – and he insists that it should be. We cannot concentrate on everything (or on everything in the same way), but this does not mean that the results of our interest and value-laden fields of research are less secure. Dennett is using the classic argument of the separation between the context of discovery and the context of justification here.[84]

Dennett concludes that science has a privileged status to other ways of knowledge and claims that this is strongly supported by the weight we place

82 See Dennett (1998).
83 See Dennett (1998).
84 He admits that there can be epistemic settings where lacking neutrality can lead to bad science, e. g. when medicaments tested on men only. He admits, too, that science can be misused and imposed with bad effects on societies that were not accustomed to the blessings of science before. But these are flaws of application, not of science itself, he states. See Dennett (1998).

on scientific arguments and the track record scientific reasoning has. Even if science is not perfect, it is the best knowledge we have, and it is getting better every day.

All in all, Dennett is defending science and its truths from a scientific point of view. If one cares about scientific truth, science is the best way to achieve it – and if, e.g., religious leaders accept scientific truths that contradict their own worldview, they are weakening their positions. They would have the option of denying the possibility of gaining truth by observation and rational thought. They could argue that to love and praise) god is the only valuable thing to strive for, and scientific truth does not matter. By taking such a completely incommensurable position, they would be able to protect themselves against scientific criticism. Of course, people have the right to decide on their own which side they find more convincing; the majority in Western countries now seem to be in favour of scientific and not of religious truth.[85]

In a more recent paper[86] Dennett adds some arguments to the viability of comparing science with religion. Dennett responds to the above counter-argument – which states that truth is to science what god is to religion – by asserting that faith in the truth unifies all believers. »Truth-telling is, and must be, the background of all genuine communication, including lying. After all, deception only works when the would-be deceiver has a reputation for telling the truth.«[87] Later on Dennett concedes that an inflated concept of Truth is not defensible and agrees that local truth can suffice. This sounds like an attempt to reconcile postmodernism and modernism, but some sentences later Dennett suggests that scientific truth is the *only* acceptable local truth, because all representatives of competing truth-systems eagerly refer to scientific support if there is any. He neglects that this could as well be explained by the fact that science is the *ruling* truth-system in our world.

After admitting that truth can hurt (which none of the postmodernists would deny) Dennett asks why science and its technology are so persuasive to cultures that are not yet accustomed to it. If science was spread by Western imperialist policy and was just another way of seeing the world, it would hardly be such a success. There is much to this argument, but note that it contains an ad populum fallacy: Essentially, it states that if so many people believe in science, it must be true.

85 Many don't seem to see an inconsistency in adopting both. This short example cannot depict the complexities of the debate but is merely meant to illustrate the possibility of two radically contradicting worldviews and does not deny that there are possible overlaps between the two.

86 See Dennett (2001).

87 See Dennett (2001). As the rest of this section is referring to this text and as my source is a website which does not allow me to point to pages in a reproducible way, I shall skip further references to this text.

At the end of his essay Dennett concentrates more on arguments made against protecting other cultures from the bad effects of the »blessings »of science, but that discussion can be separated completely from the question of whether or not scientific truth is the only acceptable local truth. This question is rather a political one, and the answer seems clear: Of course people should be well informed and able to decide freely if they *want* the blessings of science. But the fact that science is successful in convincing people does not settle the epistemological debate between postmodernists and their enemies about Truth. Note that neither Dennett nor Boghossian are taking an anti-realistic position – on the contrary, they argue that true knowledge is possible and necessary. In section 4 I will discuss positions which deny this but still do not adopt the relativism of postmodern positions.

Upshot:
While Dennett's arguments are surely not persuasive to postmodernist ears, they point to a fruitful shift of the question, as they lead away from the unproductive discussion about the possibility of global Truth and concentrate more on the desirability of effects that relativistic positions may have. The next section accepts this shift of focus as it reviews a different attack on postmodernism, which does not focus on the question about T/truth but instead refers to the *effects* of postmodernism on scientific standards.

3.2.1.3. The Sokal Affair – Revealing the Lack of Standards in Postmodernism?
When it comes to discussing the critics of postmodernist thought, Alan Sokals famous hoax experiment cannot be excluded from the discussion. In 1996 Sokal managed to publish a nonsense article filled with postmodernist catchwords titled »Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity« in the leftist peer-reviewed academic journal *Social Text*.[88] This of course shed bad light not only on the editors of *Social Text* but on the existence or effectiveness of academic standards in postmodernist communities, and was intensely debated for quite some time.[89]

The details of the quarrels concerning Sokal's position are of minor importance here; more interesting is the question of how Sokal's successful hoax affects the question of the relativity of Truth. Alas, despite the paper's significant impact, it does not seem to add much to the debate between postmodernists and »modernists« – at least not after already discussing more recent arguments from Boghossian or Dennett (which themselves drew much inspiration from the Sokal debate). Similar to Boghossian and Dennett, Sokal

[88] See Sokal (1996).
[89] An extensive list of contributions to the debate can be found at Sokal's website. See Sokal (2007).

affirms the notion that science is in principle capable of finding external, objective truth, which is exactly what his enemies deny.[90]

The more interesting aspect of the Sokal affair is surely the questions it raises about academic standards. The whole point of submitting the article to *Social Text* was to show that postmodernists didn't have any effective standards for distinguishing between nonsense and a genuine contribution to their field. This is a different kind of criticism, as it does not rely directly on the existence of objective global criteria but on the reliablity of local ones. By this it can hurt postmodernists like McCloskey who hold the view that local standards can be perfectly applicable even while denying the existence of global ones, because the hoax showed that *Social Text* did not seem to have any viable local standards. However, even if the acceptance of the hoax text is an embarrassment to the editors of *Social Text* and the postmodernist community, more systematic research is needed if one aims at showing a robust connection between the rejection of global standards and the loosening up of local ones.[91] And even if such a correlation were found, this would not be an argument in favour of an epistemology defending the notion of objective global Truth but rather a completely pragmatic one: While a postmodern attitude may be harmful to scientific standards, this does not show it is *wrong*.

The Sokal affair illustrates again that the fight between modernists and postmodernists is not likely to be settled soon – if ever. It also shows that there are more fruitful and interesting questions to pursue, e. g. questions about the appropriateness of standards for solving a specific task, which can be tackled by modernists and postmodernists in the same way. Before we come to those questions in section 4, let's finally have a critical look at the postmodernist *economic* methodologists presented above.

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90 At that point of the debate misunderstandings seemed to soar due to the linkage of the debate to political issues. The postmodernists whom Sokal attacked were far more radical than e. g. McCloskey and sometimes concluded from their denial of the existence of objective truths to the inappropriateness of the scientific project in general. See e. g. Chadha (1997). Such radical positions may be the reason why the modernists feel the need for defending science and its methods on such a broad scope. See Sokal (2000).

91 There are even commentators who argue that Sokal's hoax article is actually a good piece of science and if one takes a postmodern perspective such a position seems indeed defensible. The article may be full of (intentionally placed) errors concerning physical theory, but its *philosophical* argument of relating anti-foundationalist epistemology to new developments in quantum physics is not completely absurd. See Horgan (1996). If you take Sokal's hoax as a decisive attack against postmodern philosophy, consider the possibility of placing some philosophical nonsense referring to the newest (and correctly reported) trend in physics in a physical journal without the editors noticing the *philosophical* shortcomings of the text. That does not seem impossible, either, does it? And surely this would not be interpreted as being harmful to modern physics.

3.2.2. Criticisms of Postmodern Economic Methodology

I will start with some criticisms that are directed against Bruce Caldwell's conception of pluralism. The next section will deal with attacks against McCloskey's rhetorical analysis of economics.

3.2.2.1. Problems with Pluralism in Economics

It makes little sense to discuss monism here as a critique of pluralism, as that would probably not lead to much more than the presentation of two opposing and irreconcilable views. Where monists believe that there *can* be one method underlying all scientific reasoning, pluralists are denying exactly this claim. In his review of Caldwell's *Beyond Positivism,* Abrahm Hirsch hints at more specific critical points, the first one being an inconsistency of Caldwell's position – which shows up in the title itself. »Beyond Positivism« is inconsistent with Pluralism, as it implies that positivism can be overcome somehow, whereas »a true pluralist would not say that we are beyond anything«[92]. Caldwell replies to that point by saying that pluralism can consistently claim to be beyond positivism, but not in the sense that positivism itself has to abolished as wrong method; however, its strongly prescriptive claims *can* be criticised by a pluralist as pluralism, as opposed to prescribing single methods.[93] I think Caldwell can escape quite easily from the charge of inconsistency, but when Hirsch asks for more prescriptive content and bemoans Caldwell's concentrating only on the critical part, Caldwell as a pluralist cannot really reply but merely expresses his view that the lack of prescription is actually a strength of pluralism. A last point Hirsch makes against pluralism regards its justification: Pluralism is justified by the fact that philosophy of science has, up until now, failed to find a single universal methodology.[94] But this fact does not justify pluralism. The fact that something has not yet been discovered cannot justify giving up the quest. But in the case of methodology and the task of finding a unified method, I think there are strong indicators that the quest is not likely to come to an end soon – or ever. This may not justify pluralism as a settled position, but it gains plausibility as an interim-position, which is exactly how Caldwell defines it.[95]

Interestingly, whereas Hirsch criticises Caldwell for being exclusively restricted to internal criticism, Lawrence Boland points out that Caldwell's acceptance of external criticism is a flaw in his conception.[96] Indeed, Caldwell does not completely abandon external criticism. Boland reproaches Caldwell for taking philosophers' views as external standards when appraising eco-

[92] See Hirsch (1985), p. 178.
[93] See Caldwell (1985), p. 192-193.
[94] See Hirsch (1985), p. 184.
[95] See Caldwell (1984), p. 243.
[96] See Boland (1997), p. 149 et sqq.

nomic theories.[97] There is surely some point to this argument, but it is not an argument against pluralism in general, only to the specific way in which Caldwell chooses to defend it. Refuting Pluralism as such is nearly impossible, as this comes down to proving the correctness of a single methodology. The biggest drawback of pluralism is its lack of power for providing *any* methodological advice.

McCloskey's rhetorical analyses raised many more discussions than did Caldwell's pluralism, and due to her provocative style and clear positioning as a postmodernist methodologist she is even better suited to cast a critical eye on postmodernism in economic methodology.

3.2.2.2. Problems with the Rhetorical Turn in Economic Methodology

As noted above, there are many criticisms of McCloskey's approach. I will discuss some of them, but as in the section about criticisms of pluralism I will skip the most basic attack, which is the attempt to defend a foundationalist view of objective science. This objection has already been dealt with in some detail in section 3.2.1; additionally, a discussion of this criticism is not likely to be fruitful.[98]

The first point I will discuss is, however, similarly fundamental. It addresses McCloskey's claim that all (capital M)-Methodology and any kind of prescription is a fifth wheel and unjustifiable. Two concerns are raised against this assertion: Firstly (and understandably), many methodologists are not at all pleased by being told they are useless or should stick to pure descriptive analysis. Even if they accept that positivism (or modernism) has failed, they want to retain *some* prescriptive part in their works and feel that they have found ways to justify it.[99] In their view, an exclusively descriptive methodology would cease to be methodology proper and would rather be history of science. And indeed, McCloskey herself is not sticking to her alleged credo of the abandonment of prescriptions. Her books and papers are full of recom-

97 I will come to Boland's position in more detail in section 5.2, as he proposes a methodology that aims at more than being purely descriptive but is highly critical of prescriptive methodologies nonetheless.

98 I also do not know of any attempt to criticise McCloskey along these lines. Since Popper's attack against the logical positivists this road has been seemingly blocked in philosophy of science. Note that many of the arguments given below can be used against any form of social constructivism or normatively turned Kuhnian strand of philosophy. For the sake of clarity and precision I stick to McCloskey as the argumentative enemy here.

99 See Caldwell (1982), p. 217. Caldwell's pluralism is not prescriptive, as it is a meta-methodological position, but it encourages prescription and criticism on a local level.

mendations on how economics has to change[100] and why it is on the »wrong track«[101].

Understandably, this has led to many attacks charging her with inconsistency.[102] How can McCloskey attack prescriptive Methodology on the one hand and give prescriptions on the other? I think McCloskey can be defended from these charges; she is only attacking capital M-Methodology, which claims to offer a single prescriptive method valid for all problems and theories.[103] On a local level, criticism and prescription is welcome even for McCloskey. Stanley Fish gives an illustrative example when saying that there is no universal theory of baseball, but surely »useful practical advice how to throw strikes and keep them off the bases«.[104] As a tribute to her provocative style, McCloskey's formulations are sometimes unclear and thus lead to charges of inconsistency. Concerning prescription, she could have avoided much confusion if she had explained more openly her evaluative standards (may they be taken from literary criticism or anywhere else). It should be clear that given her rejection of global standards, these local standards must finally rest in her subjective value judgements. All McCloskey can do is to persuade economists of the aptness of *her* own standards, but that would be perfectly consistent with her writings.[105]

Other accusations of inconsistency include McCloskey's claims about *Sprachethik*. She argues consistently that honest and *herrschaftsfrei* rhetoric would lead to an improvement of economics. On the other hand, her writing is full of personal attacks and ridiculing of critics.[106] There is surely much to this point, but keep in mind that it rests on a tu quoque fallacy: Even if McCloskey does not live up to her own standards, that does not mean per se that her proposals are wrong. But still, such findings weaken her position and lower the persuasive force of her work. In a similar vein, it is hard to determine whether McCloskey is in general affirmative or critical of the state of economics.[107] Trying to salvage her position, one could argue that she thinks

100 She claims, for example, that the neoclassicals should become »more feminine«. See McCloskey (1993), p. 76.
101 See e. g. McCloskey (2006).
102 See e. g. Caldwell/Coats (1984) and Mäki (1995).
103 Falsificationism is often said to be a methodology of this kind, but I think it is wrong to attribute this view to Popper personally, as he noted the problem of justifying a prescription as early as in the first edition of *The Logic of Scientific Discovery* and speaks of his methodological rules as conventions. Admittedly, Popper intended these conventions to *define* the scientific game. In later works, however, Popper developed his concept of critical rationalism, which is highly critical of imposing any rules. See Caldwell (1991) for a nice comparison of the two Poppers.
104 See Fish (2003), p. 414.
105 That is at last how I read her own defence. SeeMcCloskey (1994), p. 199.
106 See Backhouse (1995), p. 301.
107 See Mäki (1995), part 7.

that many achievements of economics would be acceptable, but she holds that the majority of economists are methodologically uninformed and stick to an unjustifiable version of modernism. But again, due to her provocative style her work is highly ambiguous, which makes it hard to tell what exactly McCloskey wants to say. I can't help but consider this to be a downside of McCloskeys work[108]. Sometimes it seems that she is not even *trying* to be clear about the status of a claim. In this sense, at least, the »received view« that McCloskey fights against was certainly superior, more honest and hence easier to attack as well.

Yet another criticism does not concentrate on alleged inconsistencies but rather accuses McCloskey of attacking a straw man in her plea against »modernism«.[109] Indeed, it is doubtful if any economists would subscribe to the list that McCloskey created in order to define modernism. Not only do economists not follow these rules, they don't even »officially subscribe« to them. Working economists know the muddles of practice too well to be able to accept phrases like »Observability entails objective, reproducible experiments;« as their official methodology. Similarly, McCloskey's attack against »Methodology« completely ignores the post-positivist literature. Taking her writing literally (and that should be acceptable for any scientific writer) McCloskey confuses capital M-Methodology with logical positivism. This explains the fury that can be found in her attacks, but her criticism seems to be kicking a dead horse rather than being innovative or provocative. This shows up best in her remarks that economists are using untestable assumptions. For a logical positivist of the 1920s this might be problematic, but it certainly is not for any philosopher of science of our time.

Finally, McCloskey has convinced many that the rhetorical analysis of economics is an interesting and even entertaining approach. Nevertheless, methodologists and economists alike have mostly *not* been convinced to adopt her approach. Even if one accepts her findings of rhetorical devices in economic papers, one can still ask the simple but damaging question, »So what?« Of course, McCloskey is at pains to give answers to this question; some of them have been listed in section 3.1.2.2. But it seems that her arguments have not been persuasive, i. e. they are unsuccessful by her own standards. Why should we accept the standards of literary criticism when discussing economic models? Why should economists change their style of writing the way McCloskey wants it?[110] But before pushing too hard in criticising McCloskey, let's keep in mind that this is a problem virtually any methodologist has to face. Practising economists do not like being told what they should do, and other methodologists prefer to criticise the views of their colleagues in-

[108] This is a downside which may go hand in hand with the pleasure of reading her works.
[109] See Caldwell (comment on McCloskey), p. 576
[110] See Backhouse, p. 299.

stead of adopting them.[111] So the fact that rhetorical analysis has not reached practising economists and that other methodologists have found points to criticise is no good reason to reject it across-the-board. However, this does suggest that it is not very helpful for »improving the quality of the discussion of economic models« which is a primary goal of economic methodology. So far, McCloskey has not been persuasive in promoting her own standard of persuasiveness.

3.3. Postmodern Methodology – A Summation

It is time to step back and have a broader look at the extent to which postmodernism changed economic methodology.

It should be clear now that the more radical authors of postmodernism did not aim to design an overarching philosophical system as Quine still did with his holistic empiricism; instead, they claimed that scattered local knowledge is all we should aim for. The careless and often ambiguous style of some postmodernist authors offers much room for justified attack, but the general postmodern claim that there is no firm, single foundation for our beliefs is hard to reject after the death of positivism. In that sense postmodernism has won the day, with the exception of those postmodernists who try to defend the relativity of *facts* as something that is *more* than their personal belief, i. e. an objective fact itself.

As we saw in chapter 2.3, empiricist methodologies face several difficulties when applied to economics. Methodological pluralism takes the postmodernist movement seriously and proposes to allow for various different standards when discussing different theories. Postmodern works surely have an advantage over the very general writings of philosophy of science, since the postmodernists tend to deal very carefully with the peculiarities of the models and theories under consideration and hence do not adopt a »Mickey-Mouse view« of science, as their modernist predecessors were often accused of doing. But »pluralism« cannot say anything meaningful about how to evaluate economic models, and the standards proposed by the advocates of McCloskey's rhetorical approach do not seem to be convincing in many respects. This is why the major strands of postmodern economic methodology are not of much use for my project of theory evaluation. It seems that the debate about Truth and relativism led away from improving the discussion of economic theories and was rather detached from the questions economists had.

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111 It should be noted once again that the rhetoric approach has been a great success in economic methodology and McCloskey is far from being the only one who subscribes to it. She is only the most prominent one.

4. TRYING TO OVERCOME THE DICHOTOMY OF EMPIRICISM AND POSTMODERNISM: ESTABLISHING A PRAGMATIC VIEW ON ECONOMIC METHODOLOGY

Now we are in a position to make a fresh start and ask the central question of theory appraisal for economic theories: How can we assess economic theorising? In this chapter, I will discuss several approaches that escape the relativistic trend of postmodernism without ignoring or trying to refute the arguments that I called irrefutable in the foregoing chapter, i. e. without searching for a firm fundament as the basis for prescriptions.

The aim of this chapter is to arrive at a methodological position that retains evaluative standards without falling back on the dogmas of empiricism and falsificationism. The main constructive stance opposing radical relativism will be the diverging *usefulness* of different methods for contributing to the solution of specific problems.[1]

Even when acknowledging that problems themselves may differ throughout history or among cultures, the approach of taking the contribution of a theory or model for solving a specific problem as the evaluative basis for appraising its usefulness in a specific context looks promising if we are aiming to escape the dichotomy of dogmatically empiristic positions and relativistic views, as the notoriously confusing notion of truth is no longer in focus.[2] This restricts the sense in which I will use the term »pragmatic«: Evaluative standards are constrained by the problem under consideration. More specifically: A pragmatic approach to theory evaluation is antifoundationalist, problem-oriented and does not presume knowledge of truth nor any other universal fundament.[3] From this follows that the quality of theories shows up in whether or not they are actually *used* (be it in science or in other applications) in the process of solving a problem.[4]

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1 Note that I consider »problem« to be a very wide concept that is not restricted to »practical problems« but can include any sort of theoretical questions as well. If a problem is as abstract as the question, »How do we discover truth?« a problem-oriented methodology must suspend judgement for competing theories, because there is no way to answer the question of which theory is better in solving this problem. For a detailed discussion see section 4.2.

2 Even if problem-orientation is central to Popper's work, the second chapter showed that either he or his followers did not take it really seriously.

3 I do not adopt a pragmatic theory of truth; rather, I avoid the notoriously misleading term »truth« altogether.

4 Note the similarity to Richard Rorty's pragmatism, see e. g. Rorty (1994), p. 61.

4.1 Finding a New Balance

In the following section I will elaborate on some methodological concepts that circumvent the problems of empiricist positions but neither fall back on relativism nor take external standards as their evaluative basis. Critically discussing each approach will finally lead to a methodological framework that is well suited for theory evaluation in economics.

4.1.1. An Apt View of an Inexact Science

When discussing theory appraisal in economics it is most important to understand what economists actually do and what the status of their theories and models is. Daniel Hausman was among the first to stress focusing on actual scientific practice in the philosophy of economics while still holding a normative stance. Opposing falsificationist criticisms (especially of Terence Hutchison[5]), he actually defends economics against claims that it is empirically empty due to the heavy usage of unspecified ceteris paribus clauses. In an introductory remark, Hausman ironically refutes the conviction that unspecified ceteris paribus clauses inevitably lead to complete empirical emptiness: »it is certainly not the case that, ceteris paribus, we are all immortal or that ravens are pink«.[6] Contrary to Hutchison and his empiricist followers, Hausman sees the vast usage of ceteris paribus clauses in economics as justifiable and unavoidable. Relying heavily on the philosophy of John Stuart Mill, Hausman reconstructs economics as an inexact and separate science that legitimately uses an a priori method, which he characterises as follows:[7]

1 *»Formulate* credible *(ceteris paribus)* and pragmatically convenient generalizations concerning the operation of relevant causal factors.
2. *Deduce* from these generalizations and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.
3. Test the predictions.
4. If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then *compare* alternative accounts of the failure on the basis of explanatory success, empirical progress, and pragmatic usefulness.«

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5 See section 2.2.1.
6 Hausman (1992b), p. 41.
7 Hausman (1992a), p. 222.

While at first sight this characterisation may strikingly resemble a falsificationist picture, there are important differences: In the first step, Hausman underlines the importance of the prima facie credibility of basic generalisations and does not ask economists to make »bold conjectures« and take high risks in their predictions, as a falsificationist would do. The last step diverges from the Popperian falsificationist account in the regard that it does not speak of falsification at all, even if the predictions are not confirmed.

These points need some additional elaboration: For Hausman, economics is an inexact science. After rejecting several possible sources of inexactness, such as the probabilistic nature of economic laws or laws being only rules of thumb, he settles on a view that locates the source of inexactness in the implicit qualification of economic laws.[8] The laws by themselves are exact, but the complex and uncontrollable nature of the economic domain makes stable circumstances impossible, which rules out controlled testing of the basic laws.

An example may clarify this view: The law of demand states that consumers buy more of a good when its price decreases. This law includes an implicit ceteris paribus clause, and this is the reason why the law does not always hold. For example, in a recession the demand for catering-services may drop, even if the prices fall. Several other disturbances can be easily imagined: A scandal at the company, a decrease in quality or the emergence of a substitute product. So in many cases the demand for a product may fall even if its price is decreased.

Therefore, the status of economic laws cannot be justified by frequent empirical confirmation. But how can these laws reasonably be said to be causally working when we do not see the results they suggest on a regular basis? To take the easiest example of consumer demand: Why do we have reason to believe in the law of demand even if we frequently (but not usually!) see growing demand after *rising* prices? Again referring to John Stuart Mill, Hausman lists three conditions for a sentence S to count as a law even if it contains implicit qualifications:[9]

1. S is lawlike. (Hausman does not go into details here, but it seems safe to assume that lawlikeness includes the formal need for an universally quantified conditional and must semantically fulfil the criterion of making counterfactuals credibly true.[10])

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8 See Hausman (1992b), p. 41 et sqq. or Hausman (1992a), p. 132 et sqq.

9 See Hausman (1992b), p. 42.

10 Take the example of the law of demand again: It is a universally quantified conditional (for all products holds: if their price drops, then c.p. demand increases) and it makes credibly true the counterfactual »if the price of this products falls, demand will increase.« Not all universally quantified conditionals satisfy the second condition, particularly those that are only accidental generalisations and

2. When one removes the vague qualifications of S, S is in some »natural« class of cases often confirmed and seldom disconfirmed. »Natural« refers here to a range of cases that reflect the diversity found in the world and means to exclude biased testing.
3. Scientists have some knowledge of the interfering factors that violate the ceteris paribus condition in S and can refine the law by specifying this knowledge in a non-ad hoc way.

In addition to this classificatory list, Hausman provides more arguments for why many economics laws[11] may be acceptable as scientific laws, even if they are in reality often contradicted. The main point is that these contradictions can be considered excusable because the ceteris paribus clauses are the weak link in the whole amalgam that is set to test. If the researchers have some knowledge about the interfering factors, they can give legitimate excuses why a prediction has not come true. Finally, a law should be refinable, in the sense that this knowledge about interfering factors can be incorporated in order to make the law more reliable in the future.[12]

As shown in section 2.3.1.4, a disconfirmation never shows the source of error. A falsificationist would not allow assigning degrees of faith in certain premises, but Hausman is convinced that economists are justified in believing that general economic assumptions such as utility maximisation (in its broadest sense) are less likely to be the source of error than the implicit ceteris paribus assumption.[13]

This leads directly to the a priori method Hausman suggests: The set of premises is externally justified, be it by introspection, induction or just by the fact that it allows for explanations that employ a very familiar kind of folk-psychological reasoning. The following calculations gain their credibility primarily from these premises, and *not* mainly from their predictive success. This may lead to modelling behaviour that appears to be very dogmatic. It would be dogmatic indeed if the basic premises were treated as sacrosanct against all new evidence.[14] As said, there are good a priori reasons to believe in many economic laws, be it the law of demand, the Gossen laws, or utility maximisation. Hausman writes: »Without qualification and a margin of

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hence are not real laws. See e.g. Goodman (1955), p. 75ff. for a congenial discussion.

11 Apart from the often quoted law of falling demand famous economic laws include the law of diminishing returns, the Gossen laws or the law of supply.

12 See Hausman (1992a), p. 141.

13 See Hausman (1992a), p. 207.

14 If that was the case it would need at last explanation, how economics managed to exist for over 200 years, if one does not assume that it is based successfully on fraud.

error, they are false, but, with these, they seem true.«[15] Additionally, economic laws possess several pragmatic virtues, as they make economic theory tractable, consistent, determinate and formally complete. Some of these laws have about their only justification in those pragmatic virtues, but their acceptance can be justified nonetheless: If economics were based on more »realistic« premises, it would lose much of the simplicity or normative force it has.[16] Empirical adequacy is not the only aim of science, and this can provide reasons for economists to stick to their basic laws.[17]

Hausman's reconstruction of economics as an inexact science that legitimately employs an a priori method helps to defend much of what economists are doing from empiricist attacks, but Hausman does not argue that everything economists do is scientifically legitimate. He discusses in detail cases in which sticking to standard economic theory was reasonable and others in which this led to ignorantly rejecting evidence (using the example of preference reversals).[18] For Hausman, economists have reasons to try to preserve standard economic theory, and they can defend the attempt to formulate unified and complete models against the piecemeal engineering of more psychologically grounded theories. By this, economics becomes a separate science with a distinctive method. However, economists are surely on the wrong track if they turn the defence of their method into an attack of others that have more direct links to empirical investigations.

The idea that empirical counterevidence is not sufficient for theory change is at least as old as Thomas Kuhn's structure of scientific revolutions. As a result of such »dogmatic« but justifiable behaviour, economics becomes a separate science. Its basic premises may rest on a kind of folk psychology, but economics is best understood as clearly separated from psychology, as it does not care about actual individual motivations. Explanations for many macro-phenomena can be given in the abstract language of economics and would not gain much from a deeper psychological analysis. Again referring to Mill, Hausman postulates the causal factors of economics to be separate from other sciences: »One can separate the subject matter of political economy from other social phenomena as if the desire for wealth were virtually

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15 See Hausman (1992a), p. 210.

16 Note again that if economics is seen as a normative theory of rational behaviour, it cannot be based on empirically observed behaviour, because – as known since the days of Hume – an »ought« cannot be derived from an »is«.

17 Another, more indirect defence of much of the abstract reasoning economists perform can be given by Hausman's view of models. For Hausman, a model in itself is neither true nor false but merely a kind of predicate that defines a system. By that definition, Hausman allows for much unapplied conceptual exploration as is common in economics. Of course, ultimately models must somehow fit to empirical data. See Hausman (1992b), p. 25 et sqq.

18 See Hausman (1992a), Chapter 12.

the only relevant causal factor«[19]. Even if exact prediction will never be possible, there are separate causal factors at work, which are only visible in tendencies of the economic system. The fact that there are interfering factors that often counteract classic economic forces does not deprive these forces of their causal status:[20] Hausman takes consumer demand as an example to show how there can be knowledge about the workings of several factors such as price, the price of substitutes, income and tastes and still exist no way to make precise predictions. Here, a change in tastes would be an interfering factor that makes prediction impossible, but still the classic economic factors can be meaningfully said to have a causal status.

The isolation of factors makes economics unrealistic, but still the economic factors describe an aspect of reality. In contrast to Albert, Hausman sees the vagueness in economics as justified, but similar to Albert he pleads for more empirical work: Even if economists can excuse the inexactness of their theories, excuses are not very helpful in the long run. Therefore, after justifying the general method of neoclassical economics against unjust attacks, Hausman criticises some orthodox economic research for its dogmatism and lack of empirical content. He states that while economists have reason to strive for a unified theory, they overemphasise this aspect. And while sticking to »economic« factors may be legitimate, an integration of other, more psychological factors would be helpful in many cases. Hausman takes Akerlof and Dickens' »The Economic Consequences of Cognitive Dissonance«[21] as an example.[22] The main argument of this paper is that workers in risky jobs can improve their satisfaction with their jobs by underestimating the risks. The inclusion of non-pecuniary costs is a psychological factor that can be perfectly integrated in economic rational-choice models.

Upshot:

After having delivered many arguments justifying the practice of neoclassical economics, Hausman claims that economists should be more open to integrating other than pecuniary factors into their models and should shift priority from unifying theory to learning from empirical data, as difficult as that may seem.

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19 Hausman (1992a), p. 145. Note that Hausman is referring to a narrow interpretation of the homo oeconomicus here that is only affected by the »desire for wealth«.

20 For an excellent elaboration on the concept of causation see Mackie (1974), p. 59 et sqq. The general idea is in short that »causes are insufficient but necessary components in sets of factors that are unnecessary but sufficient for the effect to occur« (Hausman (1992a), p. 295.). Of course, this does not solve the ontological question of whether causality exists as a form of power or is nothing more than an unexceptional succession of events in time that cannot be explained any further. For a discussion of this point see Esfeld (2007).

21 See Akerlof/Dickens (1982).

22 See Hausman (1992a), p. 258 et sqq.

4.1.1.1. Critical Discussion

Hausman surely depicts a convincing view of economics and its underlying methodology that has the advantage of being descriptively accurate without accusing economists of irrationality or gross dogmatism. But as convincing as this picture is, there are some deficits.

The first and most general is the normative vagueness of Hausman's work. He provides various pragmatic defences of economic practice and in the end argues for caring more about empirical data and less about a unified theory. He states that in general, economists are justified in their methodology, but sometimes they exaggerate. While this seems prima facie to be a correct diagnosis, Hausman fails to specify the exaggerations more precisely or to give substantive argumentative background to his normative claims. He writes that for economists, »piecemeal theorizing that relies on substantive generalizations with limited applicability is apparently not worth considering. But such theorizing seems to be needed.«[23] This is essentially the same plea that Hans Albert made in 1950s when he asked for a sociologisation of economics. But is it really astonishing that Hausman asks for more such work, if he looks exclusively at neoclassical economics that is actually *defined* by a separate methodology of rational-choice theorising? Hausman correctly identified neoclassical economics as being a separate science. His normative plea to give up on the separation a little is therefore not convincing. It would probably work better the other way round: Why not ask psychologists and sociologists to apply their methods to economic problems than to ask economists to give up their (separate) methodology? If Hausman's prescriptions are promising, certainly other sciences should be interested in analysing economic problems.

Additionally, Hausman's picture of economics is clearly too narrow. Even if he unmistakably states that he only deals with mainstream neoclassical economics, he even excludes models that use equilibrium theory but employ non-pecuniary costs. This is vital for his separation thesis to make sense, but it is doubtful if even mainstream economists would agree to such a characterisation. At the latest since the works of Gary Becker, economists do not draw the demarcation-line to other sciences not in terms of the economic domain they discuss but merely by the rational-choice methodology they employ. Even if, using this definition, economics is still separate and does not allow for irrational behaviour or true altruism, it is much less separate than Hausman's picture suggests, because it already integrates all sorts of costs.

Next, Hausman does not pay much attention to justifying his empiricist convictions. While they are undoubtedly convincing as far as scientific practice is concerned, one could expect more attention to the shortcomings of empiricist epistemology from a philosopher – citing John Stuart Mill will not

[23] Hausman (1992a), p. 244.

do for convincing the enemies of empiricism. This is especially true because Hausman provides so much defence for neglecting empirical data and sticking to an a priori-justified theory. Hausman deals in a similarly pragmatic way with induction: He accepts it as an integral part of science that can increase the credibility of a hypothesis. Again, for philosophers this might be problematic, but since Hausman wrote his book in order to change economics,[24] it is probably more apt to avoid lengthy discussion of the problem of induction because they are not likely to solve it.[25]

There is another criticism of Hausman by Uskali Mäki, who says that inexactness and separateness – which are, according to Hausman, the key concepts for understanding economics – are not independent factors, and takes the view that economics' being a separate science is one reason for its inexactness.[26] Again, there is much to this point, and philosophically it hits Hausman's concept. But from a pragmatic point of view it does not matter very much, as the description of economics as a separate and inexact science is not affected by the dependency of these two main characteristics.

One could summarise most of the criticisms of Hausman by saying that while his picture is brilliant on the descriptive side, it leads at best to normative vagueness. If theory appraisal is the goal, something must be added to Hausman's still-impressive work.

4.1.1.2. Lessons Learned

In which sense is Hausman's work a pragmatic solution to the dichotomy of empiricism and relativism?

It is one, because it shows how vagueness and tendency-statements in economics can be justified without referring to an empirical basis and why claiming more severe falsification is probably not helpful for the advance of economics. Hausman shows why employing an a priori method is not unscientific if there are pragmatic reasons for the a priori statements. So Hausman is clearly neither a falsificationist nor a classic empiricist. But he is not a relativist, either: Not everything that economists do is justified. Hausman criticises in particular their unwillingness to import central findings of neighbour sciences into economics if that would force them to give up the equilibrium reasoning.

24 See Hausman (1992a), p. 262.

25 There are, in fact, some newer philosophical discussions aiming at re-establishing induction as a valid method. For an overview see Rosenthal (2007). While Hausman does not offer a solution to the problem of induction, he argues at length that using induction is unavoidable by showing how it slips in even within Popper's radically anti-inductivist philosophy – and how Popper's views become entirely relativist if he does not admit this. See Hausman (1992a), p. 200 et sqq.

26 See Mäki (1998a).

His justification of neoclassical economics is not relativist, either. Here Hausman argues in favour of scientific values such as simplicity, generality, tractability and even theoretical beauty. Many of his justifications use explicit pragmatic reasoning (the word »pragmatic« can be found 29 times in his book compared to just 2 times in Mark Blaug's 1982 classic).

Finally, Hausman's characterisation of models as predicates that are neither true nor false by themselves allows him to take a less grim view on »formalism« than empiricist methodologists usually adopt. Note that Hausman does not argue that the best methodology will prevail automatically as the result of competition in the marketplace of ideas. He defends a normative methodology that avoids many of the shortcomings of classic empiricist positions, the main problem with his position being that there is no »epistemic meat on the normative bones«[27], meaning that his normative claims are somewhat underargued. I will try to fill that gap in the following sections.

4.1.2. A Pragmatic Interpretation of Milton Friedman's Methodology

Milton Friedman's seminal 1953 essay on economic methodology has dominated the discussion among economic methodologists in many ways. As far as practicing economists are concerned, it is the only piece of methodology that a large number of them are aware of.[28] In economic methodology, it is surely the most highly debated statement ever made. In general, the more philosophically inclined methodologists have been highly critical of Friedman's position, calling it unclear or even self-contradictory and false.[29] I will not repeat many of these criticisms here, but will rather present a new reading of the essay that has emerged only recently. This reading stresses the pragmatic aspects of Friedman's methodology and can hence help for finding a methodology that avoids both the empiricist and the relativist sins described in the preceding chapters.

The most commonly held view reduces Friedman's essay to the point that the assumptions of a theory do not matter because all we should expect from

27 Hands (2001), p. 310.

28 See Hausman (1992a), p. 162.

29 For some important discussion see e.g. Musgrave (1981), Rotwein (1959), Samuelson (1963), Boland (1979), Musgrave (1981) or Mäki (2009) for a recent book collecting several papers on F53 (short for Friedman (1953)). I will not deliver an introductory summary of Friedman's essay here, as it is hard to summarize it in a neutral way due to the huge amount of different interpretations available in the literature. The pragmatic interpretation which I will develop here is therefore not the only one consistent with the text. Of course, in my view, it is the one that fits best Friedman's general point, even if it has to live with some inconsistencies.

economics is good predictions. This is a grossly misleading interpretation, as I will show.[30]

Before presenting the new interpretation, I shall discuss Daniel Hausman's arguments *against* Friedman, because they are typical for a large class of accusations made against Friedman's essay. Interestingly Hausman calls Friedman's methodology »pragmatic instrumentalism«[31], but as we shall see, Hausman himself does not take this label seriously.

According to Hausman, Friedman claims that the assumptions underlying a model are irrelevant and all that is relevant is predictive success.[32] Hausman tries to spot an error in this claim by providing an analogy: Suppose you want to buy a used car. Friedman would say that the only relevant test for assessing the quality of the car is checking whether the car drives safely, economically and comfortably. Looking under the hood and checking the status of the components is not necessary. Hausman claims it is obvious that no one would buy a car without looking under the hood. Similarly, we should check the assumptions of theories as well and not merely rely on predictive success as the only criterion. Hausman takes this to be an argument against Friedman's position.

This critique is typical for a class of accusations made against Friedman's case. Such accusations, however, are attacking a straw man, because a more thorough reading of Friedman's essay easily shows that he does *not* hold the position that the assumptions of a theory are irrelevant.

When Hausman writes »what is relevant is not whether the assumptions are perfectly true, but whether they are adequate approximations and whether their falsehood is likely to matter for particular purposes«,[33] this is meant as an attack against Friedman – but ironically it is rather a reformulation of his position. As Friedman says, »To put this point less paradoxically, the relevant question to ask about the ›assumptions‹ of a theory is not whether they are descriptively ›realistic,‹ for they never are, but whether they are sufficiently good approximations for the purpose in hand.«[34] Reading the foregoing quote it becomes clear that Friedman does not claim that the assumptions of a theory do not matter, but quite the opposite: It is crucial for a theory

30 See Schliesser (2005), Schröder (2004) and Hoover (2004) for some other recent interpretations of Friedman's classic that agree on this point.

31 Hausman (1992a), p. 162. This is due to the fact the Friedman takes predictive success as a criterion only for the phenomena a theory is intend to explain.

32 See Hausman (1992b), p. 71.

33 Hausman (1992b), p. 72. This quote points to an inconsistency in Hausman's attack, who on the one hand claims that assumptions should be adequate »for particular purposes« and on the other hand says that wide (not narrow) predictive success should be used for judging the adequateness of assumptions. Friedman is more consistently asking for narrow predictive success.

34 Friedman (1953), p. 15.

whether the assumptions are sufficiently rightly chosen! Similar to Hausman, Friedman claims that realisticness[35] is the wrong criterion by which to judge assumptions. He tries to show this by a reductio ad absurdum: »A completely 'realistic' theory of the wheat market would have to include not only the conditions directly underlying the supply and demand for wheat but also the kind of coins or credit instruments used to make exchanges; the personal characteristics of wheat-traders such as the color of each trader's hair and eyes [...]«[36]. The recent discussion of Friedman eventually acknowledges that Friedman does not encourage ignoring the assumptions but demands to look for pragmatically usefully assumptions instead of mechanically trying to improve their realisticness.[37]

The above arguments should make it clear that Friedman does *not* consider the assumptions of a theory to be irrelevant; rather, he points to the deficits of the naïve demand for more realistic assumptions in economics. Friedman takes a contrary position to this view: Model building necessarily requires simplification, and good models can »explain much by little«.[38] This is why interesting models must rely on assumptions which are descriptively unrealistic – and making them more realistic does not lead automatically to better models. Modelling is different from mere abstraction; it necessarily involves construction and is not just a matter of extracting parts from reality. These are all well-accepted arguments supporting the view that it can be reasonable to use unrealistic abstractions. What neither the above arguments nor Friedman imply is that all unrealistic assumptions lead to good models.[39] His point is rather that some unrealism is necessary, and it is even an advantage if it is unrealism of the right kind.

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[35] Note that Friedman equates »realistic» assumptions with descriptively accurate ones. Therefore, his thesis is not an ontological but a methodological one. »Realisticness« is a term introduced by Uskali Mäki that distinguishes descriptively accurate (realistic) assumptions from the philosophical position of realism (the relation of theories to the world). See e. g. Mäki (1998b)

[36] Friedman (1953), p. 32.

[37] Friedman notes that it is not all clear per se what counts as assumption and what counts as implication, because it depends on the aim of modeling if a statement like »firms maximise profits« is used as a basic assumption or if it is an implication. See Friedman (1953), p. 8 and p. 26-27.

[38] Friedman (1953), p. 14. See Bornholdt (2005) for a recent statement of this methodological credo even in the natural sciences.

[39] Sometimes it seems that this view is attributed to Friedman, even if it is obviously absurd. (See e. g. Samuelson (1963), p. 233.) Such critics seem to forget that Friedman accepts only those assumptions that lead to correct predictions. Besides that, it is a simple logical error to conclude from Friedman's statement »the more significant a theory, the more unrealistic the assumptions« that unrealistic assumptions imply significant theories.

When looking for a new interpretation it is »[…] necessary to detach oneself from the technicalities of the argument and to ask quite naively what it is all about.«[40] I. e. even if many critics validly find inconsistencies in F52[41], it is still possible they misunderstand Friedman's main point which is why their criticisms are not particularly convincing. Besides that, even philosophically problematic methodological advice can be helpful for practitioners – and it is clear that Friedman wrote his text for economists and not for professional philosophers.[42] The pioneering publications that gave the discussion of F53 its pragmatic turn were Hirsch/DeMarchi 1990 and Mayer 1993.[43] In the following, I present a personal summary of a pragmatic reading of Friedman's methodological position:

1. It is important to note that F53 was written at a time when Popper was unknown to most economists. F53 is best understood as a plea against the unproductive methodological quarrels between empiricists, who argued in favour of gathering data for deriving a more descriptively accurate economic science, and the neoclassical school, which seemed to advocate an a priori method of blackboard economics and did not care much about the empirics at all.[44] Friedman's position lies in the middle of the two extremes, as he demands empirical adequacy in the outcomes but not at the foundations of economic theories.
2. Friedman's much-criticised credo »the more significant the theory, the more unrealistic the assumptions«[45] and its even more counterintuitive inversion »the more realistic the assumptions, the less significant the theory« can be salvaged: At this point of his argumentation, Friedman is merely advocating to »explain much by little«[46]. The examples Friedman employs (recall the quote about a realistic representation of the wheat market above) to show why it is of no use to dogmatically insist on more realistic assumptions support this view. It is doubtful whether Friedman licensed the formalist revolution by this, as he clearly stresses

40 Mayer (1993), p. 213.
41 From this point on, F53 is used as an abbreviation for Friedman (1953).
42 See e. g. Hammond (1992), p. 229.
43 Other important contributions to this new line of interpretation include e. g. Boumans (2003), Hoover (2004), Schröder (2004) and Schliesser (2005). Hoover is probably the most extreme case, as he even attributes causal realism to Friedman.
44 The most prominent case of a methodological debate around this question is the so-called ältere Methodenstreit between Gustav Schmoller and Carl Menger at the end of the twentieth century. It is doubtful, however, if the protagonists really held the extreme positions that are nowadays attributed to them. See Backhaus/Hansen (2000).
45 Friedman (1953), p. 14.
46 Friedman (1953), p. 14.

the need for testable predictions.[47] He is pleading not for unrealistic assumptions but for evaluating the quality of assumptions only indirectly by their ability to create useful and empirically valid models for solving given problems.[48] This does not differ in principle from a Popperian account of science as critical guessing, where all theories are nothing but hypotheses. In short, again: What matters is the adequacy of assumptions, not their »truth«.

3. Friedman's position includes explanation as a valid aim of science and is not restricted to prediction or merely finding correlations. Contrary to a widely accepted interpretation, Friedman does not see prediction as the *only* aim of science. Rather, the way he uses the term »prediction« *includes* explanation – he explicitly notes that prediction, in the sense he uses it, is not restricted to future or novel facts.[49] Accepting this, Friedman is not an instrumentalist who cares only about prediction. Rather, his views can be interpreted as a pragmatic type of critical rationalism, since he advocates falsifying theories by empirical test but allows for inductive reasoning more openly than Popper does.[50]
4. F53 is an early piece of methodology advocating the economics of science approach, which will be discussed in section 4.1.3. The economic approach to methodology may be the underlying reason for the success F53 had. One quote from F53 shall suffice here for substantiating the claim:[51] »The gains from greater accuracy, which depend on the purpose in mind, must then be balanced against the costs of achieving it.«[52]
5. As Hirsch/DeMarchi conclude in their extensive study, Friedman's methodology is founded in American pragmatism and is therefore placed in the middle ground between empiricism and relativism[53] that I am exploring in this chapter.[54] Friedman is taking problem-solving in the centre of theory evaluation. Therefore, classic criteria such as logical

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47 See Hands (2003) for a discussion.

48 Friedman's strong focus on problems is the reason why he opts against the search for realistic assumptions. E. g., if preferences are better seen as constant or not depends on the problem one tackles and not on the »nature« of preferences.

49 See Friedman (1953), p. 9. Additionally, he uses the word »explain« 23 times in his essay, but only sets it cautiously in quotation marks 3 times – which shows that Friedman does not in general have a problem with the term.

50 Recall the discussion about verisimilitude and corroboration in section 2.3.2.2 where it was shown that Popper cannot avoid inductive reasoning completely.

51 A more thorough analysis can be found in Schröder (2004), p. 190 et sqq.

52 Friedman (1953), p. 17.

53 Actually, they rather see Friedman as being in the middleground between empiricism and apriorism, but as Friedman firmly expresses normative claims, he can surely be seen as an opponent of relativism, too.

54 See Hirsch/DeMarchi (1990).

> consistency and even truth do not matter *per se* but only when they are relevant for the problem in hand.[55]

Presented like this, Friedman's position sounds rather acceptable, but – as intense as the discussion centred around Friedman's essay still is – a defence of this reading is surely needed. In the following I will defend Friedman's central argument in favour of unrealistic assumptions and present in greater detail the pragmatic aspects of his methodology.

When accepting the pragmatic interpretation of F53, it is important not to confuse the notion of »pragmatism« with a pragmatist theory of truth or a kind of anti-theoretical conviction. Pragmatism here is to be understood as the conviction that it is important whether or not a theory »works« – and that this is independent from whether or not it is a true representation of the world.

I will start the defence of my pragmatic reading with the most provocative point of Friedman's methodology, which is surely his position concerning the value of realistic assumptions. Common sense and orthodox methodology[56] hold the view that good economic theory must be based on realistic assumptions. Friedman, however, seems to argue backward, from the corroboration of the implications to the quality of the assumptions. In a strict logical sense, this is of course erroneous: If A I holds, it is wrong to conclude from that I A. But Friedman does not argue like this; his reasoning is more complex. He recommends starting an analysis by carefully examining the empirical situation one aims to study, i. e. to start with a study of the implications of a theory.[57] Then he advises to try to rationalise these findings by building a model based on assumptions.

This model is tested against the empirical data, mostly by trying to retrodict historical data. If this procedure is successful, this counts as a confirmation of a model. Contrary to Popper, Friedman allows for induction here, i. e. the more a model is confirmed (=not falsified) by empirical testing, the more reason we have to believe in it.[58]

Now, in what sense are the assumptions of a model unrealistic? There is more than one line of interpretation, as Friedman seems to use the term »un-

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55 Friedman takes problem-orientation much more seriously than did Popper, who conceived of science as a quest for truth (and truth is a concept that is problem-*in*depdendent by definition). This allows Friedman to accept some economic theories that Popper must reject, because they have counter-instances outside the domain of problems they were made to help solve.

56 E. g. Hausman and Samuelson (1963) argue along these lines.

57 See e. g. Hirsch/DeMarchi (1990), p. 103.

58 See Friedman (1953), p. 29. Similar to Hausman, Friedman avoids a detailed discussion of the problem of induction, probably because he believes that no solution is available.

realistic/realistic« in various ways.[59] First, as noted above, assumptions are false in the trivial sense that they are never complete. But Friedman wants to state more than that. For him, modelling includes not only abstraction, i. e. omitting details from reality, but to go beyond the data and create assumptions about causality via abduction[60], as the *mechanisms* that the researcher postulates are usually empirically unobservable. If this is granted, assumptions will be unrealistic in a sense that is different from descriptive incompleteness; they are »unrealistic« because they contain more than what is observable.[61] In a third sense, assumptions may be unrealistic because they are directly opposed to observation or contradict common knowledge.[62] This is a type of unrealism that can be attributed to many assumptions in economic theory. All three of the above cases show that it is possible for assumptions to be unrealistic in some sense and yet useful for a specific purpose.

But again, Friedman's claims go further: He argues that realisticness is not a criterion for assumptions at all.[63] Now, usually economic assumptions (such as utility maximisation) are defended in terms of their realisticness, which probably means that they describe the motives of human behaviour in a plausible way. But how can we judge if the seemingly plausible assumptions are realistic? This can be done only by looking at the implications they yield. This is the core of Friedman's argument: If one tries to defend assumptions independently from their implications, one is restricted to vague and conventionalist plausibility-considerations, which cannot deliver a solid basis for research.[64] According to Friedman, plausibility considerations are even harmful to scientific reasoning: Appearances are often deceiving, and relying too much on what seems plausible hinders creative progress in model building – our plausibility assessments can change radically when a seemingly implausi-

59 See Hirsch/DeMarchi (1990), p. 15 et sqq.

60 Abduction is a technical term coined by Charles Sanders Peirce and simply means »studying the facts and devising a theory to explain them.« Peirce (1867), 5, p. 145.

61 This statement does not touch the philosophical position of scientific realism, which is a *theory* about the truth-status of causal connections in scientific theories. The above argument is headed against a more naïve form of realism, which identifies realism with a one-to-one correspondence to observation.

62 Friedman does not actually make these distinctions, which led to many ambiguities concerning his position. See section 4.1.2.1 for a discussion.

63 See e. g. Friedman (1953), p. 41.

64 See Hirsch/DeMarchi (1990), p. 16. Mäki points out that even if there is no direct test for assumptions, they can still be tested in other contexts and hence attributed a certain degree of realisticness before they are applied in a new model. See Mäki (2005b), p. 9. Friedman attributes this kind of reasoning to the utility-maximisation assumption. See Friedman (1953), p. 28.

ble model yields good results and becomes more and more accepted.[65] One of the best examples for this is surely Darwin's theory of evolution, which is based on assumptions that seemed strongly implausible at time the theory was developed.

To sum up, Friedman *starts* with the implications and cares about the assumptions afterwards, but this is not to say the assumptions are irrelevant; quite the opposite! First, of course the assumptions must lead to implications, which fit the observable world at least roughly in the relevant aspects. Second, they should be of a kind that encourages further research, i. e. they should be general, simple and adoptable to new applications (this is what Friedman calls »fruitfulness«). Third, they should be as simple as possible, but not simpler.[66]

As should become clear from the foregoing discussion about assumptions, the process of enquiry is of high importance in Friedman's methodology. In contrast to Popper he is not very interested in justifying theories as »scientific« – Friedman's point is more about a what the process of finding, discussing and improving theories should look like – with the provocative result that the »realisticness« of an assumption is not per se a valid point of criticism, nor is the realisticness of an assumption a helpful aim for practicing researchers. This view can be labeled »instrumentalist«, but it is certainly not atheoretical. One additional remark should be made to clarify this: Even if Friedman argues against realistic assumptions, this does not imply that he favours absurd assumptions.[67] When we think of assumptions we aim at explaining phenomena, therefore we must be able to imagine some causal connection between the assumptions and the implications they are made to explain. Otherwise any correlation between assumptions and implications would count as a theory, which is clearly not Friedman's position.[68]

It is clear that Friedman sees predictive success as the main criterion for theory evaluation. But if Friedman stresses the predictive success so much, how can he allow for abstract theorising that does not make concrete predictions at all? Again, there is a pragmatic solution for this: Friedman sees abstract theory as a language that can serve as a »filing system«[69] and can help to organise data without making predictions, but ultimately the theory has

65 Besides this, many defences relying on plausibility certainly lack plausibility for non-economists, again the notion of a fully rational economic man being the most prominent example. If economists try to defend their models by predictive or explanatory success in reply, they confirm Friedman's view.

66 See Friedman (1953), p. 10.

67 As Mäki points out, »the required degree of approximation is not simply to be maximized [or minimized!] but is relative to the purposes that the theory is supposed to serve«. Mäki (2005b), p. 7.

68 See Hirsch/DeMarchi (1990), p. 153 et sqq.

69 See Friedman (1953), p. 7.

to be helpful, even if in some very distinct sense, for the purpose of prediction. The picture is similar to Quine's view of logic and mathematics, which in Quine's opinion are ultimately justified by empirical confirmation, as they are on the very basis of many theories that are well corroborated.[70] Seen like this, Friedman acknowledges the role of theory and seeks to establish a kind of reflective equilibrium between empirical data and well-established principles of economic theory. To sum up: Pure theoretical and at first empirically empty reasoning is useful, because it eventually leads to better predictions than theory-free correlation processing ever will.[71] Scientific *proof*, however, is unattainable. As said before, the backward reasoning from effects to causes is deemed to deliver uncertain and tentative knowledge. But in opposite to those who want to set science on solid ground, Friedman sees science as a process:[72] We can never be sure if a particular theory lists real or false causes for a certain effect, but the more support that can be independently found for it, the more we reasonably become convicted of it.[73] Friedman admits that there is much subjective choice involved in his theorising, and hence the assumptions he makes are probably tailored to strengthen his convictions rather than refute them. Again similar to Quine, Friedman's idea of a good theory is best characterised as a web of beliefs, which draws its persuasive power from its internal and external coherence: The emerging picture is by no means conclusive, but it sets an argumentative level for potential opponents. Of course, when we adopt such a view of science, crucial experiments and falsification seem to drop away.[74] After having laid out the pragmatic reading of Friedman's methodological position, let's see how it can deal with some recent criticism.

4.1.2.1. Critical Discussion

Taking into consideration the huge number of different interpretations Friedman's methodological work has received, the problem first and foremost is certainly the terminological vagueness of F53. As I tried to show above, it

[70] See section. 3.1.1.1.

[71] This view is supported by the fact that even the most abstract and seemingly absurd theoretical constructions, such as a calculus with irrational numbers, is used in substantive and well-confirmed theories like quantum physics, which is surely preferable to a purely statistical science. This imaginary construct of a »purely statistical science« shows how absurd a far-going atheoretical position would look.

[72] See Hirsch/DeMarchi (1990), p. 135 for more on this.

[73] All this reasoning is pragmatic, of course, and it neglects difficulties such as Goodman's new riddle of induction (See Goodman (1955), 89 et sqq.). But if we took paradoxes seriously all the time, empirical progress would certainly be unattainable.

[74] Schliesser (2005) concludes that for Friedman the »major purpose behind prediction and testing is … not confirmation or refutation, but the generation of information to improve theory.« (Schliesser (2005), p. 72).

is nonetheless possible to distil a coherent methodological position from it, but due to the ambiguities of F53 there is room for doubt if this is really Friedman's position.

As many standard criticisms have been implicitly or explicitly[75] dealt with in the above section, it makes little sense to present those criticisms here in detail again. Instead, I will concentrate on two contributions that deserve separate discussion, because they are generally sympathetic to F53 but offer some suggestions for clarification: first, Musgrave's famous distinction of different types of assumptions and second, Mäki's recent comments to F53.

Alan Musgrave distinguishes between three classes of assumptions and accuses Friedman of making the error of lumping them all together. The classes of assumptions are defined as follows:[76]

1. Negligibility assumptions: These are assumptions about properties of the world that do *not* affect the hypothesis in question. An example found in Friedman's essay would be the assumption that the colour of the traders' eyes is negligible.
2. Domain assumptions: If the negligibility assumption is true only for a certain domain, the scientist can still stick to it by transforming it into a domain assumption. A property of the world is negligible within a certain *domain* of phenomena. Again, the colour of the traders' eyes is negligible – not in general but within the domain of models for finance markets.
3. Heuristic assumptions: If it turns out that there is no domain where a factor that is assumed away in a model is negligible (i. e. if there is no domain assumption applicable), the scientist can still proceed and transform the domain assumption into a mere heuristic assumption, i. e. a rule for simplification when building the basic components of the theory, excused by the promise of dealing with the disturbing factors at a later stage of model building.[77]

According to Musgrave, implicit negligibility assumptions do not lead to »descriptively false« models when they are essentially correct: The colours of

[75] Recall e. g. the discussion of Hausman's point in the beginning of section 4.1.2., which was taken as a representative for the bulk of criticisms that directly oppose the »F-Twist«.

[76] See Musgrave (1981), p. 278-382.

[77] A nice example from economics is the rational-choice approach, which *heuristically* looks for »rational« (i. e. »explicable by maximising behaviour«) explanations of human behaviour without holding the thesis that irrational behaviour is negligible nor assuming that economic explanations can exist only in the realm of purely rational decisions This makes the heuristic assumption of rational choice untestable, of course.

the traders' eyes *are* indeed negligible in an economic context, and hence a model that implicitly makes such a negligibility assumption and ignores the colour is *not* descriptively false but rather realistic. However, making wrong negligibility assumptions is nothing but an error of theory; therefore Friedman's credo »the more significant the theory, the more unrealistic the assumptions« does not hold here.

Musgrave's argument against Friedman's case is similar with domain assumptions: If a theory makes false domain assumptions, this is a clear disadvantage of the theory.[78]

The discussion of heuristic assumption is the final clue to clearing up the misunderstandings between Friedman's and Musgrave's position. Musgrave states »At any rate, his central thesis 'the more significant the theory, the more unrealistic the assumptions' is not true of 'heuristic assumptions' either.«[79] It is hard to see how Musgrave wants to judge the realitsticness of a heuristic assumption if he accepts that they are untestable.[80] The question is not whether heuristic assumptions are true or false but whether or not they are able to generate fruitful fields of research.[81] Musgrave's distinction between three classes of assumptions is certainly a brilliant addition to the assumption-debate, but it does not refute Friedman's position, as it fails to show why seemingly implausible heuristic assumptions such as rational choice are nothing but an error of a theory. As far as the two other types of assumptions go, it seems that Musgrave is attacking a straw man rather than Friedman's position (at last in the pragmatic interpretation): Nowhere does Friedman say that making false negligibility or domain assumptions helps to generate significant theories. He says that significant theories are mostly based on unrealistic assumptions, not that any unrealistic assumption creates a significant theory! When Musgrave stresses that wrong negligibility and domain assumptions usually lead to bad theories, this can be interpreted as stressing Friedman's point that the empirical correctness of the implications is relevant for judging assumptions: As soon as wrong negligibility and domain assumptions lead to wrong predictions (which they most probably do instantaneously), they are immediately ruled out. If one takes the ability to predict seriously, one cannot come up with wrong negligibility

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78 The acceptance of domain assumptions is probably the main point of misunderstanding between Friedman and many critics (take Hausman (1992b) again as an example): Friedman recommends evaluating the assumptions only for specific purposes, whereas many of his critics aim at broad predictive success. The question is, however, if broad predictive success is achievable at all. Friedman holds the view that it is not – there is no »theory of everything«, so narrowing the domain of theories is always necessary.

79 Musgrave (1981), p. 385.

80 See footnote 77 in this chapter.

81 See Mäki (2000), p. 326.

or domain assumptions.[82] The case is different with heuristic assumptions, as they are neither directly comparable to reality nor lead directly to empirical implications. Here it seems still correct to allow for assumptions that seem prima facie implausible or unrealistic. Musgrave has done a great job at clarifying what Friedman *cannot* mean by unrealistic assumptions; but as I see it, he has failed to refute my pragmatic reading of F53.

Now let's have a look at Uskali Mäki's recent discussion of F53. Mäki has written dozens of contributions to the F53 debate, and I shall discuss only the most recent here, as there is a continuous development in his writing and the older ideas reappear in his newer works as well. Mäki's comments to F53 are particularly interesting, because they do not reject its main thesis out of hand but deliver clarifying suggestions for ambiguous points of the essay and the debate around it.

First, there is a terminological point Uskali Mäki has continuously raised and which I have adopted in my writing: He argues that it is better to speak of the »realisticness« of assumptions than of their »reality« or »realism«, because even false assumptions have reality (once made, they exist and are hence »real«) but a low degree of realisticness.[83] The philosophical position of realism does not depend on the use of realistic assumptions:[84] The belief that isolating the main causal factors may depend on the omission of others and on the introduction of false idealising assumptions is perfectly compatible with the philosophical position of realism.[85]

In a recent overview article, Mäki presents a »map of multiple perspectives« on F53, which shows the huge variety of possible approaches to the essay from realist to instrumentalist and even social constructivist.[86] This leaves two possible conclusions: Either the essay is hopelessly vague and ambiguous and therefore has no message at all, or the philosophical attributes attached to it are hopelessly useless for labelling the essay and understanding Friedman's points. While the ambiguities are certainly a problem for the philosophical discussion of F53, they are not necessarily a problem for economists, because the different labels may not make a difference for the lessons to be learned from Friedman. F53 does certainly not promote a single coherent methodological position, but still many arguments it puts forward are worth considering.[87] One could argue that Friedman delivers a »best-of« mix

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82 Marcel Boumans adds that Friedman encourages empirically exploring the domain of a negligibility assumption. See Boumans (2003), p. 320.

83 See Mäki (1989). Where realism is a theory of theories, realisticness is a property of them, Mäki suggests. I have adopted this terminology in this chapter.

84 See Mäki (2005b), p. 18 et sqq.

85 See Mäki (2005b), p. 11 and p. 20.

86 See Mäki (2005a)

87 Thinking in bigger terms, this calls for a discussion of how to assess consistency as a quality criterion at all.

of central insights that mostly became known in philosophy of science only decades after the publication of F53.[88] Here is a list of concepts that were already being discussed by Friedman in 1953 – at a time when Popper's »Logic of Scientific Discovery« had not yet been translated into English.[89]

1. Kuhnian aspects in theory-choice: Friedman describes in detail why subjective judgement is involved at several levels of theory choice and how social factors of science influence the stabilization of paradigms.[90]
2. Underdetermination and holism: Friedman states that »observed facts are necessarily finite in number; possible hypotheses infinite. If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are.«[91] Later he gives a detailed discussion of the interchangeability of assumptions and implications.[92]
3. Theory-ladenness: Friedman states that »even when a test can be made, the background of the scientists is not irrelevant to the judgements they reach.«[93]
4. Constructive falsificationism: Friedman states: »Here there are two important external standards of comparison. One is the accuracy achievable by an alternative theory with which this theory is being compared and which is equally acceptable on all other grounds.«[94]
5. Economics of scientific theories: Right after the preceding quote, Friedman continues: »The other arises when there exists a theory that is known to yield better predictions but only at a greater cost. The gains from greater accuracy, which depend on the purpose in mind, must then be balanced against the costs of achieving it.«[95]
6. Pragmatism and eclecticism: The pragmatic aspects of F53 have been pointed out above. One could easily go a step further and call the somewhat inconsistent mix of methodological insights an early piece of postmodern eclecticism.

It is one thing that F53 is hard to label and presents a somewhat inconsistent mix of methodological positions. However, there are internal inconsistencies and fallacies of reasoning, too. The most prominent is, again, Friedman's ar-

88 Mäki calls this the F-Mix rather than the F-Twist; see Mäki (2005b), p. 2.
89 When a short quote is available in F53, I included it as a short »proof« in the list. Note, however, that all of the listed items appears more than once in F53.
90 See Friedman (1953), p. 22-23 and p. 30.
91 Friedman (1953), p. 9.
92 See Friedman (1953), p. 27-28.
93 Friedman (1953), p. 30.
94 Friedman (1953), p. 17.
95 Friedman (1953), p. 17. If you think this is an over-interpretation, read Schröder (2004).

gumentation about unrealistic assumptions. Mäki argues that F53 does not distinguish between »falsehood proper« and »incompleteness« – in both cases Friedman speaks of unrealistic assumptions. This leads Friedman to the argument that perfectly realistic assumptions are unattainable, and therefore all attacks against unrealistic assumptions are wrongheaded. Even if we grant Friedman's point that the unrealism of assumptions is not a valid point of criticism, his thesis is not well argued: »One cannot justify false assumptions by citing the trivial fact that all theories are necessarily incomplete.«[96]

Mäki tries to reconstruct F53 as a realist statement and uses, similar to Musgrave, a paraphrasing technique in order to show that most of the seemingly unrealistic as-if assumptions can be read as formulating the ceteris paribus conditions needed for isolating the main hypothesis in question.[97] I will discuss the question of whether a good methodology for economics should be realistic or instrumentalistic in section 4.2, for it should be clear now that this is a question that cannot be settled by an interpretation of Friedman's methodology alone; it is a separate matter.

4.1.2.2. Lessons Learned

Even if F53 is terribly unclear in some respects and even has some internal problems, there are a lot of lessons to be learned from it. The main thesis that a theory should not be judged by the realisticness of its assumptions holds even after critical discussions. Surely, F53 did not perform very well at delivering this message to economic methodologists, but in economics, it did the job rather well.[98]

Hausman and Friedman are often considered antagonists in economic methodology, because Hausman insists on the need for *credible* assumptions as a foundation for good economic theories, while Friedman argues precisely against this.[99] Nonetheless, their positions have much in common:

1. They both stress the importance of empirical research and take a stance against blackboard economics.
2. They both stress the need for tacit expert-knowledge when judging whether data rejects or supports a hypothesis.
3. They are both interested in finding mechanisms that link causes and effects.

96 Mäki (2005b), p. 11.
97 See Mäki (2005b), p. 16 et sqq.
98 See Mayer (2004).
99 Note that Hausman takes »credibility« as criterion for the assumptions, so that Mäki's claim that the as-if assumptions are in fact meant to be true ceteris paribus claims does not help here to reconcile the positions. Friedman's stance is directly opposed to »credibility«, as shown above.

4. Mäki's reconstruction of as-if explanations is very close to Hausman's view. According to this view, Friedman is not advocating the postulation of »unrealistic« driving forces in economic models, but only their unrealistic isolation.
5. Similar to Hausman, Friedman sees economic models not as true or false by themselves, but rather as a language – or in Hausman's terms, as a set of predicates that can fit or not fit to the observations.
6. Friedman believes in induction, similar to Hausman, and recommends the use of theories that worked before in different circumstances.[100]

The analysis of Friedman's methodology and its comparison to Hausman's is a crucial step towards a pragmatic methodology that lies in between radical empirical positions and purely theoretical ones. Friedman is concerned with solving problems and finding adequate theories for that task. He convincingly showed why seemingly more realistic assumptions are not always preferable. Additionally, he has many insights to offer that reappeared in postmodern methodology; however, Friedman does not use them to establish relativism but takes a pragmatic stance that focuses on solving problems. The major disadvantage of F53 is the huge variety of readings that are possible, but as I am primarily concerned with finding a pragmatic balance between positivist and relativistic positions and *not* with interpreting F53, this is only a minor drawback for my project.

4.1.3. Taking Pragmatism Seriously: Economics of Scientific Knowledge

The basic idea behind the Economics of Scientific Knowledge (ESK) has already appeared several times in this work. For example, in section 2.3.2.3 I employed economic cost-benefit reasoning in order to point to some of the shortcomings of positivist positions.[101] In this chapter, ESK reappears as a radically pragmatic position. It connects well to the foregoing pragmatic interpretation of Friedman's methodology and presents a way out of many »empiricist vs. relativist« quarrels.

The basic idea of ESK is using economic reasoning for tackling problems of philosophy of science. I will discuss two fundamentally different approaches:

The first approach is similar to my pragmatic interpretation of F53, as it puts the problem-solving capacity of theories in focus and encourages cost-

[100] Again, Friedman does not recommend ignoring the assumptions or, as Hausman puts it, not »looking under the hood«.

[101] Remember e. g. the argument saying that Newtonian physics can still be acceptable for solving some problems even if it is constructively falsified by Einstein's theory of relativity, because it is much simpler to use and therefore has lower »costs«.

benefit analyses of the respective theories for helping to solve specific problems. As said earlier, theories that are less precise may have pragmatic (and epistemic) advantages of tractability and are therefore preferable in some contexts.[102] Costs and benefits are of course not restricted to monetary values but include epistemic and pragmatic costs and benefits. Therefore, cost-benefit analysis is a very general heuristic for evaluation.[103]

The main twist of the second approach of ESK is to see scientists not as seekers of truth but as actors pursuing their own interests. While the search for truth may still be a major driving force for scientists, within an economic interpretation of science, truth is at least not the *only* driving force or even the most relevant one.[104] Such a view renders any methodology that tries to define rational criteria for choosing theories futile when it fails to explain why *using* the criteria is in the interest of an individual scientist. Therefore, this part of ESK concentrates on the institutional aspects of science that can channel egoistic individual behaviour into epistemically desirable outcomes.

I will start with a discussion of the cost-benefit approach: Such an analysis encourages thinking in alternatives: As Lakatos noticed, one cannot assess theories independently but must *compare* them to relevant alternatives. Scientists might then very well choose a theory that is e. g. less precise but simpler.[105] It depends on what they want to do with the theory: Explaining basic structures, in many cases, requires a different theory than does predicting future events.[106] By including efficiency as a factor in philosophical considerations about science, many classic paradoxes can be solved. Take the Raven paradox as an example:[107] The problem behind this paradox is the logical equivalence of the statements »All ravens are black« and »All non-black objects are non-ravens«. If you agree that a black raven is a confirmation for the thesis that all ravens are black, than logic forces you to accept that every non-black object that is not a raven confirms the same thesis. However, neither in daily life nor in science we would count the finding of a white shoe as a valid confirmation for the thesis that all ravens are black. Nicholas Rescher has provided an economic explanation for this:[108] As the set of non-black objects is much bigger than the set of ravens, it is much more *efficient* to look directly

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102 Remember the discussion of the KIDS vs. KISS methodologies for agent-based modelling, where I argued that with increased descriptive accuracy some basic effects can be overlooked. See section 2.3.2.4.

103 See Hansjürgens (2004), p. 343.

104 Note that the relativist's arguments against the very concept of »Truth« are set aside for the sake of simplicity here.

105 This acknowledges Kuhn's (and Friedman's) view that theory choice can never be a fully rational process but has to include personal judgement.

106 Again, see section 2.3.2.4 for a comparison of the KIDS and KISS approaches.

107 See Hempel (1945), p. 11 for the locus classicus.

108 See Rescher (1989), p. 109-113.

for black ravens. Because of the relative rareness of ravens compared to non-black objects, the observation of a black raven confirms the thesis »all ravens are black« to a much higher degree than every non-black non-raven does.

If such efficiency considerations are quite new in philosophy of science, the second approach of ESK might even be shocking to traditional philosophers of science. ESK does not only acknowledge that scientists do not always look for truth, but states that they might *not* even look after the pragmatically best theory for solving a given problem. In some models of ESK, scientists try instead to maximise their income and their reputation without caring much about good theories at all!

Such a view undoubtedly will cause mistrust in many readers: If scientists do not rationally seek truth, but rather pursue their own interests – doesn't that lead to the demise of the whole scientific project? Not necessarily. This is where economic reasoning kicks in: In pretty much the same way in which economists are eager to show that for socially desirable outcomes one does not need to rely on well-meaning motives of the members of a society, they try to show that scientists do not need to look after truth individually, but nonetheless may discover it collectively. In other words[109], it might be the case that an invisible hand is working in the way science is organised, one that can turn selfish motives of scientists into epistemically desirable outcomes. Science is supposed to be similar to the market-system in central aspects, so that Adam Smith's claim about individual egoism leading to collectively preferable results does hold here as well (on average, of course).

If this is the case, two previously antagonistic claims can be brought together: First, science is influenced by »irrational« social factors – which means that it is not as clean and rational as the positivists would like it to be – and second, its results are nonetheless epistemically distinct and hence rationally defensible. Defending such a claim makes it necessary to dig into the institutional organisation of science. The twist of ESK consists of shifting the discussion of scientific rationality away from single scientists and their theories to an overall view of the scientific process. Even if we cannot define purely rational criteria for theory choice, there is still plenty to say about how science should be rationally organised. This means that ESK builds heavily on the insights gained by Thomas Kuhn and the sociological approach, as it grants the social nature of science. On an individual level even the slogan »anything goes« is acceptable for ESK, as economic theories (and hence ESK) do not care about the behaviour of single individuals but only about the average quality of outcomes. So ESK can accept anything the positivists were fighting against, but still offers a defence of their view of science as a rational enterprise.

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109 Note that the notoriously dubious term »truth« is carefully avoided now.

In order to show how ESK arrives at normative conclusions, it is convenient to look at some more concrete examples.[110]

Philipp Kitcher was among the first to use economic models for analysing science, and one of his normative results favours pluralism: »Intuitively, a community that is prepared to hedge its bets when the situation is unclear is likely to do better than a community that moves quickly to a state of uniform opinion.«[111] In a rather formal way, Kitcher provides an argument for the possibility of the cognitive division of labour in science.[112] In his model, cognitive division of labour comes about as a collective result of each scientist selfishly promoting the goal of being the first in delivering a theory that her community will eventually accept. The idea behind the proof is that even theories which do not seem very promising can attract expectation-maximising scientists if there is e. g. low competition from other scientists. By this mechanism, an equilibrium emerges between promising theories with high competition and less promising ones with lower competition. This is clearly an invisible-hand argument, even if Kitcher does not actually use the term. While the argument does not offer criteria for good economic theories, it offers useful information for social planners who want to promote the efficient collective achievement of scientific results. Like many economic arguments it is normative by providing arguments for self-regulation of complex systems.

Economists have engaged with ESK as well. Paul A. David is a prominent example that I will use in order to extend the given picture a little.[113] David starts from the assumption that science is somehow epistemically privileged, but this speciality does not result from the superior rationality of the scientists but hinges rather on the special way in which academic science is organised. Referring to the Nobel laureate Douglas C. North, David's approach can be characterised as looking for »the connecting links between institutional structures … and incentives to acquire pure knowledge.«[114] The most distinguishing characteristic of academia, according to David, is its »openness«: Results are made public as quickly as possible and are then available for everyone to study and to criticise. The economic twist is here, again, to analyse the way in which this institutional characteristic turns self-interest into scientifically (and socially) desirable behaviour: If scientists want to be rewarded for their work, they are forced to publish it and to accept criticism. This leads to a high reliability of scientific results, as scientists lose their rep-

110 Important works include e. g. Hull (1988), Rescher (1989), Kitcher (1993), Dasgupta/David (1994), Laudan (1996), Wible (1997), Lütge (2001), Albert (2002) and many others. ESK has recently become a major trend in philosophy of science. See Stephan (1996) and Hands (2001) chapter 8 for overviews.

111 See Kitcher (1993), p. 344.

112 See section 7.4 for the formal proof.

113 See Dasgupta/David (1994) and David (1998).

114 North (1990), p. 75.

At the very least it creates sharp debates and pushes each paradigm to its very limits, thereby fostering progress in the long run.[120]

Parts of ESK obviously overlap with sociological studies of science, the main difference being the normative nature of the economic analysis. Economists are not only interested in how people behave, but they assume self-interested rational behaviour and try to deduce the results of this assumption under specific constraints. After such an analysis they recommend reforms of the institutional settings. And of course, even if the arguments of Kitcher and David are principally in favour of science as it is organised today, this is a normative position.[121]

Christoph Lütge is the last author that I will present in this overview of ESK. His work is particularly interesting for two reasons: First, he delivers nice reconstructions of classic criteria for theory choice in economic language;[122] and second, he utilizes James Buchanan's arguments in order to move beyond the classic picture of welfare economics that he claims to be the underlying normative principle of the approaches of Kitcher, Wible and Laudan.[123]

Until now, it was assumed that science (on average) brings about socially or scientifically desirable outcomes. As it turns out, however, it is hard to define what is desirable for a society and even for science as a whole. Traditionally, economists took Pareto optimality as their normative ideal: A social optimum is achieved if nobody's well-being can be improved without diminishing the well-being of someone else. James Buchanan has been critical of this normative ideal, for at least two reasons: First, the criterion uses an unachievable ideal that is not helpful as an evaluative standard for real situations; and second, it is an external-efficiency criterion that ignores the wishes of the people.[124] Buchanan replaces the Pareto criterion by a contractarian

120 Kuhn has already seen this. See Kuhn (1962), p. 38-39. Of course, there are costs of dogmatic behaviour, too. Most notably, these are higher transaction costs between paradigms, problems with incommensurability and reluctance to accept refutations.

121 More critical approaches are also available, of course – for example, there is an intense discussion about the effects of the economic power of certain publishing houses and their quality control mechanisms. I have omitted this discussion here, because it would lead too deeply into details and is likely to cause confusion about the meaning of the term »economic« here, as it suggests a too-narrow use of the term.

122 I do not present a summary or an example here, as Lütge's method does not differ in principle from Kitcher's or David's in this part. See e. g. Lütge (2001), p. 147 et sqq. for a discussion of the costs and benefits of Popperian falsificationism. See Lütge (2001), p. 130 et sqq. for a discussion of traditional quality criteria of science such as internal and external consistency, testability, explanatory power, simplicity, etc.

123 See footnote 109 in this chapter for the references.

124 See e. g. Brennan/Buchanan (1985), p. 151 et sqq.

utation if competitors detect errors in their results. Another institutional rule of science is the winner-takes-all principle – only those scientists who publish *new* results first are rewarded.[115] This leads to the highly innovative nature of science.[116] So even if the rules of science are fairly different from the standard market assumptions[117], economic and institutional analysis can contribute much to a better understanding of the special nature of science. Be sure to note here that neither Kitcher nor David argue for laissez-faire or low regulation in science; they merely argue in favour of some aspects of the current institutional setting of science.

The comparison of Kuhnian philosophy of science with the economics of standardisation[118] can give additional insights when allowing for invisible-hand arguments. The economic interpretation of Kuhn's philosophy provides reasons for the persistence of paradigms: It is in the rational self interest of each scientist to dogmatically defend his old results, as he loses reputation if they get falsified or – even worse – if the paradigm he is working on becomes outdated. The more human capital has been invested, the higher are the opportunity costs a paradigm switch generates.[119] Note that in contrast to the standard view on Kuhnian philosophy, from an ESK perspective dogmatic behaviour is by no means irrational; it is instead highly rational for the individual scientist. Additionally – and this is again an invisible-hand argument – dogmatic behaviour might not even be harmful to science as a whole.

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[115] The reward is nowadays mostly measured by means of the »impact« of a publication, meaning the number of references it attracts. The status of the »impact-factor« is highly controversial, mostly because popularity is not the same as substance: e.g. in some cases, an exceptionally bad publication can be cited quite often.

[116] As I am only introducing the idea of ESK, the depiction remains fairly rough here. To be conclusive, the above arguments have to be discussed empirically, of course. For example, it may well be the case that some scientific results are not reliable at all, because within certain communities there is no critical discussion of the ideas. This may be the case in communities small enough to make personal dependence among the majority of scientists highly probable. Additionally, the reward for confirming or falsifying old results of others is relatively low, which may cause a low level of reproducing experiments or even checking proofs. See Beck-Bornholdt/Dubben (1998), p. 192 et sqq. Clearly, there is room for improving the institutions of science.

[117] For example: First, in contrast to standard market-goods, scientific results become non-rivalry goods after publication; and second, there is no room for cost saving by means of imitation due to the winner-takes-all principle of the reward system.

[118] See section 7.1.

[119] Note, however, that this argument is not confirmed by empirical studies. See Lütge (2001), p. 80.

consensus-criterion about abstract rules. Full consensus in every question would be terribly costly, Buchanan argues. Therefore, full consensus can only be demanded for the most general abstract rules at the constitutional level.[125] Note that rules which are Pareto-superior to the status quo should find consensus – but now the hierarchy is turned upside down: The (Pareto)-efficient is not the declared optimum; the rules that find consensus are declared more efficient than the existing ones. Normativity is completely naturalised here: There is neither an external standard nor an external source for it. The only source for normativity is the consensus of the citizens.

Lütge builds heavily on this idea and transfers it to philosophy of science: Normative philosophy of science is not about single theories but about abstract rules of the scientific game. These rules are justified by the consensus of the community. Social constructivism is avoided by allowing internal factors to restrict the room for consensus. That is, Lütge acknowledges that theories are empirically underdetermined, but still claims that there are theories that do *not* fit empirical data and therefore will never find consensus.[126]

In this picture, the normative question is different from Kitcher's and David's, as it cares neither about single theories *nor about desirable outcomes,* but only about consensus for rules. Like Buchanan, Lütge does not ask for full consensus for every single question, but proposes a hierarchy of consensus: First, consensus for the constitution of a country is assumed. Therefore, the election process and the decisions of elected science-politicians are accepted. The politicians may find consensus at the most abstract level in saying that any form of science that is based on *some* set of rules is better than science with no rules at all.[127] In a second phase, scientists form communities by finding consensus that *defines* their community. The acknowledged experts of the field probably heavily shape these rules.

To sum up, good science is conducted by rules that can be legitimised through a hierarchy of consensus levels. Such a construction aims at avoid-

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125 Consensus is easier to achieve for abstract rules, because the more abstract the rule, the less people will hope for personal advantages or fear disadvantages. As people live behind a »veil of uncertainty« concerning their future, they will tend to approve fair rules. See Buchanan/Tullock (1962) and Brennan/Buchanan (1985) for the detailed development of these ideas.

126 Lütge is rather vague at this point. He seems to presume the existence of empirical data that is independent from the views of scientists. See Lütge (2001), p. 209. Elsewhere Lütge recommends accepting empirical knowledge that was used when designing a new theory as safe background knowledge for that theory. See Lütge (2001), p. 167.

127 Note that even rules for voodoo-science could be included here; so probably even Feyerabend would agree on this level. See Lütge (2001), p. 212. Buchanan is arguing for an economically balanced amount of rules and argues that too little and too many rules can both reduce efficiency. See e. g. Buchanan (1975), p. 12-14.

ing the role of a philosopher-monarch who tells scientists what they *should* do.[128] The role of the philosopher of science is now to look at the consensus rules of science and analyse how they solve dilemma situations. The term »dilemma situation« refers here to situations with a prisoner's-dilemma structure. As it is easy enough to imagine it for oneself, I will omit the famous PD-matrix in the following illustration.

Here is a basic example of such a dilemma situation: Each scientist of a community may have an advantage when he breaks methodological rules, but collectively the community can gain from established institutions.[129] Institutional economic analysis is used to assess the rules. For example, within a community there may be rules that define the standards of how certain experiments are properly conducted.[130] The above methodological dilemma is solved through excluding scientists who do not adhere to the given standards from the scientific community if their attempt to defraud is detected; by this, compliance to the norm is enforced. Norms like this can have, of course, both negative and positive effects: They reduce transaction costs and facilitate communication among researchers, but they can also hinder innovation, at least from a short-term perspective. The task of the philosopher of science is now similar to an institutional economist: He analyses institutions and their efficiency in promoting certain (scientific) aims. According to Lütge, the institutional economic analysis of science has to fulfil three tasks:[131]

1. Delivering and explaining a hierarchy of consensus as explained above.
2. Testing the efficiency of methodological rules compared to the scientific aims as defined by a higher consensus level. Testing is done by means of an economic reconstruction of historical theory-choice, i. e. it must be shown how the acceptance of methodological rules historically led to Pareto improvements for scientists.
3. Based on the knowledge gathered in the foregoing steps, the economist of science can propose reforms of internal (methodological) or external (organisational) rules. These proposals are considered empirically testable hypotheses.

Lütge provides the so-called Devon-controversy in geology as an extensive case study (which I cannot reproduce here due to space constraints). In the concluding sections Lütge reconstructs several rules of the scientific community as rational reactions to dilemma situations. The winner-take-all or pri-

128 See Lütge (2001), p. 213. For criticism, see section 4.1.3.1.

129 The scientific institutions are often informal ones, as methodological rules do not have legal status.

130 Note for example that such rules differ substantially between psychology and experimental economics. See Hertwig/Ortmann (2003).

131 See Lütge (2001), p. 217.

ority principle that was mentioned before is an example: Lütge grants the innovation-driving and efficiency-promoting effects of this rule, but also sees negative aspects. The priority principle makes plagiarism very tempting and can hinder collaboration and communication between scientists – especially when they work on long-term projects and are under high competitive pressure.[132] Here we find a prisoner's-dilemma situation again: Everyone would be better off if there was no plagiarism, but each individual scientist can gain from successful plagiarism. There are already rules in the scientific community that make plagiarism less tempting: E. g. scientists can lose their PhD title and thereby the ability to work as scientists if plagiarism is detected in their doctoral thesis. Because perfect control is impossible, a certain level of trust among scientists is indispensable for efficient work. Lütge concludes that the ideal of science as friendly competition among gentlemen has contributed a lot to resolving issues of scooping and plagiarism and has helped to solve the underlying prisoner's dilemma.[133] The task of the economist would be to propose more efficient rules, after having diagnosed a dilemma structure in the way science is organised. Such work is unfortunately only just beginning, but e. g. the emergence of open-access journals can be seen as a step in this direction.[134] Until now, ESK is mostly stuck in the second phase of economically reconstructing the behaviour of scientists.

One final remark on ESK: It is important to note that ESK is completely neutral regarding theories of truth. Its pragmatic nature might suggest a pragmatic truth criterion or even instrumentalism, but in fact, the economic analysis of scientific *behaviour* in certain institutional settings is completely neutral to the actual *aim* of the scientific game.[135]

4.1.3.1. Critical Discussion

We have now seen how the advocates of ESK try to re-establish a normative stance in philosophy of science. It is easy to imagine that there are problems with this approach, which I shall discuss below.

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[132] This is surely more common in the »hot fields« of the natural sciences than in humanities or social sciences.

[133] See Lütge (2001), p. 228.

[134] Open-access journals can make science more efficient for many reasons: They save licensing costs for the libraries and they facilitate communication and help to spread scientific results in poor regions of the world, thus fostering competition and pluralism. The emergence of scientific blogs has changed communication in a similar way. See e. g. Lindner (2007), Harnad/Brody (2004) and Reichardt/Harder (2005).

[135] However, the aim must be somehow detectable, otherwise no means-ends reasoning can be employed, because one cannot know whether the aim is achieved.

At first there are doubts about whether the »market of ideas« is indeed similar to commercial markets. James Wible provides several arguments why science is not really a market system:[136]

1. The »business cycles« in science are very long, so market stabilisation in terms of self-correctiveness works very slowly.
2. Scientific discoveries are treated as public goods. After publishing an essay, the respective scientists lose the exclusive property on their ideas.
3. Science has moved away from its demand side, many scientific works seem to be a kind of *l'art pour l'art.* Scientific standards are set by scientists themselves.
4. As opposed to commercial markets, there are no real prices in science. There are merely proxies like reputation.

Additional arguments offer further support for the thesis that science is different from fully competitive commercial markets. Suppose a scientific paradigm looks promising at the beginning, but proves unfruitful after some time:

1. There is low incentive to change the paradigm if the peers stick to it as well, because there is *no external control.*
2. Change is obstructed by the *financial incentives*, too: Scientists are mostly paid by the state, and success is difficult to control.
3. *Network effects*: Starting a new paradigm is difficult; one has to attract a substantial number of peers.
4. *Capital:* The more specific (human) capital individuals invest, the more difficult it is for them to switch, because the capital will be useless afterwards.
5. *Rational ignorance* could be a problem: It would be very costly for scien tists to inform themselves about new, possibly unfruitful paradigms.
6. *Psychological factors* like group-identity or feelings of discomfort towards any change, caused by the uncertainty about an individual's role in a future system, stabilize the present paradigm.

These arguments are important drawbacks for comparing science directly to commercial markets. But as I tried to point out in my overview, this is not the approach ESK takes. Rather, it uses economic reasoning to analyse science as a result of individually rational behavior. The above arguments show why science cannot react as quickly as commercial markets can, but it does not show that a long-term analysis of science by means of economic theories

[136] See Wible (1997), p. 153 et sqq.

is wrongheaded in principle. Rather, some of the above arguments already *use* economic reasoning.

The next problem to address is that ESK usually starts with the assumption that science *is* indeed somehow epistemically superior and on average, running just fine. Starting at this point seems to assume away the attacks of postmodern relativism rather than facing them. However, with ESK normativity has no universal domain but is rather instrumental, as it is restricted to hypothetical imperatives:[137] If you accept certain aims, then you should proceed in such and such a way. Put like this, the charge of dogmatism becomes less plausible, as ESK only *assumes* an epistemic advantage of science but does not depend on it. In theory, ESK could be used to analyse any other sort of enquiry as well. There is a reason why I add the limiting remark »in theory«: Of course ESK is not neutral in an absolute sense, but it relies heavily on the standard assumptions of rationality such as completeness, reflexivity and transitivity. Therefore, it is unsuited for the analysis of practices that fundamentally reject the very notion of rationality.[138] However, it seems fair to argue that large parts of science do *not* reject the standard view of rationality, and therefore the analysis of economics by means of ESK seems legitimate.

Additionally, there is a slightly different problem of normativity: Is the idea of analysing economics by means of economic methods not somehow cyclical? To find an answer, I have to take a big swing: When you think of an alternative to this cyclical analysis, you have to adopt a foundationalist position. If you accept the problems of foundationalist justifications that were discussed in the first major sections of this work, you are hopefully inclined to accept a somehow cyclical procedure as suggested by holism, coherentism or any naturalist epistemology. Once it is accepted that philosophy is

137 In this restricted view of normativity, the problem of reflexivity that Wade Hands lists (see Hands (2001), p. 391) does not emerge, because the aim is not »deriving a notion of good and bad science« anymore. For the sake of clearness, Hands' argument should be reproduced here: ESK says that scientists do not seek truth, but truth emerges nonetheless. If this is granted, then ESK theorists do not seek truth, either. Now, how can we know if *their* theory (ESK) is true? The claim that truth »emerges« in ESK is only true if this very theory (ESK) is true. Now the argument is circular, or rather, we are at the beginning of an infinite regress. Again, the escape lies in rejecting universal normativity or speaking about »truth«: ESK states that by means of an invisible hand, the way science is organised leads to positive (by consensus), unintended results. This can be applied to ESK science as well without the argument becoming circular, because the judgement of whether results are considered »positive« or not is an external value judgement that is independent from ESK.

138 E. g. parts of Heideggerian philosophy that are explicitly headed against this notion of rationality may be an example. See Carnap (1931), p. 231-232.

not necessarily the only justified »foundation«[139] for analysing science, the right question to ask is not whether or not an argument is circular, but rather whether the circle ESK provides is a vicious or a virtuous one.[140] The following example should make clear, that not every circular argument is deemed to be faulty or »vicious«. If every circle were vicious, it would be impossible to construct isochronal clocks, for example: Before we could build such a clock, we would need another one to test it. Similarly, it could be argued that there is a circular nature to building hammers, because you always need a hammer in order to build a new one.[141] Obviously, it is *not* impossible to build hammers or better clocks, and the reason is that the web of propositions involved is complex enough that each in some way supports the other, but there is no vicious circle. Rather, there is »a fruitful self-correcting feedback loop«[142] that can lead to continuous improvements. Self-applicability, which is given for ESK, is actually a good thing from this perspective. Therefore, it seems hard to find a vicious circle in analysing the behaviour of economists by economic methods, or (more generally) a methodology reflecting on its own rules.

In the imperialist, Beckerian sense the economic method, if skillfully adopted to the task at hand, is a theory of every kind of human behaviour[143] and in this sense should be applicable to science as well. There is no reason why ESK should be unable – due to some alleged circle – to criticise the way economics is organised. ESK has little to do with the actual *content* of a specific branch of science, as it only looks at the behaviour of scientists. The very idea behind this is purely pragmatic: ESK theorists do not seek a firm ground from which to assess economics and make prescriptions; the reasoning goes the other way round. First, they aim to understand the rules of the scientific game, and only then to make suggestions for reform that respect the status quo – everything else is considered to be hopelessly naïve moralising.[144] As Wade Hands has put it:

139 It seems strange that something that is so far from being settled as philosophy has ever been accepted as a foundation. For this reason, naturalist positions reject any kind of »first-and-fundamental philosophy«. From this perspective, it can be acceptable to start directly with economics as a »naturalising base«.

140 See Vollmer (1985), p. 217 et sqq. for the distinction between vicious and virtuous circles.

141 Vollmer (1983), p. 810-811.

142 Vollmer (1983), p. 801.

143 See Lawson (2004a).

144 Ken Binmore's perspective on ethics parallels this view: He argues that in ethics, too, it does not make sense to look abstractly for »the good«; rather, one should understand how certain equilibria that solve moral-dilemma situations came into being. With this knowledge, moral reforms could be proposed that have at least a chance of being effective. See Binmore (2005). As you can see again here, pragmatism implies that not normative philosophy but detailed analysis comes first.

> The general philosophical perspective on norms (ethical or epistemic) is essentially an usher's or central planner's view; the problem is to find the right rules so that agents who play by them will produce the right stuff.... For economists ... the question is more about emergence and sustainability.[145]

If you accept the foregoing arguments, the problem is not about ESK being circular, but rather about whether one accepts its results. In its current state, ESK provides more formal than empirical arguments, and there is a long way to go for having a thorough understanding of the institutional arrangements that form scientific behaviour. Accepting ESK is therefore more or less a value judgement about accepting formal economic arguments as conclusive. There are reasons for accepting them, but as Daniel Hausman[146] has pointed out, such reasons are rather a priori in their nature, because the core of economic theory is hard to put to the test due to its various ceteris paribus assumptions, which are hard do not match up with reality.

Another critical point concerns the practical value of ESK. Is there really much to it, or does it just tell us that competition is a good thing? As I said before, the whole project is very much in the beginning; for now, it is rather about reconstructing science than reworking it. In its current phase, ESK is more like a new paradigm for methodology than a »real« methodology on its own. This is reinforced by the fact that large parts of ESK do not care about single models at all. Whether this is a weak point or a strength is again a matter of the relevant problem: If one wants to criticise and understand single economic models, the institutional branch of ESK is not much of a help, but if the aim is improving economics or science in general, it seems a much more convincing approach. As the latter is probably the broader aim of the majority of methodologists and ESK *has* some (instrumentally) normative propositions to offer (even if in a very early state), it is undoubtedly a useful approach for economic methodology, even respecting all its restrictions.

The final point of criticism is directed against the use of Buchanan's consensus criterion and his contractarian theory as a basis for instrumentally normative judgements. Remember that the main idea is reconstructing a hierarchy of consensus in order to establish a normative basis. However, this concept has a shortcoming, which is common to all forms of contractarianism: The alleged consensus is never real. Even if the rhetoric stresses »methodological individualism«, one is forced to assume consensus when in fact there is none. The problem may be less harmful for philosophy of science than for social philosophy when the aim is legitimising constitutions by means of a hypothetical contract. Still, a fictive story about agreement is not

145 Hands (2001), p. 392. It might be added that philosophy is often not even about rules, but rather about how an ideal agent would behave.
146 See section 4.1.1.

likely to settle real methodological quarrels.[147] Hence, the contractarian vision is doomed to remain at the most general level, which is why it is usually not very helpful. Note again that attacks of this kind do not affect the instrumental version of ESK, which employs cost-benefit analysis in order to find the right means for given aims. Note, too, that Buchanan's theoretical consensus framework is, despite the practical problem of finding the hierarchy of consensus, quite convincing, as it does not presume any normative aim apart from what citizens or scientists can agree upon.

4.1.3.2. Lessons Learned

The upshot of this chapter should already be quite clear, so I will keep this short in order to avoid too much repetition: ESK does not offer an answer to the universal normative question about theory choice. It is rather the making explicit of the old pragmatist programme, as on the one hand it looks for *useful* (and not necessarily true) theories for solving problems by means of cost-benefit analysis and on the other hand evaluates the way science is organised in order to achieve this aim. In this way it provides a concretisation of pragmatist philosophy, as it does not only speak abstractly about »useful« theories and »efficient« organisation but puts some economic meat on the pragmatic bones.[148] If economists are the target group, such »economic meat« is much more likely to convince them than e.g. taking the standards of literary criticism for the evaluative basis as proposed by McCloskey. It is, however, important to note that ESK does not sneak the standards of empiricism in through the back door, as it allows scientific communities to set their own standards. As ESK is a meta-theory throughout, it is deemed to remain very general and therefore seems to lack substance. Note, however, that this can be seen as an advantage as well, because by shifting normativity to the meta-level ESK can escape the postmodern criticism against rule-based methodologies.

In the next section I will discuss in more detail some standards for theory evaluation in economics.

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147 Buchanan's idea of creating consensus is ultimately based on efficiency arguments. So even if he states that consensus comes first, it is the convincing force of proposing Pareto-superior (i.e. more efficient) institutional arrangements that are supposed to create the consensus. Even at the most basic consensus level, the acceptance of any form of state as superior to anarchy, Buchanan's arguments rely on this argumentative scheme. See Buchanan (1975), p. 5 et sqq. It is however doubtful if a Pareto-superior state would easily lead to consensus, because even if there is an improvement for everyone, individuals can still argue about distributional questions. See Aufderheide (1998). For a substantive comment on the limits of contractarianism see Kraus (1993).

148 See Hands (2001), p. 386.

4.1.4. Pragmatic Theory Evaluation

We have seen how some pragmatic approaches go beyond the dichotomy of empiristic philosophy of science and their postmodern counterparts. In this section my aim is twofold: First, I want to sketch out more fundamental arguments from philosophers or philosophically inclined economists that favour a pragmatic approach for theory evaluation and argue against both the old empiricist methodologies and the postmodern relativists. I will not give a detailed summary of the positions, but restrict myself to arguments that can be used for defending a pragmatic approach.[149] After that, I will turn to a more concrete discussion about criteria for pragmatically evaluating, criticising or defending economic models. A high degree of repetition is unavoidable in this chapter, but the main insights of this and the forgoing subchapters will be condensed to a consistent picture in section 4.2.

4.1.4.1. Pragmatic Views on Philosophical Problems of Methodology

Larry Laudan is probably the most prominent exponent of a methodology that tries to pragmatically overcome both positivism and relativism. His starting point is a philosophical attack against the foundations of many postmodern methodologists: Laudan argues in detail that incommensurability and underdetermination do *not* necessarily lead to relativism concerning scientific methods. Here are some of his arguments:

Laudan argues *against* the notion that the Duhem-Quine thesis makes the acceptance of *any* theory rational in the face of any evidence whatsoever – an idea often held by relativists, because the Duhem-Quine thesis allows for adjustment of auxiliary assumptions or background knowledge even to the extent of changing the meanings of words. Laudan admits that in this way it is *possible* to protect any thesis from falsification, but he denies that such a procedure can be called *rational*.[150] In most cases it is probably more rational to reject a thesis than to try to protect it, if it contradicts the evidence. According to Laudan, rational arguments in favour of a theory need to show much more than just the *compatibility* of a theory with the evidence. For example, the theory should *explain* the evidence as well, granted all the difficulties clarifying what that means, but Laudan states that fiddling with a theory in order to make it compatible with evidence that it formerly contradicted most likely reduces its explanatory power.[151] Laudan argues even more fundamentally against judging theories exclusively by their empirical consequences. He

[149] This approach can lead to misrepresentations of some positions, of course. I accept this disadvantage, as my aim is not representing positions but delivering arguments for pragmatism in theory evaluation.

[150] See Laudan (1996), p. 35.

[151] See Laudan (1996), p. 37 et sqq.

claims there are fundamental differences between a theory's (first) entailing the evidence as a consequence, (second) being supported by it and (third) explaining it. Laudan lists some cases where the data supports a hypothesis without being a consequence of it: For example. he states that the detection of Brownian motion supported the atomic theory without being a consequence of it.[152]

Next, he shows by a simple counterexample that there is a difference between logically entailing the evidence and explaining it: Every theory logically entails itself, but arguably no theory explains itself.[153] Similar to Friedman, Laudan argues that theories are made to offer solutions and *explanations to specific problems,* and that this is how they should be judged.[154] Therefore, even if Laudan grants underdetermination (in the sense that there is always an infinite number of theories that are logically compatible with the evidence), such logical compatibility is a far-too-weak criterion for choosing a theory – and that is why the underdetermination thesis does not offer a real argument for relativism. If one asks more of theories than just logical compatibility with evidence, namely explaining phenomena and getting evidential support from them, the Duhem-Quine thesis »cuts no ice whatsoever«.[155] There are other interpretations of the Duhem-Quine thesis that are much weaker, as they do not state that *any* theory can be rationally defended come what may, but merely that there are no criteria that can uniquely determine theory choice. This, however, is only a threat to the project of theory choice, if one expects the *unambiguous* choice of theories from it.[156] There has been much confusion between the strong and the weak versions of the Duhem-Quine thesis, and often arguments for the weak thesis – that choice between two theories is never determinate – have been used to argue for the much stronger thesis, which says that theory choice is completely ambiguous. As stated earlier, Thomas Kuhn argues that theory choice always involves subjective elements and is therefore never mechanically determinate.[157] But it surely does not follow from this that any theory is as good as any other.[158] Laudan lists a set of standards from which he believes that, even if it does

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152 See Laudan (1996), p. 35.
153 See Laudan (1996), p. 37.
154 See Deichsel 2009 (forthcoming) for a comparison of Laudan's methodology with Friedman's.
155 Laudan (1996), p. 38.
156 See Laudan (1996), p. 43.
157 See footnote 94 in chapter 2.
158 Again, a comparison to ethics might help clarifying the point: Even if in many cases it may be hard to decide between two ethical theories, it does not follow that murdering is equally as good as helping the poor or that any behaviour is as good as any other.

not mechanically determine theory choice, still restricts it in quite a substantial and rational way:

- Prefer theories which are internally consistent;

- prefer theories which correctly make some predictions which are surprising given our background assumptions;

- prefer theories which have been tested against a diverse range of kinds of phenomena to those which have been tested only against very similar sorts of phenomena.
  Even standards such as these have some fuzziness around the edges, but can anyone believe that, confronted with any pair of theories, and any body of evidence, these standards are so rough-hewn that they could be used indifferently to justify either element of the pair?[159]

It should be clear now that strong underdetermination of theory choice (»all theories can be rationally made compatible to any empirical data«) is probably wrong, and weak underdetermination (»empirical data always allows for more than one theory«) does not necessarily lead to relativism.

Now let's turn to Laudan's arguments against the thesis that incommensurability prevents the rational choice of theories. In philosophy of science it is nowadays widely accepted that complete reciprocal translation of theories is impossible. Laudan accepts this, but argues that full translatability is not a necessary condition for rationally comparing theories.[160] Laudan grants that rank-ordering theories by their verisimilitude requires translatability, but denies that such a ranking is the only way of rational comparison. For example, after settling for a more-or-less precise definition, standards such as »problem-solving ability, maximum internal coherence, simplicity, and minimum anomalies«[161] can be used for comparing theories without the need for object-level translation between the theories at hand.

Laudan grants the existence of Kuhnian losses[162], i. e. problems that were solved by the old theory and are not by the new one, but he still maintains that rational theory comparison is possible. First, Laudan denies that incommensurability is as widespread as followers of Kuhn often claim. According to Laudan, in many cases scientists have *rationally* adopted new paradigms

[159] Laudan (1996), p. 47.
[160] See Laudan (1996), p. 10 ff.
[161] See Laudan (1996), p. 11.
[162] Apart from arguing against incommensurability, Laudan heads an argument against Kuhn by observing that he cannot have both Kuhnian losses and full incommensurability between paradigms, because you need some comparison of theories to detect the Kuhnian losses in the first place. See Laudan (1996), p. 10.

and were quite able to assess their advantages. Of course, such decisions necessarily depend on scientific or epistemic values, but Laudan states that this is unavoidable and does not make the choice irrational at all.[163] In contrast to Kuhn, Laudan asserts that criteria such as internal consistency or predictive power often provided convincing arguments for new theories, which led to their quick adoption in the respective scientific communities. In Laudan's view, Kuhn and his followers have exaggerated the possibilities of dissent in science and hence neglected the cases in which standard criteria for theory choice gave rather unambiguous results.[164] In short, Laudan has shown that incommensurability is not omnipresent and can be overcome when employing standards that do not require reciprocal translation of theories. It is certainly not the case that most scientists dogmatically stick to their views until they die, especially when they are faced with convincing counter-evidence.

Laudan's own proposal for settling methodological quarrels accepts the fact that decisions are necessarily based on subjective values. He suggests a weighing process of the costs and benefits related to the weight and number of the problems a theory solves compared to the weight and number of the anomalies it has.[165] The counting and weighing of problems is, as Laudan admits, surely problematic, as it prevents a mechanical procedure and hence unambiguous outcomes. The proposal of such a procedure seems to therefore support a relativistic view rather than refute it, because there are no objective standards for weighing and prioritising. However, *methodological* relativism can be overcome, even if one grants that there is no way of objectively determining the weight or importance of aims. This leads of course to a restriction of methodology to purely instrumental rationality, but Laudan states that this is the most one can expect from theory evaluation: »Good reasons are instrumental reasons; there is no other sort.«[166] So Laudan accepts relativism when it concerns the *selection and weighting* of problems and goals. However, he proposes »normative naturalism«, as Laudan calls his meta-methodology, for finding the best *methods* to solve problems or achieve goals once they are selected. Determining the efficiency of methods for solving problems that are selected by the respective scientific communities is an empirical question for Laudan. His normative naturalism rests on the conviction that methodological rules can be tested for their efficiency to bring about the desired results by analysing the history of science.[167]

[163] See Laudan (1996), p. 92.
[164] See Laudan (1996), p. 93. In Donovan/Laudan/Laudan (1988), p. 14 et sqq. there are the results of empirical studies of scientific change which pretty much refute the thesis that theory choice is irrational or arbitrary.
[165] See Laudan (1996), p. 82.
[166] Laudan (1996), p. 178.
[167] See Laudan (1996), p. 131 et sqq. Again, such a procedure sounds cyclical at first, but as with any naturalist strategy, one could argue that the circle is virtu-

In the course of his investigations, Laudan contradicts Feyerabend's credo »anything goes«.[168] He accepts that Feyerabend found cases in which nonconformance to methodological rules have led to great advances, but he denies that this is enough for disproving that a set of rules was on average the best strategy for reaching certain desired aims. In this view, methodological rules are neither necessary nor sufficient conditions for progress towards an end. Hence, a methodological rule would be refuted not by some counter-instances of genially rule-breaking scientists, but only by showing that an alternative set of rules – or in Feyerabend's case, the absence of rules – would be more efficient in promoting a set of given goals. When methods are seen as means for solving problems, incommensurability loses its relativising force for methodology, because incommensurability of problems does not lead to incommensurability of methods for solving them. By taking problem-orientation seriously, Laudan frees science from any general values such as the »quest for truth«, but he is by no means committed to an engineering kind of science that is restricted to solving only practical problems. Relativism concerning problems allows for the most abstract problems conceivable and therefore does not reduce science to narrow-minded »toolbox-thinking«.

Bas van Fraasen's pragmatic theory of explanation provides additional arguments for a genuinely problem-oriented approach to theory evaluation.[169] Van Fraassen's first step is a redefinition of empiricism that he brands »constructive empiricism«.[170] According to this theory, the only goal of scientific theories is to be empirically adequate. Even if van Fraassen grants that certain theoretical claims may be in fact true or false, this does not matter for theory evaluation as long as the consequences of the theory fit the observations.[171] Accepting the weak underdetermination thesis, this allows of course for a plethora of possible theories, because for any given set of observations, there are infinitely many theories that are equally empirically adequate. Van Fraassen would accept this, as he strictly separates the pure, epistemic values of science from the pragmatic ones. The epistemic value of creating em-

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ous rather than vicious. See section 4.1.3.1 for a longer discussion of this point. Laudan's testing of methodologies does not presume a »final, correct way of testing«, it merely claims that it is an empirical question whether a methodology is a good means for the ends it promotes.

168 See Laudan (1996), p. 101 et sqq.

169 The sketch of van Fraassen's theory is particularly short, as the basic ideas are presented again in the discussion of Boylan's and O'Gorman's causal holism later in this section.

170 See van Fraassen (1980) on nearly every page of the book.

171 Note how well this fits the as-if assumptions in economics: Economists grant they may be true or false, but they do not care about this, as long as the outcomes of the theory are empirically adequate. This *allows* for realism concerning scientific theories, but *denies* the relevance of such questions for science, which is, again, a pragmatic way out of an old dichotomy.

pirically adequate theories is, according to van Fraassen, the only *goal* of science, but that in no way excludes the employment of pragmatic criteria such as simplicity or explanatory power for theory *selection*.[172]

Interestingly, explanation is not a genuine goal of pure science for van Fraassen; it is rather a side effect of the *application* of pure science.[173] This follows straightforwardly from his assertion that finding empirically adequate theories is the *only* goal of science. However, this does not mean that explanations are unimportant. Van Frassen's theory of explanation solves an important problem of the classic DN-model of explanation. The DN-model's basic idea – that providing an explanation for a fact means deducing it from some given facts plus a natural law – is problematic, because of so-called explanation asymmetries such as this: The length of a tower can be deduced by the length of its shadow plus the position of the sun plus some geometric operations. However, we would not agree that the length of a shadow can *explain* the length of a tower, even if we can deduce it from a natural law plus some given facts.

Van Fraassen set out to overcome such problems with his pragmatic theory of explanation. At the basis of this theory is the idea that explanations are essentially context-dependent; he characterises them as answers to *contrastive* why-questions. Every event allows for many different why-questions that require different answers or explanations. Take, for example, a car crash: There are many possible why-questions to ask, and each one requires a different explanation: Why did the driver crash the car rather than driving safely on normally (probably a psychological or medical explanation required)? Why did the metal of the car bend and not break (mechanical explanation required)? Why was the street narrow rather than wide (political explanation)? This could be continued forever, and it shows that explanations cannot just occur between theory and data but need context, viz. a specific, additional why-question.[174] Explanations are therefore a part of the pragmatic side of the scientific coin. This view offers interesting new aspects for theory evaluation and enables us to escape both positivism and relativism: Theories can be judged according to their ability to answer certain why-questions and, according to van Fraassen, this is *independent* from their epistemic quality: »even what part of … background information is to be used to evaluate how good the answer is … is a contextually determined factor«.[175] To clarify the difference between pragmatic and epistemic aspects, van Fraassen often stresses the explanatory difference of being a daughter and being a woman – which is pragmatically *not* the same, even if every woman is a daughter and

172 See van Fraassen (1980), p. 97.
173 See van Fraassen (1980), p. 157.
174 See van Fraassen (1980), p. 156.
175 van Fraassen (1980), p. 156.

every daughter is a woman, so that there is no epistemic difference between the two terms.

Van Fraassen's twist pushes many of the relativists' attacks to the pragmatic side, where relativism is not that big of a problem, as context dependency is easily granted here. Of course it is impossible to determine objectively what questions we should ask. But his constructive empiricism does not lead to »anything goes« when it comes to theory evaluation. Note that constructive empiricism does not require a defence of pure science as a neutral description of the world. Pure science is about constructing models that are empirically adequate to the phenomena they refer to. Van Fraassen introduces a crucial distinction between »observing« and »observing that«, and he offers arguments that only »observing that« is affected negatively by theory-ladenness: »Suppose one of the Stone Age people recently found in the Philippines is shown a tennis ball .... From his behaviour, we see that he noticed them .... But he has not seen *that* it is a tennis ball«.[176] But even »observing (without 'that')« is not neutral or objective for van Fraassen: Observation is of course observation-for-us and not neutral observation.[177] So while he grants the postmodern point that neutral observation is not possible, he does not subscribe to the far-reaching postmodern relativism and holds that some quality standards are well applicable for human beings.

Avoiding the danger of digressing too far from *economic* methodology, I turn to the work of Thomas A. Boylan and Pascal F. O'Gorman, who have developed a pragmatic methodology for economics which they call »causal holism«. The sense of the foregoing philosophical »digressions« becomes clear when one identifies their methodological framework as a synthesis of Quine's holism with van Fraassen's constructive empiricism. Like van Fraasen, Boylan and O'Gorman put explanation entirely to the pragmatic side of economics (which they call applied economics) and see the epistemic aim of pure economics only in delivering adequate descriptions.[178] Following Quine they accept that theory-free description is impossible, as theory and description are inseparably linked, which leads to an antifoundationalist picture in

176 van Fraassen (1980), p. 15.

177 However, theory-ladenness can even occur on an intersubjective level, e. g. scientific training can enable a person to see differences in objects where an untrained person does not see them. Observation-for-us therefore does not entail the claim that all humans see the same things, but allows for the qualification »under optimal epistemic conditions«, which surely includes scientific training. See van Fraassen (1980), p. 17-19. Nonetheless, the distinction between observables and unobservables is notoriously hard to draw. See Klee (1997), p. 299 et sqq. This problem will be dealt with more closely in the following section and especially in section 4.2.1.

178 Note that this does not contradict any interpretation of the *interests* and desires of scientists. See Boylan/O'Gorman (1995), p. 162.

which every statement is evaluated by how it is connected to other convictions in an overall web of belief. To give a short example, even the simple descriptive statement »the lower the price, the higher the sales (c.p.)« involves theoretical assumptions and cannot be observed neutrally or directly.

Since causal holism sets descriptive adequacy as the only epistemological goal, it suspends judgement on the reality of unobservable theoretical entities and on the truth-status of theories in general.[179] Boylan and O'Gorman accept many of van Fraasen's conclusions, but there are important differences. A highly crucial point is, of course, the definition of »observable«.[180] In the section »Causal Holism and Descriptive Adequacy« the authors extend the notion of »observability« even to causality. They argue that the recognition of causes does not differ in principle from the recognition of complex objects – they are learned in the same way.[181] Further, causal holism contradicts constructive empiricists in the definition of empirical adequacy. It rejects van Fraassen's stance that observability is a theory-independent question.[182] For causal holists observation (without »that«) cannot be theory-independent, and hence empirical adequacy is a highly complex issue that involves a holistic evaluation of the competing models and their descriptive resources[183] – the »logical notion of isomorphism fails to grasp the rich complexity of empirical or descriptive adequacy which results from its embededdedness in 'historical' time«.[184] Accepting this, it is hard for causal holists to define a standardised method for evaluating the descriptive adequacy of models. But the main point is a negative one: Causal holists refrain from speaking about the »essence« of entities. An example from economics will help to clarify the point: Causal holists would not agree with the realist's interpretation that maximising revenue is the »essence« of firms – causal holists would not care about the essence as long as a model is empirically adequate.[185] This view leads to the rejection of unification as a normative goal

[179] However, like van Fraassen, causal holists agree that models literally say something about unobservable mechanisms which are true or false. See e. g. Boylan/O'Gorman (1995), p. 202.

[180] See Fleetwood (2002) and Boylan/O'Gorman (2006) for a detailed discussion of the status of unobservables in causal holism. Fleetwood, as a realist, sees an essential tension between causal holism's ontology (which grants the existence of unobservables) and its epistemology (which suspends judgement on them). Boylan and O'Gorman defend their position against this charge by stressing the dangers of a realistic interpretation of the ontology of economic theories.

[181] See Boylan/O'Gorman (1995), p. 163 et sqq.

[182] See van Fraassen (1980), p. 57.

[183] See Boylan/O'Gorman (1995), p. 168.

[184] Boylan/O'Gorman (1995), p. 166.

[185] See Boylan/O'Gorman (1995), p. 175.

of economics and establishes theoretical pluralism as more than an interim position.[186]

Apart from philosophical subtleties, the most palpable impact of causal holism is its position towards holistic learning from experience, which extends Hausman's discussion of ceteris paribus clauses in economics. Remember Hausman's argument that the ceteris paribus assumptions are probably the weakest links in economic theory, which protects the theoretical core from empirical falsification and makes learning from experience almost impossible.[187]

From the perspective of causal holism, this would exclude economics from the domain of science.[188] Boylan and O'Gorman argue (again referring to Quine), that more holistic testing is required, which means that the theoretical core of economics is not privileged over the ceteris paribus assumptions. This shows that causal holism rejects Hausman's interpretation of economics, which consists of accepting the core of economics by means of common-sense realism.[189] As causal holism refrains from realistic interpretations, Boylan and O'Gorman insist that learning from experience must be possible for any part of theory – including the theoretical core. This seems prima facie desirable, but following Lakatos and Kuhn the protection of a theoretical core and the resulting defence of paradigms or research programmes can lead in the long run to changes of the core as well, if an old paradigm or research programme is substituted with a more promising one. This means that »learning from experience« does not necessarily have to take place directly at the level of the theoretical core of neoclassical economics but can emerge over time through the arrival of new paradigms or research programmes in the normal course of science.

Apart from the above point, which shows why protecting some parts of theories can be reasonable, Boylan and O'Gorman do not explain what working »on a ship at sea« – where no part is protected from the bar of experience – could look like. So one could say that they reject relativism, but their normative concept needs further refinement.

All three conceptions presented in this chapter share the commonality of accepting the postmodern insight that science cannot be built on firm ground; yet they are convinced that this neither makes science an irrational enterprise nor leads to an »anything goes« relativism. Laudan, van Fraassen and Boylan/O'Gorman develop concepts for empirical research that allow

[186] See e. g. Boylan/O'Gorman (1995), p. 177. Again, Boylan and O'Gorman are following Quine in this. See Boylan/O'Gorman (2003), p. 4. Note that Caldwell, as a pioneer of pluralism in economic methodology, cautiously called pluralism an interim position. See footnote 95 in chapter 3.

[187] See Hausman (1992a), p. 307.

[188] See Boylan/O'Gorman (2003), p. 9.

[189] See Boylan/O'Gorman (2003), p. 17.

for relativism concerning the problems one deals with, but not for relativism when it comes to the assessment of the solutions: Laudan's important move was to stress the problem-solving capacity of theories as the primary criterion for their quality; van Fraasen overcame difficulties of traditional empiricism through his strong distinction between epistemic and pragmatic aspects of theories; and Boylan/O'Gorman tried to apply a blend of Quine's holism and van Fraassen's constructive empiricism to economics.

Alas, all three conceptions discuss matters on a fairly abstract level and are (even in the case of Boylan/O'Gorman) not easily applied to economics. This is the reason why the next section deals more concretely with the understanding and improving of model building in ecomomics.

4.1.4.2. Reflections on Model Building in Economics

How can models be justified that are full of counterfactual assumptions? In which way do economic models help us to understand what's going on in the real world? How do – and how should – economists actually learn from the bar of experience? I will briefly discuss several pragmatic approaches to these questions in this section. All authors presented in this section have normative positions about how to improve or assess economic theory, but they don't call dogmatically for more empirical work at the core of economics, nor do they demand tougher falsification. In this sense, all contributions are small steps towards a solution for the dichotomy of (dogmatic) empiricism and postmodern relativism in economic methodology.

4.1.4.2.1 Justifying Idealisations

An important field for discussion is the role of idealisations in economic theory. If strong idealisations could be justified, this would be a major difference from the empiricist claims discussed above.

Before discussing idealisations it is important to stress some fundamental aspects of economic model building and how it helps promote learning from the »bar of experience« in a holistic fashion. As always, I will restrict myself to theoretical models, leaving the econometric models aside. Economic modelling can be defined as the *idealised* reconstruction of essential features of economic systems.[190] It is important to note how the process of model building actually works. Obviously there are different approaches,[191] but they all have one important point in common: They acknowledge that models are not merely simplifications of the real world. Modelling is instead a creative process in which functional mechanisms are *suggested* and not merely extracted from what we observe in the world.

190 See Frigg/Hartmann (2006) for an excellent overview of the role of models in science.

191 For a nice and short example of a »model-building handbook« see Varian (1997).

Mary S. Morgan has argued that learning from models is a creative process that happens at two stages of model building: the construction and the manipulation of the model. In her view, models are not passive mathematical structures but always involve a »story« that relates them to the world;[192] models are like instruments that need to be used or manipulated if we want to learn something from them.[193] These arguments about the particularities of economic building show once again why we must go beyond traditional empiricist standards when doing theory evaluation in economics.

Additionally, many features of models are introduced for convenience and better mathematical tractability even if they contradict empirical knowledge.[194] Therefore, economic models are not only necessarily incomplete but are also based on counterfactual assumptions. These assumptions are mostly characterised as idealisations that stress the essential features of the relevant problem. Now the question arises of how the use of idealisations can be justified. To answer this, it is important to note that idealisations are common even in the »hardest« natural sciences like physics – at the very least since Galileo Galilei's free-fall experiments. Interestingly, idealisation on the assumption-side in physics was linked with an increase in predictive power.[195] As I will show in section 4.2.1.2, idealisations are not a sufficient reason to refrain from a realistic interpretation of economics. In fact, idealisations are necessary for any form of theorising; without them we would be trapped in the messy world of photographic description. Idealisations help to unify observations under a theory that would otherwise have no connection to each other. In short, learning from experience requires theorising and theorising requires idealising. Of course there can be losses in empirical adequacy due to idealisations, but if one looks at the degree to which economists employ idealisations in their models, it seems clear that predictive accuracy is not the only aim of their enterprise.[196] For example, a game-theoretic reconstruction of patent races as delivered by Etro 2004[197] makes – as do most game-theo-

192 See e. g. Morgan (2001). Morgan builds on ideas first mentioned in Gibbard/Varian (1978).

193 See Morgan/Morrison (1999), p. 10 et sqq. As I am not primarily concerned with a *characterisation* of models, but rather aim at finding a new balance for theory evaluation and hence a *justification* for the use of (false) economic models, I will leave the topic with these short remarks.

194 An example are models that assume one-product firms in an atomistic market.

195 See Hüttemann (1996), p. 91.

196 Hüttemann even takes the use of idealisations to be an argument against van Fraassen's dictum that empirically adequate prediction is the aim of science (See Hüttemann (1996), p. 127). I would not go that far, because some idealisations (as in Galilei's case) can lead to higher empirical adequacy.

197 A more detailed discussion of this paper will be given in section 5.1.3.

retic models – at best only vague tendency predictions which are nowhere tested for their empirical adequacy.

Nancy Cartwright has argued that idealisations are necessary for finding interesting mechanisms so that the highly abstract and hence descriptively unrealistic nature of neoclassical economic modelling is not a disadvantage as such. Cartwright characterises neoclassical models as thought experiments and makes clear that all thought experiments rely on idealisations or false assumptions. However, according to Cartwright the idealisations in economics are not of a helpful or »Galilean« kind as they are tailored to facilitate mathematic tractability and therefore do not merely isolate relevant features but add additional restrictions to the model, which makes the results highly dependent on them and reduces their applicability.[198] However, she offers no proposals for what better idealisations would look like.

4.1.4.2.2 Learning from Models

Marcel Boumans refers to Cartwright and deals with the question of how learning from experience is possible in economics. His proposal goes against the classical ceteris paribus laws of the empiricist tradition: Boumans argues that the traditional notion of ceteris paribus laws is ill-suited for the social sciences, as here the conditions are never constant and especially hard to control. Therefore, economists should instead look for general relationships that »continue to hold … as various other conditions change.«[199] This shifts the focus from searching ceteris paribus laws to investigating the domain for which a relationship holds. Hence, Boumans refers approvingly to Friedman's approach where specifying the domain is an integral part of a hypothesis that must be empirically enquired.[200] In that way one is much more likely to find relations that are actually useful under real-world conditions. Clear specifications of domains where relationships hold independent from disturbing factors can lead to decomposing the complex economic system into a hierarchy of subsystems »where the elementary units … are bound only by simple relationships. Their simplicity implies that they probably represent autonomous relationships.«[201] This presumes, of course, that fixed simple patterns create the complexity of economics by their interdependence. How can the simple structures that underlie complex system be untangled if the ceteris paribus condition never holds and controlled experiments are not

[198] See Kaldor (1978), p. 202 for a similar point. See Cartwright (2002), p. 11 & p. 15.

[199] Boumans (2003), p. 310.

[200] See Boumans (2003), p. 318-319. On page 328 Boumans concludes that it is not the domain of negligible changes that should be investigated, but rather the domain of the phenomena that can be successfully predicted.

[201] Boumans (2003), p. 325.

possible?[202] Again referring to Nancy Cartwright, Boumans discusses »ceteris neglectis regularities« as a possible substitute: invariant relationships could also occur if the influence of the environment is negligible. The problem is, however, that highly autonomous relationships are in most cases rather imprecise or last only for short timespans.[203] Therefore, Boumans agrees with Friedman's recommendation to not look for general models with high autonomy but to investigate for which phenomena a model holds. Boumans concludes that strictly controlled experiments are not necessary for finding lawlike relationships in economics.[204] However, as explained above, he agrees that this may be possible only in small, restricted domains. Still, Boumans points to a way out of relativism without going back to the traditional empiricist notions.

Gibbard and Varian offer an interesting perspective on theory evaluation in economics that directly contradicts the empiristic picture and proposes a new standard: In an article where they characterise most economic models as »caricatures«, they argue that the point of these economic models is not so much accurate prediction but rather »emphasizing – even to the point of distorting – certain selected aspects of the economic situation«.[205].

If one accepts that models are caricatures that are not only based on false assumptions but also make false and intentionally distorted predictions, it becomes harder to justify the use of idealisations (though it is still possible). Gibbard and Varian argue that simple models are needed for generating results that can be meaningfully reduced to the assumptions, because models that bring in many and countervailing economic forces tend to bury their effects, i. e. can provide no explanation why a certain result has been obtained.[206] When the results of caricature-models are robust to changes in idealisations and auxiliary assumptions, this can increase our trust in the models.[207]

In a recent paper Hindriks has further reflected on the puzzling idea that it is possible to explain by employing »false« models.[208] The descriptive side of

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202 This refers of course to macroeconomics and does not deny the possibility of experiments in the domain of individual decision making.

203 See Boumans (2003), p. 326.

204 See Boumans (2003), p. 328.

205 Gibbard/Varian (1978), p. 665.

206 See Gibbard/Varian (1978), p. 673.

207 See Gibbard/Varian (1978), p. 674. Alexander Rosenberg criticises this point and states that the explanatory power of a theory is actually *decreased* if »a wide variety of equally plausible alternative theories« (Rosenberg (1989), p. 67) is available. I think he misunderstands the term »robustness«, which refers to varying the assumptions of one theory and not comparing structurally different theories. See section 4.1.4.3 for a more detailed discussion of the robustness criterion.

208 See Hindriks (2008). I adopt Hindriks' talk of »false models»only for convenience; I still accept Hausman's view that the models themselves are predicates that

his analysis is similar to Gibbard and Varian: Hindriks accepts that not only the assumptions but also the implications of economic models may contradict our best knowledge. However, if they are »false« in the right sense they can be very fruitful »explanatory engines«. Hindriks draws – again – an analogy to Galileo Galilei's law that all bodies fall at the same speed in a vacuum. Here, the assumption of bodies falling in a vacuum is false, and the predictions do not hold in the real world, either: A feather falls slower than a stone outside a vacuum. Hindriks compares this to the Miller-Modigliani theorem, which states that the way a firm is financed is irrelevant to its value in a world where finance markets operate perfectly. Again, the assumptions *and* the predictions are false: finance markets do not operate perfectly, nor is the way a firm is financed irrelevant to its value. However, as in Galilei's case, the theory behind such models *can* be counterfactually true. Hindriks argues that good models are »explanatory engines« because they help to ask contrastive why-questions in van Fraassen's sense. They provide a background foil for further research and learning from experience.[209] The unrealistic assumptions and the resulting unrealistic predictions are »the contrast relative to which a certain fact is to be explained«[210]. The explanations of different questions are delivered by relaxing assumptions: in Galilei's case the vacuum-assumption and in case of the Modigliani-Miller theorem the perfect-markets assumption. This does not contradict Friedman's methodology, as he would recommend changing the assumptions, too, if they lead to false predictions. The relaxation of assumptions of the Modigliani-Miller theorem led to important contrastive questions such as »Why is leverage relevant to the value of a firm rather than being irrelevant?«[211], »Why do firms retain some of their equity rather than relying on debt only?«[212] and »Why do firms opt for this particular equity-debt ratio rather than another one?«[213]. This suggests what Hindriks means by calling false models »explanatory engines«: A possibly counterfactually true theorem that delivers false predictions because it is based on false assumptions can stimulate a fruitful line of research and generate (possible) explanations by implicitly suggesting the relaxation of assumptions. The stimulation of questions is a new normative aspect for theory evaluation that lies completely beyond the empiristic picture.

Vilks adds another argument in favour of models based on empirically false assumptions.[214] In his view, models based on false assumptions can

cannot be actually »false«. See section 4.1.1.

209 See Hindriks (2008), p. 338&340.

210 Hindriks (2008), p. 342.

211 Hindriks (2008), p. 352.

212 Hindriks (2008), p. 355.

213 Hindriks (2008), p. 355.

214 See Vilks (2002) The falsity of assumptions refers of course to assumptions that are known to be false in other contexts or are known to be causing false predic-

sharpen our intuition about relationships in economic systems, as they foster tacit knowledge in a way similar to the study of fictive case studies. In his view, economic models do *not* describe the reality »essentially correctly« and do *not* deliver knowledge about the economy, but serve rather as a training instrument that shapes the worldview of economists.[215] However, Vilks stresses that it is not at all clear which models should be studied and to what degree this should be done, as adverse effects of dealing with formal models are evidently conceivable as well. The relation between models and the real world consists, in his view, of a kind of family resemblance: Even if no part of the model refers to reality – neither the assumption nor the predictions – there still is a similarity. Vilks argues that learning and understanding always works this way for humans – our brains are not made for mechanically deducing from theories; rather, they learn in a creative, parallel and connective way.[216]

Eric Schliesser extends this discussion and sets out how theory can be an engine for empirical discovery.[217] The course of his arguments shows that empirical confirmation is probably not what we should expect from economic theory; rather, we should concentrate on how we can improve it. This is again a normative proposal that goes beyond the classic empiricist ideas.

According to Schliesser, some theories enable measurements that can lead not only to confirmation or disconfirmation of theories but as well to the generation of new theories.[218] His thesis is that those theories which enable measuring the »nature, scope, and robustness of a theoretical constant«[219] are particularly good engines for new theories, as they enable scientists to learn from the shortcomings of the current theories by generating interesting empirical research questions. The Duhem-Quine thesis and the resulting difficulties of testing complex and uncontrollable systems are for Schliesser only problematic when justification is the aim of enquiry.[220] The forward-looking activity of science is not undermined, and even if falsification is made impossible by the Duhem-Quine thesis, disconfirmations in no way hinder the refinement of theories or the development of new ones. The crucial question is, therefore, how empirical research can be guided by economic theories in a way that helps to improve the theories themselves. Schliesser argues that the search for constants could be a useful regulative idea for this.[221] I agree in part, but as I will propose later in this section, the search for constants is

tions. Friedman's argument that the falsity of assumptions cannot be known beforehand is hence rejected here.

215 See Vilks (2002), p. 25.
216 See Vilks (2002), p. 27.
217 See Schliesser (2005).
218 See Schliesser (2005), p. 51.
219 Schliesser (2005), p. 65.
220 See Schliesser (2005), p. 67.
221 See Schliesser (2005), p. 71.

not enough for guiding research – a research programme such as the heuristic interpretation of rational choice is also needed in order to make meaningful empirical research possible in economics, and this will be discussed in the next subsection.

All the discussed approaches to economic modelling try to justify – in a pragmatic way that takes the peculiarities of economics into consideration – the use of unrealistic models for help in understanding the real world. Thus they are neither empiricist nor relativistic.

4.1.4.2.3 Rational Choice as Heuristic

The last approach to model building presented in this section is, in my opinion, by far the clearest and most complete pragmatic justification for economic modelling: Homann and Suchanek justify the rational-choice approach by calling it tautological and a mere heuristic.[222] In this interpretation it actually loses any direct empirical content and therefore cannot be called »unrealistic« anymore.[223] This is the main twist of the heuristic approach: What was called unrealistic before is now assumed to be a heuristic principle without any ontological relevance. From this perspective, the important question is no longer whether or not economic explanations are »realistic« but only whether or not one can find an economic reconstruction of some observed behaviour that delivers an explanation in terms of individually perceived costs and benefits – it doesn't matter if those factors »really« motivate human beings or not.

Economic modelling means, then, to unravel hidden costs and benefits and to construct motives and incentives that underlie a given situation.[224] The empirical side of economics does not consist of its basic model of rational behaviour but of the description of situations in cost-benefit terms. The underlying rational-choice modelling scheme is not built on empirical research, nor is it considered falsifiable; it merely delivers a perspective on human behaviour.

Many critics have argued that this framework is too narrow for understanding human behaviour. Homann and Suchanek reply that every science

222 They admittedly develop their position as an extension of Popper's view of the rationality principle, as discussed in section 2.3.1.1.

223 Notice the proximity to Gary Becker's approach, discussed in section 2.4.2.1. Homann and Suchanek deliver methodological reasons for justifying this approach. Needless to say, they are not alone in doing this. Similar arguments can be found e. g. in Kirchgässner (1991), Vanberg (2002) Vanberg or Pies (1998).

224 As always, this refers to situations in which many individuals interact and not to the decision procedures of single individuals. Knowledge of average preferences is nonetheless necessary for analysing the behaviour of collectives: E. g. if the assumption that most people prefer more money to less was incorrect, economists could hardly make any predictions.

has a method and as a result, a narrow perspective on the world (e. g. medicine cares only about how to cure diseases); so an alleged narrowness is not a disadvantage as such, but rather the opposite: specialisation and division of labour is as necessary for scientific progress as it is for the efficient organisation of modern societies. Fruitful interdisciplinary research requires clearly separated and well-defined methodologies.[225] Put in a paradoxical way, the heuristic view of the economic man encourages pluralism and interdisciplinary research in economics by being strictly dogmatic concerning the methodological basis.[226]

The defence of rational-choice modelling is presented along pragmatic lines: Complexity reduction is always necessary, and for the solution of some problems it is often *pragmatically* most convenient to reduce them to rational-choice problems: A search for »truth« does not tell us where and how to reduce complexity, because it is far too general – every detail is important when looking for »the truth«. Applying the rational-choice methodology to a given problem guides the researcher to reduce complexity.[227]

Homann and Suchanek *define* economics by its method and thereby avoid fundamental criticism of it: If economics is bound to rational-choice explanations by definition and if it is successful with *some* applications, it is methodologically justified, in the sense that it cannot be rejected as being »unrealistic and therefore useless«. Once this is accepted, the central question has shifted from »is the rational choice approach justifiable?« to »which problems can be solved by the rational choice approach?« In other words, the central methodological matter is now the *fruitfulness* of the rational-choice approach, not its intuitive appeal or realisticness. With their heuristic interpretation of the economic man, Homann and Suchanek take the view that the rational-choice approach can be fruitfully applied even where it is seemingly falsified. They, too, refer to Galilei's laws of falling bodies and compare them to fundamental laws in economics.[228] As Galileo »saw« his laws at work *everywhere* and attributed the differences of falling speed to variances

225 See Suchanek (1993), p. 3. Homan and Suchanek stress that that there is no well-defined *domain* for economics but rather that the findings of other social sciences can be integrated into economics as additional restrictions. See Homann/Suchanek (2000), p. 395 et sqq.

226 Hausman sees this dogmatism as a valid point for criticism. See Hausman (1992a), p. 235. Homann and Suchanek disagree, because their view on rationality is completely open, as it allows for the inclusion of *any* costs and benefits, even purely psychological ones. In this way the results from other sciences can be integrated into the economic approach.

227 See Homann/Suchanek (2000), p. 341.

228 In particular, they take the law of falling demand and the dominance of defection in prisoner's-dilemma situations as examples for fundamental economic laws. See Homann/Suchanek (2000), p. 344.

in boundary conditions, Homan and Suchanek see social-dilemma situations like the prisoner's dilemma *everywhere;* and if people sometimes cooperate and sometimes don't, this does not affect the general theory but should be explained by unravelling the different institutional settings.[229] This fits perfectly to Hausman's description of economics as a separate science, one where the theoretical core is protected. Note that this does not make it a narrow science, because the economic method can be applied to a plethora of distinct problems and is by no means restricted to an »economic domain«.

Homann and Suchanek defend their view against several critiques that have been headed against rational-choice reasoning. They reject Simon's view that modelling individuals as »satisficers« would be superior to modelling them as »optimisers«, because they see satisficing as an optimisation process that includes the search costs. Optimising is a more general approach that can integrate search costs when necessary.[230] In a similar vein, Homann and Suchanek reject Simon's concept of bounded rationality: While they accept that humans do not have complete knowledge, nor do they react at unlimited speed, they again recommend attributing these facts not to the basic rational-choice model but to the restrictions, if they are relevant for the problem under scrutiny. In Homann's and Suchanek's view, economists should concentrate on modelling the *results* of human behaviour, not on modelling the decision process. Consequently, they reject attacks from experimental economists that are headed against rational-choice modelling, too. The heuristic rational-choice approach is committed to the view that humans somehow react *systematically* to changes – otherwise, no economic theory is possible. Where experimental economists search for deviations from the classic narrow interpretation of rationality, Homann and Suchanek recommend looking for (rational) reasons of such deviations.[231]

It should be clear by now that the heuristic interpretation of the economic man is very liberal and does not in principle prefer monetary incentives to others (such as psychological ones). This is one reason why they reject Buchanan's defence of the economic man as a worst-case scenario.[232] Another reason is that they do not see the traditional homo oeconomicus to be a worst-case scenario, but a rather realistic one, at least in competitive situations or when a »bad guy« forces other participants into defection. This makes Homann's and Suchanek's view somewhat inconsistent, as they first stress the broad interpretation of »utility« but at the end of the day adopt the traditional view, which excludes e. g. true altruism.[233] This reveals a fun-

229 See Homann/Suchanek (2000), p. 358.
230 See Homann/Suchanek (2000), p. 364.
231 See Homann/Suchanek (2000), p. 367.
232 See Homann/Suchanek (2000), p. 371 and footnote 222 for a short remark on Buchanan's view.
233 See Homann/Suchanek (2000), p. 372-373.

damental lack of clarity in their position: Do they see the homo economicus as an empirically grounded model, or not? For the most part, they stress the heuristic character of rational-choice models and stress that economics is *not* a realistic theory of human behaviour – and should not be.[234] At other times their argumentation seems different, such as when they state that given situations and their incentive-structure *really* motivate individuals.[235] As long as such statements are not substantiated with good arguments – which is not the case in the rare[236] occasions Homann and Suchanek make them – it would be philosophically wiser to suspend judgement on knowledge of the »reality« of human motivations.

Köllmann has discussed the position of Homann and Suchanek, and while generally favourable of their view, he criticises their lack of justification.[237] It is certainly true that Homan and Suchanek often write as if any other methodology besides the rational-choice paradigm is »beside the point«. The justification of the approach remains implicit: Similarly to Laudan, Homann and Suchanek acknowledge that while their methodological perspective (seeing rational choices everywhere) is something pre-empirical, the *success* of this pre-empirical heuristic is an empirical matter. If there were other heuristics that could provide better solutions to the economic problems Homann and Suchanek are interested in, they would substitute the economic-man methodology.[238] If this is granted, dogmatism (in the sense of Kuhnian normal science) can be actually a good thing, as it leads to detailed research and tough competition. As Köllmann points out, Homann and Suchanek sometimes do not seem to acknowledge this when they call many criticisms of the rational-choice approach »beside the point«: if every change of the core of economics was »beside the point«, amendments like game theory, institutional economics or the inclusion of search costs into the optimisation process would have never been made.[239] Therefore, it would be more honest if Homann and Suchanek would admit that some suggestions are beside *their* point of reconstructing human interactions as dilemma-situations of rational egoists.

So while the defence of rational-choice modelling that Homann and Suchanek deliver is persuasive, they overemphasise the strengths and correspondingly neglect the weaknesses of this concept. Their argumentative twist is binding economics to the rational-choice approach by definition, so that problems in need of a different approach are assigned to sciences distinct

234 See e. g. Homann/Suchanek (2000), p. 376 for an explicit statement.
235 See Homann/Suchanek (2000), p. 371.
236 Since I found only the one referenced above, it is perhaps fairer to attribute the inconsistency to carelessness rather than calling it a fundamental error.
237 See Köllmann (2001), p. 32.
238 See Homann/Suchanek (2000), p. 378.
239 See Köllmann (2001), p. 16.

from economics; but whereas rational choice, in the broad interpretation that Homann and Suchanek apply, is surely well-suited for many problems such as analysing the incentive-structure of different institutional settings, it inherently lacks e. g. the capability for predicting the *amount* of the effects.[240]

This section on model building in economics has covered a lot of ground and presented many methodological positions that neither follow the empiricist strand of chapter 2 nor the relativism of chapter 3 but instead propose new criteria that offer a pragmatic way out of this dichotomy. While there is much to be learned from the rather scattered methodological remarks discussed here, a broader framework for theory evaluation is still lacking; this will be presented in chapter 4.3. This framework will be applied to four case studies in order to show how economic models can be fruitfully criticised.

But first, the next section gives an overview of traditional criteria for theory evaluation and how they can be applied to economics.

4.1.4.3. Some Criteria for Theory Evaluation

After having mentioned some criteria for theory choice in the preceding sections rather unsystematically, here is a more systematic summary of criteria by which theories can be evaluated. At this time, at least two points should be considered to be beyond question:

1. There *are* criteria for theory choice. Relativism is plausible only at the most global level of problem- or goal-selection. As soon as a problem is given, it is possible to assess the quality of possible solutions in a rational (even if not a mechanical) way.
2. Empirical adequacy is not the only criterion for evaluating theories. While empirical adequacy *is* important, claiming a more empirical science of economics is often of little help and can even be confusing when appraising economic models.

While the discussion in the foregoing chapter concentrated on the peculiarities of economic modelling and how to justify it in ways that differ from traditional empiricist standards, I will now present criteria to evaluate economic models or theories.

Of course, the acceptance of such criteria rests ultimately on values that gain their status through their endorsement by the scientific community.[241] In that sense, relativism is granted: there could be values other than the ones

[240] Models based on rational-choice approaches need to be calibrated if they are to be useful for prediction. Such calibration sometimes includes chance, noise and other errors that cannot be reconstructed as rational behaviour.

[241] See Kuhn (1977). Kitcher is arguing along similar lines when he discusses the similarities between ethical and scientific revolutions. See Kitcher (2008). Carrier argues in a recent paper that empirical underdetermination combined with the ac-

suggested by the criteria listed below – take again praise of god as a radical counter-example. A list of criteria for scientific theories is therefore rather an explication of the values most scientists agree upon than a normative definition of what science should look like.[242] But if our notion of science is more or less fixed, the listed criteria are both explications of scientific values *and* criteria for rational theory appraisal if we accept those values. This means epistemic values are of course not objective eternal platonic ideas, but this does not lead to »anything goes« as long as the epistemic values are justified by a reflective equilibrium of aims and methods to achieve them.[243]

Unsurprisingly, the most important criteria for good scientific theories are the classics, sometimes called »necessary criteria for good scientific theories«.[244]

- Non-circularity: Good theories should be free of vicious circles, because this makes them tautological.[245]

- Internal consistency: If a theory is logically inconsistent, everything can be deduced from it, so the theory is of no help at all (if applied without judgement).[246]

- External consistency: Theories should not contradict other well-established theories or background knowledge. Of course, many major breakthroughs in science did just that – but arguably, they did not gain their status *because* they were inconsistent with accepted knowledge but rather *despite* this.

tual theory choices made by scientists leads to »expounding hidden variables of the scientific community«. See Carrier (2009).

242 The same holds, of course, for »rationality« or in fact any term at all – there are no essential definitions that can capture the »heart« of a term once and for all.

243 See Laudan (1984).

244 See Vollmer (1992) and Lütge (2001), p. 129 et sqq. for a more detailed discussion of these criteria. Note that at the latest since Lakatos' constructive falsificationism it has been widely accepted that theory appraisal is never absolute but always involves comparing theories to their predecessors or competitors.

245 Note that some circularity is unavoidable if one rejects the possibility of foundationalist justification and opts for a reflective equilibrium method. Goodman, Vollmer and other naturalist philosophers argue that such circles are *good* circles. See Goodman (1955), p. 78 for the locus classicus.

246 Note, however, that some theories that were internally inconsistent became huge successes afterwards. Take the old quantum theory of black-body radiation, for example. (See Da Costa/French (2002), p. 105.) Nonetheless, even if inconsistencies may be temporarily accepted, they are *the* most-agreed-upon error a theory can have.

- Explanatory power: Despite the ongoing philosophical debate about the nature of scientific explanation, scientists demand that good theories offer explanations. If a theory is merely descriptive or does not offer causal hypotheses, this is seen as a severe disadvantage.

- Testability/success in tests: Good theories should offer hypotheses that can be empirically tested. That does not mean that every single part of a theory must be testable (in most cases the theoretical core is not), but after some time theories should pass some empirical tests successfully, at least for the phenomena they were intended to predict.[247]

Other criteria like generality, simplicity, precision, reproducibility of results[248] or completeness are usually taken to be less important and are merely called »desirable criteria«.[249] The history of science has shown, however, that there were tradeoffs between necessary and desirable criteria. In particular, generality and simplicity are often decisive in the early stages of theory creation, when the »necessary« criteria are not yet applicable and are merely hoped to be met at later stages of theory development.[250] Most notably, the persistent use of the homo oeconomicus in economics is not justified by its (broad) success in empirical tests but rather by its simplicity and applicability in various different contexts. This shows again that criteria cannot be universally ranked by their importance, but that their importance depends on the specific problems dealt with. In the social sciences, which deal with highly complex phenomena and where controlled experiments and precise prediction is impossible, simplicity and generality may legitimately be more important than e. g. striving towards high precision in predictions, because the latter seems unachievable in the first place.[251] In mainstream economics the tradeoff between generality and specificity is overcome by the heuristic interpretation of the rational-choice approach: Rational choice is justified as the best we have as a *general* approach, but the models relying on it are usually very specific and will hold only for a narrow domain of events.

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247 As Feyerabend has continuously remarked, instant falsification when theories are not predicting successfully would lead to an unfair preference for established theories. The criterion of success in tests is important for ruling out sentences such as »the moon is made of cheese« to count as scientific just because they are testable.

248 If you wonder why reproducibility is not a necessary criterion, remember that many important scientific theories – such as astronomy or the theory of evolution – deal with phenomena that are not reproducible.

249 See e. g. Vollmer (1992), p. 21.

250 See Quine (1960), p. 19 et sqq.

251 Remember the discussion of the KIDS vs. KISS modelling approach in section 2.3.2.4.

As argued extensively in section 2.3, the traditional criteria are of limited help when evaluating economic models. Naturally, other criteria have been suggested. Here are some examples that are particularly interesting:

After a discussion of Aklerof's »market for lemons« and Schelling's checkerboard model of segregation, Robert Sugden proposed looking at economic models as descriptions of counterfactual worlds which should be assessed by the *robustness* of the model and its *credibility,* if it is an account of what could have been true.[252] In more detail, he argues that models are not abstractions or simplifications from reality but constructions of parallel worlds, which is why it makes little sense to assess their similarity to reality; other criteria are needed.

Checking a model for robustness means to examine the kind and degree of which variations of the assumptions affect a model's implications.[253] The aim of robustness checks is to find out if a model's outcomes depend crucially on what were intended to be merely auxiliary assumptions.[254] If this is the case, the model's outcomes are probably mere artifacts that do not tell us anything about the real world. If the results turn out to be robust to various changes in parameters and auxiliary assumptions of the model, the robustness-checking can help in determining which assumptions are essential for the primary results to occur.[255] Robustness is, however, a completely internal criterion that does not relate the model in any way to the real world. So even if an effect is robust under variations of a model, this is not enough reason to infer that it holds outside the model world as well.

Sugden suggests it should be possible to infer from a model to the real world when the model *credibly* captures relevant features of reality for the problem under consideration, because we permanently make inductive inferences from real instances to other real ones.[256] The thesis is easier to understand by an example: We observe racial segregation in real cities like New York, Philadelphia, Detroit and so on. From this we infer that other similar cities are similarly segregated. Now each city can be seen as a »natural model« of the driving forces for racial segregation. Sugden's twist is to ask »… if we

252 See Sugden (2000). See Sugden (2009) for a refinement of the original thesis. A thorough discussion by various authors follows in the same issue of *Erkenntnis*.

253 See Sugden (2000) p. 21 et sqq. Sugden was not the first to propose this criterion, but he provides a very convincing discussion. For more details see Woodward (2006).

254 E. g. main findings of Schelling's segregation model are robust, e. g. for variations in the grid-structure. See Flache/Hegselmann (2001).

255 Note that it is impossible to decisively assess the robustness of a model, because there are infinite ways to vary the assumptions, of which only a small fraction can be actually carried out.

256 See Sugden (2000), p. 24.

can make inductive inferences from natural models, why not from theoretical ones?«[257]

The crucial point in Sugden's proposal is certainly the definition of the term »credibility«. Sugden writes: »Credibility in models is, I think, rather like credibility in 'realistic' novels. In a realistic novel, the characters and locations are imaginary, but the author has to convince us that they are credible – that there could be people and places like those in the novel.«[258] Later he specifies this credibility in terms of internal and external coherence, where external coherence refers only to a kind of »family resemblance« between the model and our knowledge of causal processes in the real world.[259] This gives us a rough idea that models are credible in Sugden's sense if the causal mechanics they propose fit to our sense of the world.

Sugden's proposal seems appealing at first sight, but it has the shortcoming that credibility is only vaguely defined. He is, however, completely aware of this problem, and it would be unfair to expect an exact justification of inductive reasoning, as such a definition would be nothing less than a solution of the problem of induction. Nonetheless, the criterion of »credibility« is of questionable help for finding out which inductive inferences from theoretical models to the real world are credible and which are not.[260] Additionally, as I argued in section 4.1.2, plausibility considerations can even be harmful for science as they may decelerate scientific progress by being inherently conservative. Following Aydinonat, the role of (credible) models in economics is often different from traditional scientific aims. In many cases, all models can do is offer a possible and partial (invisible-hand) explanation for perceived facts.[261] Such explications are typically not testable, nor do they claim to be complete. Still, they may be epistemically useful: In case of the checkerboard model, a new and credible possible explanation for the emergence of segregation was offered.[262] Even if it is hard to show which causal factors are really driving society, the presentation of new ones can lead to a change in perspective and stimulate new lines of research. So the criterion that Aydinonat proposes is to ask whether a model offers a new and possible partial explanation.

The main problem with all quality criteria is of a more general nature: Every criterion needs interpretation, and no criterion can mechanistically determine theory choice. Therefore, instead of applying criteria directly, a kind of »market test« may be more suitable for determining the quality of theories, at least ex post. »Market test« refers here of course not only to the usage of

257 Sugden (2000), p. 24.
258 Sugden (2000), p. 25.
259 Note the similarity to Vilks (2002).
260 See Grüne-Yanoff (2009a) p. 93-95 for more detail on a similar point.
261 See Aydinonat (2007) for a short explication and Aydinonat (2008) for a full book. See Grüne-Yanoff (2009b) for a similar point.
262 See Grüne-Yanoff (2009a), p. 97.

theories in commercial products or services but to their usage in other scientific works as well. If a model were able to convince a certain amount of other researchers of its usefulness, this would surely be an indicator of its overall quality.[263]

»Policy relevance« is another standard that economics is often demanded to fulfil, and it is notoriously hard to meet. As I see it, »policy relevance« is not a unique criterion at all; it is rather a mixture of success in empirical tests and short-term manipulability of the matters dealt with. Since manipulability is a criterion which has nothing to do with the theories themselves, the criterion »policy relevance« reduces to empirical success and hence all the problems discussed in section 2.3 apply to it.

There are other, more technical criteria related to trusting scientific findings (not only theories!) that are usually not even mentioned in philosophical discussions but are nonetheless important. Take the following items as examples for criteria that are unquestionably[264] important when findings involve statistical analysis of data:[265]

- Adequateness of the sample size and type of randomization.
- Diligence of control for confounding variables.
- Theoretical substantiation of the statistical correlations that were found.
- Availability of relevant and reliable data for the hypothesis in question.

It is insightful to conclude this subsection about criteria for theory choice by an exposition of critical rationalism, which is highly critical of any standardised method for theory choice. In economics, critical rationalism was first introduced by Klappholz and Agassi in 1959 in response to the ongoing methodological fight in these days. Klappholz and Agassi rejected every rule or criterion that was proposed as insufficient and argued that economics cannot be saved by imposing single criteria but only by a general, open and problem-oriented critical attitude: »[T]he impatience appears to give rise to the belief that, if only economists adopted this or that methodological rule, the road ahead would be at least cleared …. Our view, on the contrary, is that there is only one generally applicable methodological rule, and that is the exhortation to be critical and always ready to subject one's hypothesis to criti-

263 The usual disclaimer »the successful is different from the good« is less true, the more empirical content a model has and the more critical of an attitude has been established in the respective community. However, the criterion has the disadvantage that it can be assessed only in the very long run.

264 Note again that the evaluation of the criteria themselves critically hinges on the values of the respective community.

265 See Beck-Bornholdt/Dubben (1998) for an entertaining yet detailed discussion of these criteria (and many more).

cal scrutiny.«[266] Because critical rationalism is directly headed *against* proposing criteria for theory choice, I will not pursue it here but will continue the discussion in the last section (5.2) of this book, in which I reflect more generally about the role methodological considerations play for scientists. In the remaining parts of the current section I will deal with different types of philosophical realism, which is often seen as the enemy of the pragmatic approaches that I have so far presented very favourably.

4.2. No Need for Truth but for a Useful Methodology

In the foregoing sections I have discussed many different methodological positions that offer a pragmatic way out of the dichotomy between empiricist and relativist methodologies. The summary is provided at the end of this section, where I try to condense the positions discussed above to a consistent blend of pragmatic methodology. At this point, I will first present the views of *realists* in economic methodology, who at first sight seem directly opposed to pragmatic solutions. But as I will try to show, the brand of pragmatism I endorse is not headed against but rather orthogonal to scientific realism.

4.2.1. Realistic Positions in Economic Methodology[267]

4.2.1.1. General Arguments Supporting Realism

Every discussion of realist philosophy of science must necessarily begin by distinguishing precisely the different forms of realism and declaring what is exactly at issue. The following list provides an overview of different realistic positions in philosophy of science, in ascending order of the strength of their claims:[268]

1. Ontological realism: This is the most modest realistic claim and merely entails the belief in the theory-independent existence of an external reality.
2. Semantic realism: Scientific theories *refer* to an external reality.
3. Weak epistemic realism: Scientific theories refer to an external reality and may be correct in their claims about it, i. e. they are capable of being

[266] See Klappholz/Agassi (1959), p. 60.
[267] The contents of this chapter have been published in Deichsel (2011).
[268] See Mäki (2001) for an overview about different forms of realisms. Note that Putnam's »internal realism« would not count as realism in this classification, because it does not fulfil even the weakest claim of ontological realism. See Putnam (1981).

true or false. This includes the semantic thesis that theories are true if and only if they correctly refer to an external reality.

4. Scientific realism/strong epistemic realism: Well-confirmed scientific theories refer to an external reality and are basically correct in their claims about it.[269]

Both weak and strong epistemic realism are deeply connected with the correspondence theory of truth, because their central point is to make claims about the properties of an external reality. If ealists relied on a coherence or consensus theory of truth, this would directly beg the question. I take anti-realism as the thesis that we should suspend judgement on the truth and truthworthiness of our theories or avoid talking about the truth of theories altogether in order to minimize the confusions that surround this concept.[270] I analyse the pragmatic aspects of the justifications for realism of two prominent realists in economic methodology: Uskali Mäki and Tony Lawson. I argue against these pragmatic aspects and try to show why an anti-realist perspective is preferable.

A first twist the realists could take to defend themselves against anti-realist attacks shall be answered shortly: When pressed by a sceptic who insists we cannot know anything for certain about the existence of entities, let alone their »reality«, modest realists could react by pointing out that they are merely referring to a common-sense notion of reality and only hold that entities used in successful scientific theories have an reality status that is similar to observable objects whose »reality« no one would seriously doubt. However, such strategic withdrawals deprive the realists' position of its bite.

Ian Hacking's work prominently uses an argument based on this line of reasoning: His central argument is that reality is not a property of *theories* as such (and in this he agrees with van Fraassen), but rather that our practical *intervening with entities* assumed by a theory makes those (and only those!) entities real.[271] More concretely: The fact that we can *do* something with electrons makes them real. Hacking's realism is of a very modest sort and refers only to a small class of theoretical entities, *not* to theories as a whole. This al-

269 The qualification that theories are only »basically« right allows for structural realism as well. (See Worrall 1989 for the locus classicus). Hence, even if I coin it »strong epistemic realism« it is still a rather modest position compared to the view that many scientific theories are completely correct in their claims about reality. Note that this definition differs from Dummett's classic (see Dummett 1982) which identifies realism with the concept of evidence-transcendent truth, which makes it impossible to claim truth for any existing theory. Please note that I do not claim my list of realisms to be exhaustive.

270 Note that I do not claim that no theory can be possibly true – there may well be theories that are true (even if just by chance), but we should avoid talking about the truth of theories.

271 See e. g. Hacking (1983), p. 262 et sqq.

lows him to argue in favour of a disunity of science, where different branches tackle different problems by means of theories that may even be inconsistent to each other because they have no truth-status whatsoever.[272] Hacking's view could be taken as a basis for an anti-realist[273] interpretation of economic theory, because even if we successfully intervene on a daily basis with the *objects* of economics (such as money, markets and individuals), thus making them »real«, economic *theory* would be freed from criticisms calling it an »unrealistic representation« of what is going on in the world: from a Hackingian perspective theory has no truth-status and is not representing anything. Here a realist's philosophy offers anti-realistic arguments.

In short, the modest realists like Hacking are not claiming anything metaphysical but at best do »descriptive metaphysics«[274] i. e. try to untangle the implicit ontological assumptions behind scientific theories. This is of course a far cry from epistemic realism.

For my project, an epistemic realism that avoids the above twists is the relevant claim to analyse, because anti-realism is precisely the rejection of the claim that we should talk about the »truth« of theories.[275] This claim is the form of anti-realism adopted by most economists (consciously or unconsciously following Boland's[276] interpretation of Friedman's classic methodological article). Economists usually don't doubt the existence of a theory-independent world, nor do they deny that their theories could be true; they merely do not want to talk about truth, and underpin this view by constantly repeating the common cliché that all models are false, but some are useful.[277]

First I will summarise some of the strongest arguments in favour of realism and after that present the approaches of the two best-known realists in economic methodology, Uskali Mäki and Tony Lawson.

The so-called no-miracle argument is often considered to give the strongest support for scientific realism.[278] In its most basic version it simply states that realism is the »only philosophy that doesn't make the success of science

272 See Hacking (1983), p. 219.

273 Note that I use the terms »anti-realism«, »non-realism« and »instrumentalism« as synonyms that are all merely defined by their rejection of any form of epistemic realism.

274 Peter Strawson coined this term. See Strawson (1959). This is why I do not deal with Hacking's realism here. Hacking's main point is that theoretical constructs can have the same reality status as observable objects, and this is a point of descriptive metaphysics and not an ontological claim.

275 From this point on, the term »realism« refers to the kind of realism described above, if not indicated otherwise.

276 See Boland (1979).

277 See Mayer (2004).

278 Even if many philosophers have used similar arguments, the no-miracle argument is nowadays attributed to Hilary Putnam.

a miracle.«[279] This means that realism can *explain* the success of science, and positions opposed to it cannot. The no-miracle argument is based on the inference to the best explanation, which becomes clear in a slight reformulation:

> First premise: It is legitimate to conclude that the best explanation of a phenomenon is true.
> Second premise: The best explanation for the success of science is that it offers true representations of the external world.
> Conclusion: It is true that science offers true representations of the external world.

The no-miracles argument is reversed in a different reformulation: Why should we believe in science at all, if we do not believe that it tells us basically the truth?

Another argument in favour of realism is asymmetrical to the no-miracle argument. It takes the fact that theories can *fail* as an argument for realism.[280] Note, however, that this argument only supports weak epistemic realism, i. e. the thesis that theories *may* be true accounts of reality.

A third argument connected to the no-miracles argument refers to *convergence* in science. Remarkably, there is convergence in the measurement of natural constants: New methods of measurement often confirm old values and increase precision. If measurements did not roughly tell us the truth about an external reality, it would be a miracle that they often converge to ever more precise values even if various and diverse techniques of measurement are involved.[281]

Now that we have seen the most important *general* arguments favouring strong epistemic realism – which are quite simple in their basic form – let's turn to realism in economic methodology, after which the subtlety of the arguments will become more evident.

4.2.1.2. Uskali Mäki's Realism

Uskali Mäki is probably the best-known realist in economic methodology.[282] His overall strategy consists of developing a discipline-sensitive brand of realism that is tailored to analysing many of the traditional problems in eco-

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279 Putnam (1975), p. 73.
280 See Vollmer (1991), p. 139.
281 There is a different convergence argument that claims the long-run convergence even of theories. I do not list it here as an argument for realism, because its basic premise that theories tend to converge is highly controversial.
282 See Mäki (2008) for a recent overview of his position. The following outline builds on this article, where not indicated otherwise. See Peter (2001), 579 et sqq. for a different synopsis of Mäki's main points.

nomic methodology. His approach can be described as »bottom-up«, which means that he tries not to invoke external philosophical concepts for criticising economics but to first understand what economists are doing before seeking a realist interpretation for it. Mäki's justification for taking a realist position is pragmatic insofar as he fears that giving up realism »would result in the worst kind of complacency«.[283] I call this a pragmatic justification because it focuses on the positive consequences that an adoption of realism would have. On the other hand, Mäki believes that realism can offer arguments against the well-known defence of abstract economic reasoning that jumps from the premise that all models are false anyway to the claim that all criticism against the falsehood of economic models is to be rejected.[284]

In a definition of realism that Mäki gives it becomes clear that his realism is based on a correspondence theory of truth, as he argues that good science pursues theories that are true by corresponding to reality (»the objective structure«): »… theories and models are true or false in virtue of the ways of that objective structure – not in virtue of whether evidence supports them or whether we are otherwise persuaded to believe in them, for example. Finally, good science pursues theories that are true, while being prepared for the possibility of error.«[285]

Mäki's realism allows him to talk of economic models resembling the real world. He distinguishes between models whose internal analysis is, for economists, a complete substitute for analysing the real world and other models that are useful surrogates for doing this. While the terminology of substitutes and surrogates may be confusing, the claim that some economists are getting lost in formal analysis – which becomes a substitute for real-world research – is quite plausible.

A main point of Mäki's work consists of trying to show how highly idealised economic models *can* relate to reality so that their analysis can be a useful surrogate for doing direct empirical research. Mäki states that »economists can be philosophical realists about their models even though these describe imaginary situations«[286] and turns the above argument against the relevance of falsehood upside down: Even if all models are necessarily false in the details, we can believe them to be *essentially* true, because the idealisations are strategic and necessary falsehoods that aim at isolating the true core of a model. Referring to Hausman, Mäki takes the high degree of theoretical isolation in economics to be the reason why it is an »inexact and separate« science.

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283 See Mäki (2002), p. 102
284 See Mäki (2008), p. 7.
285 Mäki (2008), p. 9.
286 Mäki (2008), p. 15.

Mäki compares his approach to Nancy Cartwright's[287] point that economics lies because the world is messy and the models are cleaned of disturbing factors, but in contrast to Cartwright, he believes there is a chance for the basic causal mechanisms of a model to be true, even if the messy world seems to contradict them.[288] Yet it is undeniable that some assumptions in economics are merely introduced for tractability reasons and not because they isolate central factors – take e. g. the assumption of perfect knowledge, the ignorance of transaction costs or constant returns to scale. Mäki acknowledges this and sees relaxing these assumptions (and not the ones which are needed for theoretical isolation!) as a major driving force of economics becoming more realistic »in the right sense«.[289]

Mäki borrows an important argument in favour of realism from Lionel Robbins.[290] He takes the view that economics does not create new »unobservables« but deals with entities that are close to common sense (which he calls »commonsensibles«), such as firms, households and prices. These entities have a certain amount of »reality« because we deal with them in our daily life (in contrast to physical entities like electrons or quarks). Even if the »commonsensibles« that economic theory deals with are highly idealised, the idealisation is »strongly condensed by economists' commonsense intuitions«.[291] This leads of course to the rejection of models that contradict common sense, making the differing commonsense convictions of economists a highly crucial point in theory choice. But if we accept that the basic entities of a certain economic model are based on commonsense notions, it becomes clear why the existence of the basic entities is – in contrast to physics – not the main point of a realistic position in economic methodology. Instead, the main point regards the reality of the causal mechanisms postulated by economic models.

In his conclusion, Mäki repeats the distinction between realism and realisticness.[292] He admits that it is impossible to know if his philosophical meta-theory of realism is true and that – even worse – when we agree that economic models may be false (due to several epistemic and institutional disturbances), we are forced to admit that the meta-theory may be false for the very same reasons. This finally leads to the introduction of fallibilism as the super-rule.[293]

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287 See Cartwright (1983). Even if she initially referred to physics, Cartwright's point about the lying of abstract laws is similar in economics.
288 See Mäki (2008), p. 17.
289 See Mäki (2008), p. 19.
290 See Robbins (1932).
291 Mäki (2008), p. 27.
292 See section 4.1.2.1.
293 See Mäki (2008), p. 35.

In a text called »Some Non-Reasons for Non-Realism about Economics«, Mäki rejects several premises that seem to support an anti-realistic interpretation of economics. Here is a short summary of his counter-arguments:[294]

> **Thesis 1**: »Economics postulates unobservables, therefore it is better interpreted by non-realism.« Mäki responds that this happens in every science and is no reason for non-realism, especially because many of the unobservables in economics are »commonsensibles« as explained above.
> **Thesis 2:** »Economics is based on false assumptions; this is an argument for interpreting it by non-realism.« Mäki responds again that this is true for all sciences in a strict sense, so it does not support non-realism. The relevant question is whether the false assumptions help to isolate parts of reality or not.
> **Thesis 3:** »Economics is not predictively successful, so the basic premise for the no-miracle argument is missing, which is an argument for non-realism.« Mäki responds, as explained above, that we have more direct access to economic phenomena by our common sense, so believing in the reality of basic economic premises does not need to be justified by the no-miracles argument. Besides that, he claims, taking into account the complex nature of economic systems, it would be a miracle indeed if economics was predictively successful.
> **Thesis 4:** »When accepting a theory, economists are persuaded (and not rationally convinced) by other things than truth, which is an argument for non-realism« Harshly abbreviated, Mäki responds by arguing that persuasion is completely orthogonal and not antagonist to truth, and therefore the argument is not against realism. Even if much persuasion is involved, the resulting theories can still be true.

These arguments show how anti-realism should not be justified, according to Mäki. They also show that his justification of realism often consists of attacks against anti-realism combined with an appeal to realist intuitions. However, as mentioned above, it should be noted that Mäki also provides a pragmatic justification for his realism when he expresses the fear that giving up realism could lead to justifying *anything* in economics, even if it were only »a game of just playing with fictions«.[295] Obviously Mäki believes in the good methodological consequences of realism and, again, this is what I call a pragmatic justification. While Mäki is doubtful of whether a strong epistemic realism can be achieved, he clearly sets this as an aim.[296]

[294] See Mäki (2002), p. 92 et sqq. See Hodge (2007), p. 10 et sqq. for a discussion.
[295] Mäki (2002), p. 102.
[296] See Mäki (2002), p. 104.

Below I will consider whether Mäki can live up to the task of improving economics by means of his realism. But before this, in the next section, I will present the other key realist position in contemporary economic methodology: Lawson's critical realism.

4.2.1.3. Tony Lawson's Realism

Tony Lawson's realism differs fundamentally from Mäki's.[297] Where Mäki is generally neutral or even affirmative concerning mainstream economic theory, Lawson decidedly wants to use realism as a tool for criticising current mainstream economics. Lawson starts with the premise that mainstream economics is in a state of disarray because it focuses too much on formalised deductive modelling and does not deal with real-world issues.[298] He locates the fundamental error of mainstream economics in its anti-realist methodology[299] that sees truth as an irrelevant criterion for theory evaluation.[300] His basic argument is that instrumentalism leads economists to ignore the central problem of their field – the lack of realisticness of theories – by rendering it unproblematic by definition.[301] According to Lawson, the instrumentalist is in a desperate situation if the theories are not successful at predicting empirical data. In this case, the instrumentalist usually recommends to try harder, to dig deeper and to search for regularities at a more disaggregated level – realism is the recommended way out of this problem.

Lawson states that in some sense nearly everybody is a realist, as even methodological instrumentalists often accept ontological or semantic realism. For this reason he defines his blend of realism by its »sustained concern with ontology«.[302] By this focus on ontology, Lawson hopes to learn something about the nature of social phenomena, which he thinks will enable him to give better methodological advice to economists than instrumentalists can.[303] This is a pragmatic defence of realism, as it concentrates on the positive consequences of adopting critical realism. Indeed, it is much more explicitly pragmatic than the defence Mäki gives, because Lawson's project is much more normative. In his most recent book, *Reorienting Economics,* Law-

[297] See Fleetwood (1999), Lewis (2004) and Fullbrook (2009) for volumes dealing in detail with critical realism.
[298] See Lawson (2001), p. 155 et sqq.
[299] Lawson chooses to talk about »instrumentalism» but explicitly includes the pragmatic view that theories can be false, but nonetheless good. See Lawson (2001), p. 164.
[300] See Lawson (2001), p. 161.
[301] Lawson (2001), p. 164.
[302] Lawson (2001), p. 168.
[303] Note that this is a pragmatic justification of realism similar to Mäki's. Lawson owes much of the philosophical foundations to Roy Bhaskar's work, though. See Hodge (2007) for a comparison of Mäki's and Lawson's versions of realism.

son even suggests that all heterodox traditions are best understood by looking at the social ontology they presuppose.[304]

Lawson's most important critical point concerns deductivism. He states that the formalistic models of mainstream economics necessarily rest on a deductivist mode of explanation, even if that fact may be concealed by the usage of stochastic variables or non-linear equations. According to Lawson, the fundamental problem of deductive reasoning is its dependence on closed systems that are characterised by stable, observable event regularities. However, Lawson suggests »that the social realm is everywhere open, that scientifically interesting event regularities rarely, if ever, occur.«[305] This makes deduction of future events or using theories as tools for prediction not only difficult but inherently wrong. Lawson argues that deductivism in economics needs an ontology »of structures, powers, mechanisms and tendencies, etc., that are irreducible to, but which underpin the actual course of events and states of affairs. Once this ontology is established it supports a conception of science as moving from phenomena at one level to its conditions or causes at a different, deeper, one.«[306] Lawson states that deductivists (including predictive-instrumentalists) cannot discuss these matters and are therefore unable to explain why science is in fact successfully applied to open systems where event regularities do not hold.[307]

In Lawson's view, economic laws should not be made to represent observable event regularities but rather the underlying workings of mechanisms and tendencies. He states that his (realist) perspective should be accepted due to its greater explanatory power concerning the question of how it is possible that results which hold in closed systems can often be meaningfully transferred to open systems, even if the predicted event regularities do not hold there.[308]

His studies in »social ontology« lead Lawson to claim »that economics ought really to move in a different direction entirely, to develop ways of uncovering causal mechanisms in a seemingly quintessentially open – as well as intrinsically dynamic and highly internally-related – social reality.«[309] The described social reality does not have the same ontological independence of human thought as does the natural reality, because it is a human construct and hence depends directly on human thinking. Lawson rejects the view that

[304] See Lawson (2004b), p. 330.
[305] Lawson (2001), p. 170.
[306] Lawson (2001), p. 172. This quote shows that Lawson is committed to »real« ontology and merely in debunking metaphysical presuppositions of existing economic theories.
[307] See Lawson (2001), p. 171. Note the similarities between Lawson's view and Hausman's notion of tendency laws.
[308] See Lawson (2001), p. 173.
[309] Lawson (2001), p. 175.

all causal forces of social reality are reducible to individuals, because socioeconomic structures exist prior to individual action.[310] Even if Lawson's social ontology is supposed to reveal »deeper structures« and »essential features«, it does not include the claim of ultimate knowledge about these matters and admits its findings are fallible.[311]

Lawson is convinced that if one accepts the above ontological claims, a methodology that takes individual reactions to changes in relative prices as its basis is ill-conceived, because it systematically neglects the freedom of human choice and the power of social structures. Orthodox economic theorising therefore often employs convenient fictions that state very general and tractable connections between variables, instead of looking at real and essential forces.[312] Lawson seems to suggest that a realist methodology is a necessary precondition for supplementing or replacing the (alleged) mainstream concentration on correlation analysis with causal explanations.[313]

According to Lawson, adopting his methodological views will lead to an economics that is a much more complicated and messy affair than the current mainstream.[314] In this context, the best we can hope for is a kind of interpretative explanation (i. e. not the prediction) of so-called demi-regularities, a term that is essentially equivalent to Kaldor's »stylised facts«.[315]

In short, Lawson states that economics should be concerned with the *essential* features of economic systems, and his critical-realist methodology is designed to uncover them.

4.2.1.4. Critical Discussion

We have now seen many arguments supporting scientific realism in general and two positions in the methodological literature of economics specifically. As might have been clear from the beginning, I do not adopt a realist's position in this work. On the contrary, I hold the view that realism makes no methodological difference. There is a crucial difference from the discussion about postmodern relativism in section 3.2: Where the enemies of postmodernism hoped to fight relativism by attacking the postmodern claim that science is separate from truth, the discussion is now twisted: First I try to refute the constituting arguments behind strong epistemic realism, and then I aim to show why this does *not* lead to relativism. I will argue that »truth« is almost always replaceable by other terms that are ontologically more parsimonious

310 See Boylan/O'Gorman (1995), p. 98-99.
311 See e. g. Lawson (2001), p. 178.
312 See Boylan/O'Gorman (1995), p. 105-106.
313 See Lawson (2001), p. 178.
314 See Lawson (1997), p. 270.
315 See Lawson (2003), p. 79 et sqq. Lawson approvingly names Veblen's evolutionary economics as a role model for ontology-committed research in the social sciences. See Lawson (2003), p. 184 et sqq.

(such as empirical adequacy[316] or fit with the totality of current knowledge[317]) and may nonetheless fulfil the normative intentions Mäki or Lawson had.[318] While I accept many of the conclusions that Mäki draws (and some of Lawson's), I cast doubt on whether realism is necessary for justifying these conclusions. The next sections will elaborate on these doubts.

4.2.1.4.1 Rejecting the General Arguments for Realism

The philosophical dispute about realism is, of course, not easily settled. I will first sketch some general arguments against realism before dealing specifically with Mäki's and Lawson's arguments.

Let's start with the famous »no-miracle« argument for realism. It states that the success of scientific theories can be *explained,* claiming that these theories capture elements of an external reality. It is true that anti-realism cannot offer such an explanation, but the crucial question is whether the realist move is an explanation at all. It often seems that the realist's arguments are begging the question of the anti-realists and vice versa.[319] I think this is the case with the »no-miracle« argument as well. The anti-realist would claim that we are *not* justified in explaining the success of science by its truth[320], because theories could well be successful without being true, due to empirical underdetermination.[321] In short, scientists simply accept those theories that work well – and that is all there is to say. Accepting truth (in the sense of correspondence) as the best explanation for their success means to go beyond the borders of what we can legitimately infer. From this view, the suggestion that truth explains the success of theories is no explanation at all – it is rather an illegitimate *ad-hoc* statement. We could argue equally well that the existence of god is the best explanation for why our theories work, but anti-realists are convinced that we should not do that on the same grounds of why we should not »explain« success by an independent reality: In both cases, the explanation is based on uncertain ontological claims. But we *can* know whether or not a theory is helpful for solving our problems, because that is a completely subjective judgement which does not involve an ontological claim.

316 See van Fraassen (1980) for the locus classicus of a defence for this criterion.
317 In the sense of Quine/Ullian (1970).
318 A similar claim has been made by Rorty (1994), p. 23.
319 As stated earlier, I will only argue against strong epistemic realism. Anti-realism is therefore opposed only to this form of realism. This means that claiming the *existence* of an external reality that »pushes back« against our theorising is no problem for the anti-realist here.
320 Keep in mind that I assume realism is committed to a correspondence theory of truth by definition.
321 Remember that this does not entail the claim that all theories are equally good. Underdetermination merely claims that two theories can be both perfectly empirically adequate while they seem to make different claims about reality.

A stronger argument in favour of anti-realism is the fact that even inconsistent theories can »work«[322] – which shows that success is not bound to truth, because the truth can hardly be inconsistent.

Once we talk about the acceptance of the »inference to the best explanation«, the quarrel between realists and anti-realists gets more complicated. In her daily work, an anti-realist may accept and use some theories because she holds them to be the best explanation for a phenomenon under scrutiny. For example, the anti-realist may accept increased demand for oil as the best explanation for a rising oil price.

Now, the realist can ask why the anti-realist stops short of accepting realism as the best explanation for the success of theories and hence does not give up his anti-realist position. At this point, it becomes clear why the »no-miracle« argument is question-begging and cannot settle the argument between realists and anti-realists: both may be willing to *accept* best explanations, but the anti-realist never asserts the *truth* of the explanations she accepts and so will not accept truth as the best explanation for success.

Furthermore, the argument that scientific theories can fail does not refute an opponent of strong epistemic realism, either. It merely supports what I have dubbed »ontological realism«, i. e. the view that there is an external reality that can be *incompatible* with our theories. However, it does not show that those theories that *are* compatible with the external reality are such because they are »true« or »realistic«.

We have already seen another argument favouring epistemic realism – the so-called convergence argument – which takes the increasing precision in the measurement of natural constants as an argument for realism. But can this argument really support the view that the measured constants are really »out there« and are represented realistically by our theories? The anti-realist can argue against this by stressing that the precision of natural constants only grows as long as a paradigm prevails. The argument of growing precision can be turned against strong epistemic realism by using Larry Laudan's argument of the pessimistic meta-induction, which says (briefly summarized) that we have good reason to doubt the realism of our recent theories because many old and successful theories proved to be wrong.[323] So increasing precision does not warrant realism, because the measurements are always »paradigm-laden«. Different theories may lead to an increasing precision of the natural constants, but that is what you would expect if something like Kuhnian normal science is going on. When the whole paradigm comes into a crisis and a new one arises, the old constants may cease to be meaningful at all. The convergence argument about strong epistemic realism would be convincing if we believe that the increasing precision will go on forever. This is, however, very

322 See Da Costa/French (2002), p. 105 et sqq. for a stunning overview.
323 See Laudan (1981).

unlikely. Even if realists may claim that their metaphysical stance is based on a picture that is supported by our best science, there are anti-realists (or »agnosticists«, as they are called in the source referred to above) that show quite convincingly that there is no general metaphysical picture supported by our best science, and hence divergence rather than convergence is the most likely outcome.[324]

Following the arguments against realism in general philosophy of science given above, I will now provide a discussion of Mäki's and Lawson's position and by this make the discussion more tailored to economics again.

4.2.1.4.2 Against Mäki's Realism

Before criticising some of Mäki's arguments in support of realism, I should stress that I accept many of his arguments and even generally share his point of view, except for its realist branding. I welcome his bottom-up approach, I accept his distinction between »realism« and »realisticness« and I even accept his point that many assumptions in economic models serve the tractability of models rather than their epistemic value. His argumentation on these points is careful and convincing – which is why it does not need to be repeated here. The list could go on further, but there would be little point in listing all the commonalities I share with Mäki. Therefore, I will now start to argue against the points I do not agree with.

The main point against Mäki's usage of the term »realism« would be that it is nothing more than a brand name. Mäki explicitly admits that many other methodologists contribute to the realist project, even if they don't do it »under the banner of realism«.[325] This raises the question, of course, whether the term »realism« as Mäki uses it is informative at all.[326]

The main problem when trying to refute Mäki's realism is that he does not offer a real defence for it that could be attacked. His lack of a defence shows up clearly when he tries to defend realism against McCloskey's postmodernist charges – in opposition to the very notion of an external truth – merely by stating »In my alternative realist account of rhetoric, the world and truths about the world are not dependent on persuasion amongst economists and their audiences. I reject the presumption that the occurrence of rhetorical persuasion alone rules out the possibility of attaining and communicating persuasion-independent truths about economic reality«.[327] Instead of defending realism with arguments, Mäki admits that he begins with the *intuitions*

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324 See Ritchie (2008).

325 Mäki (2007), p. 438. He even includes Hausman, who attacked realism in a recent article. See Hausman (1998).

326 These doubts are reinforced by the fact that a big share of Mäki's work consists of realistic reinterpretations of economic classics that are usually not seen as realist, most notably Friedman's methodology. See e. g. Mäki (2005b).

327 Mäki (2008), p. 30.

of a realist.[328] He then proceeds by showing how much of what is going on in economics can be rendered intelligible by his realist interpretation. I am the last to doubt that Mäki is immensely successful in this, but I do doubt whether this is enough of a justification for realism – or rather, whether he is preaching to the already converted.[329] Mäki's work does show that realism offers a good way to talk about problems of economic methodology. However, this is not enough for refuting anti-realism. If Mäki wants to defend his brand of realism pragmatically, he needs to show how his version of realism would lead to an improvement of economic research and which standards it would specifically employ apart from standards that are compatible with anti-realism, such as problem-solving capability or empirical adequacy.

The lack of this discussion in Mäki's work and, as I would say, the impossibility to show specifically how realism would change economic research, makes a pragmatic justification for realism difficult to provide. Mäki, at best, gives reasons which show that it is sometimes simply natural to assume an external world and economic models relating to it, and the realist can talk about unrealistic models that do or do not capture features of the world. Here, however, the anti-realist would talk about making assumptions that diverge from our current beliefs about the world but nonetheless make successful (structural) predictions, and by this, offer plausible explanations.

Mäki uses his realist rhetoric to argue against mere derivational unification (deriving more outcomes from the same set of premises) and in favour of ontological unification (establishing more »ontic unities« between phenomena, i. e. showing that they are of the same kind).[330] This sounds convincing, but is it really a normative guideline that differs substantially from what an anti-realist would advocate? As long as realism does not provide a unique standard to distinguish the two modes of unification, we are left with commonsense arguments that are not opposed to anti-realist positions.

This, of course, undermines any normative thrust for realism, as we are still left with anti-realism-compatible standards such as empirical adequacy plus some pragmatic values like simplicity, fertility, modesty and conservatism.[331]

328 See e. g. the very first sentence in Mäki (2008).

329 Schliesser (2010) makes a similar point.

330 See Mäki (2008), p. 25.

331 In his major work »Philosophy and the Mirror of Nature« Rorty argues that the (alleged) fixation to a correspondence theory of truth has been a major error of epistemology since the times of Plato. See Rorty (1979). Now, a realist could try to escape the trouble and argue that our theories will be true »at the end of all science«, because science approximates truth. Be aware of the difficulties of such a position, which were in part discussed in section 2.3.2.2. It is tough to argue that science approaches something we are not capable of knowing, even when we have reached it. »The end of all science« serves here as a substitute for truth but does

Mäki often speaks about the »www« (the way the world works) constraint, which refers to economists' convictions about real, causal connections in contrast to their model results.[332] But this is hardly a constraint at all if we cannot know when it is met.[333] In his most recent paper, Mäki seems to intend by the »www constraint« nothing more than checking a model's assumptions against commonsense intuition.[334] This is of course unproblematic for the anti-realist, because it is only a consistency criterion and is a far cry from making ontological claims.

As I said, Mäki pragmatically justifies his realism as a powerful instrument of criticism for economic models.[335] To me, the issue seems the other way round: realism is less critical of a methodology than anti-realism is, because it allows talking about truth while anti-realism suspends judgement on the matter. Mäki's recommendation for developing useful surrogate models instead of getting lost in internal formal analysis, or his suggestion to check models against commonsense intuition, can be kept without subscribing to realism of any form.

4.2.1.4.3 Against Lawson's Realism

Now let's see how Tony Lawson's critical realism fares against critical scrutiny. Where Mäki's work is a rather neutral outline of a realist's point of view of economic methodology, Lawson intends to overthrow economic orthodoxy. If one is inclined to accept the methodology of mainstream economics as it is – and therefore does not share Lawson's view that the search for observable event regularities fundamentally contradicts the ontology that underlies social processes – there is little reason to follow Lawson's demand for more realism at the foundations of economics. And even if one disagrees with much that is going on in mainstream economics, there is no need to accept Lawson's realist critique. It is important here to keep in mind that Lawson proposes a normative *methodological* realism: In his view, economics should deal with the real forces that move societies, and these cannot be modelled in the deductivist style, according to Lawson. Where mainstream economists cherish elegance, simplicity, parsimony, tractability, unifying power and the like,

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not concretise it at all. Rather, science becomes connected to truth by the very definition of truth, making the rebuttal circular. Note also that even at »the end of all science« there may be irreconcilable theories due to empirical underdetermination.

332 See e.g. Mäki (2005b), p. 21.

333 This is a general problem with any correspondence theory of truth: A correspondence theory may capture adequately what is commonly meant by »truth«, but it fails to give viable criteria to decide whether something is true or not.

334 See Mäki (2008), p. 27.

335 See e.g. Mäki (2002), p. 102.

Lawson wants to assign greater weight to other epistemic virtues such as truth, realism (or realisticness), credibility and plausibility.[336]

But is he justified in demanding this? There are at least three reasons why I disagree with his position: First, we cannot know what the real forces are; second, his proposal can be turned against any form of idealisation; and third, it is doubtful whether mainstream economics is well characterised by Lawson's interpretation of the term »deductivism« at all.

I will not deal with the first point in much detail here, as I have already laid it out in quite some detail during my discussion of Mäki's realism. It is important to note that this point is even more crucial for Lawson, because of his strongly normative orientation: Lawson urges economists to deal with the true and essential powers, but he fails to show how anyone could have knowledge about this. Lawson argues in the typical question-begging way that characterises the debate between realists and anti-realists: He accuses instrumentalism of ignoring the central problem of realisticness.[337] Refusing to talk about realisticness is of course the main point of any form of anti-realism, and therefore not an argument against anti-realism at all.

I will also grant that Lawson's constant demand to search for the real structures in inherently open social systems may lead to a more realistic description of those sytems, but taken seriously it prevents many forms of abstract theorising and the usage of idealisation. There are many theories that would have to be abolished straightaway if Lawson's normative realism was uniformly accepted: Just think of formal decision theory, game theory, any theory employing folk-psychological reasoning, any form of hypothetical contractarianism and even political liberalism – they are all admittedly based on unrealistic assumptions.[338]

It is questionable whether looking for the real essential powers that drive human behaviour will soon lead to theories of any use for economic problems. It seems more likely that such a procedure will spur a quest into the mysteries of the human brain and the freedom of the will. Lawson does not promote this, but rather takes his favourite project, called »social ontology«, as a starting point.[339] The sustained concern with social ontology is bound to realism by definition in Lawson's work.[340] But is Lawson justified in his de-

336 See Vromen (2004).

337 See Lawson (2001), p. 164.

338 If one extends Lawson's demand to the natural sciences, many parts of applied physics would have to be abolished as well; the usage of idealisations and counterfactual assumptions is widespread here, too. See e. g. Audretsch (1989).

339 In Lawson's view, »essence« is different from »invariant« or »ultimate« – it depends on our transitive knowledge. As he sees it, social ontology can provide the essence of economics. See Boylan/O'Gorman (1995), p. 100.

340 See e. g. Lawson (2001), p. 167.

mand that economics should be reoriented to become a science based on social ontology?

This can be denied at two different levels:

First, it is not obvious that social ontology gives us a realistic representation of the social world. Surely, the attempt to incorporate our commonsense knowledge of social systems (e. g. the claim that social processes are dynamic and inherently open processes) into the fundament of economic theory will make it more realistic by commonsense standards. But again, there is no viable criterion to judge how a »reoriented economics« based on social ontology approaches or mirrors an external reality, except for the notoriously vague notion of common sense. Or, as Wade Hands puts it, »critical realists […] offer no unique method […] that gives us access to those enduring structures«.[341] Additionally, it is even doubtful if Lawson's ontology – which assigns social structures an individual-independent existence – is indeed more realistic even by commonsense standards.

Second, for the sake of Lawson's argument, let us accept that an ontology that respects the inherent dynamics and openness of social systems fits better into the totality of our current beliefs than a mechanistic picture would. This fit is surely not an absurd standard for »realism«, but is improving it a helpful normative guideline? I have my doubts. Also, at the methodological level more realism may not be helpful: The increased detail of research based on »social ontology« is not likely to be a useful basis for theorising, because the emerging picture is too »messy« for that. While a deterministic picture of humans as rational agents may be false, this might in fact be fruitful. To be sure, Lawson would deny this, because he thinks the whole project of mainstream economics is on the wrong track. Alas, this fundamental assumption of his work is not carefully argued for. Lawson merely provides a collection of critical voices and adds the claim that mainstream economics is not successful with accommodating the data.[342] This is at best only a half-truth: Surely economics is completely unsuccessful at predicting the next financial crisis or even the growth of the GDP for more than one year. But on the other hand, there is a plethora of well-confirmed *conditioned* predictions of tendencies which become better and better in non-crisis situations without the need to refrain from the underlying »deductivist« structure.[343] Therefore, Lawson

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341 Hands (2001), p. 327.
342 See Lawson (2001), p. 168.
343 See e. g. the case studies of section 2.4. Besides, it would be confusing that governments, companies and individuals continue to hire economists to a much higher degree than they do other scientists, if their advice was not useful at all. Still, it may be the case that governments are wrong in trusting economists as much as they do, and that they are merely impressed by the formal work – see Klein (2010). Consider the fact that there are many open disputes in economics and hence many situations where forecasting is affected in a strong degree by in-

is not justified in completely rejecting the mainstream research programme. Of course, he is free to start his own project of a critical-realist economics that is based on social ontology, but Lawson will hardly gain many adherents if there is no agreement on the mainstream being in irresolvable disarray, so that the only escape would consist of changing the *goals* entirely and removing prediction (even conditioned prediction) from the wish list. So even if we accept that Lawson's ontological approach is more realistic, it does not follow that it is convincing and should be adopted.[344]

Now let's turn to the third point, namely the question of whether mainstream economics is adequately characterised by Lawson's label »deductivism«. As he describes it, deductivism is necessarily committed to a notion that characterises scientific laws as observable event regularities. This is a big misunderstanding. The mathematical-deductivist style (which is admittedly used often in mainstream economics) does not commit economists to a »flat« ontology that forbids any talking about underlying structures that cause event regularities to occur. As Vromen notes, economists try to look for more than just event regularities – and are even encouraged in this by Friedman's classic methodological manifesto[345], which I already have shown to be much less »positivist« than its title suggests. Despite the common usage of mathematical deductions, mainstream economics aims at uncovering underlying structures of the social world – they do this by devising an axiomatic theory that offers a possible explanation for the observable data.[346] Instead of calling this »method deductivism«, one is equally (or even better) justified in calling it »abductivism«, for abduction is precisely the development of a the-

dividual judgements about the future, therefore being very unreliable. See Reinhardt (2009).

344 This is, of course, a typical situation with any paradigm shift. Lawson is aware of this and therefore mainly addresses those who accept that the economic mainstream is in inescapable disarray. Note that Lawson does not intend to use his ontological research for building an alternative economics by himself. Rather, he wants to support existing heterodox schools by showing that their foundations are ontologically more realistic than those of mainstream economics. His project is essentially about improving heterodox economics by reinterpreting and refining their presupposed ontological commitments. See part III of Lawson (2003).

345 See Vromen (2004) pointing to Friedman (1953), p. 33.

346 See Reiss (2004) for a similar view. In a response, Lawson steps back from his claim that formalistic modelling is principally opposed to the search for deeper structures and waters down his position to the thesis that not all essential structures can be adequately treated by deductive modelling. (See Lawson (2004b), p. 337.) This is, however, not in contrast with how mainstream economists would define their project. Only the most dogmatic economist would insist that formal modelling captures all underlying structures and is the *only* way to go.

ory for explaining facts.[347] Mainstream economists of course reject the interpretation that they have found and even that they should find true and essentially realistic underlying structures as Lawson demands.

There is another confusing point about Lawson's sharp distinction between underlying structures and event regularities: If one accepts (as I have argued most economists do), that scientific laws are not about event regularities but rather about the underlying structures that cause them, one can still stick digging for event regularities by arguing that an underlying structure must *somehow* show up in the empirical data – if it were completely hidden, how could we ever be justified to speculate about it? This seems to imply that Lawson may be much closer to mainstream methodology that he is willing to admit: One of his main points is the denial of strict event regularities in an open system, which he takes as an argument against deductivist modelling. He prefers to talk about demi-regularities. Now, it is hard to believe that mainstream economists would really insist on the strictness of the regularities in question and would reject searching for demi-regularities – Daniel Hausman's characterisation of economic laws as tendency laws may indicate that the mainstream view is not as distinct from Lawson's view as he says it is.

To sum up, there are many points of mainstream methodology that resemble aspects of Lawson's critical realism, but it is precisely the realist aspects of his methodology that do not show why realism is justified or preferable to a more modest methodology. Simply put, Lawson commits the naïve realist fallacy that believes higher realisticness (even if seen as descriptive accuracy of the assumptions) should be an end in itself, and by this excludes many forms of theorising that are commonly accepted to be useful or successful. This is the main reason why his plea for more realism is pragmatically unconvincing.

4.2.1.5. Lessons Learned

The foregoing sections about realism covered a lot of ground. Now it is time to step back and draw conclusions: Is realism helpful for theory appraisal in economics? It should not surprise the reader at this point that I answer this question negatively. If scientists (in contrast to philosophers) want to assess theories, they want to know how *well* they work, not *why*.[348] The ongoing battle between realism and anti-realism in traditional epistemology can be completely separated from issues pertaining to theory appraisal. Even if

[347] It is questionable whether abduction is really antagonistic to deduction as a scientific method as Lawson portrays it. In scientific practice, all three modes of inference (induction, deduction, abduction) exist on a par – and no one is likely to call this inconsistent. This becomes particularly clear in Hodge (2007), p. 23.

[348] According to Goodman, scientists don't even look for truth, but for system, simplicity and scope. See Goodman (1978), p. 18. My position is more modest, as it entails no claim of what scientists are looking for.

there were a conclusive proof in favour of scientific realism, this would still allow for a purely instrumental way of assessing theories, i. e. deciding how well they are suited for solving given problems, since this question can be completely separated from their truth-status.[349] Put slightly differently, if one wants to make normative statements, pragmatic reasons are needed; however, as I tried to show above, it is difficult to defend realism on pragmatic grounds, as adopting realism does not lead to normative implications that are unavailable to the anti-realist.

I argued that the realists neither successfully justify the legitimacy of talking about features of an external reality nor explain why empirical adequacy for given problems is not enough. Remarkably, all realistic positions discussed include confessions for fallibility. If we take those confessions seriously, strong epistemic realism collapses to weak epistemic realism, and anti-realism loses its main enemy: If the realists are indeed convicted of fallibility there is little point in their distinctive claims. Why should they stress the importance of truth as a means for fighting against bad theory when they admit that truth is not even a viable criterion because everything is fallible? If they admited that, realism would have no special normative thrust. This is one of the main conclusions from my discussion of realism: Strong epistemic realism cannot be justified, and all other forms are fine for the anti-realist.

Such weaknesses notwithstanding, it should be clear that this does not imply that there is nothing acceptable in the realist's prescriptions, even if they stem from the wrong reasons. For example, within the assumption debate, the realists carefully distinguish between assumptions that isolate real factors and others that merely serve the tractability of economic theory. A certain type of anti-realism may well accept the message that it is important to filter out the crucial, the fundamental or the necessary assumptions of a theory even if it would hesitate to call them real. Such a procedure could be called »anti-realist ontology« as it is a venture into the status of the very fundamentals of economics – and by this it would retain the lessons from one of the realists' preferred projects without committing to a version of ontological realism.

Another possible form of anti-realism may even agree with Mäki's recommendation of developing useful surrogate models for analysing the real world instead of playing with substitutes; but in contrast to Mäki, the anti-realist would not ask whether a model is representing »the real world« but would focus instead on its ability to shed light on real problems. If the problem to be solved is one of policy-consulting, it should be clear even to the anti-realist that researching the formal aspects of a general-equilibrium model can

349 Note that this does not necessarily entail the claim that the realism debate is about a pseudo-problem. It might be a valid philosophical problem, but it can be separated from theory evaluation.

become a dangerous substitute for practically relevant economic research.[350] However, if some formal aspects are indeed the problem a scientist wants to deal with, the anti-realist must accept this and cannot urge her to concentrate on surrogate models. A type of anti-realism could indeed accept a kind of »as-if realism« that accepts many arguments and terminological points but rejects the interpretation that theories – or parts of them – are literally true.[351] With this in mind, the anti-realist could actually talk about more »realistic« assumptions when he uses a coherence theory of justification instead of a correspondence theory of truth.[352] The debate about realism against anti-realism would then be merely semantic, bearing no pragmatic implications whatsoever. The more realistic assumptions would then be the ones that fit better to the totality of our current beliefs.[353] It is, however, another main point (one which I have consistently argued for above), that more realistic assumptions are not always better ones; we should look for adequate idealisations for the problem at hand instead of mechanically heading towards more realisticness. If one accepts these arguments, it is difficult to defend realism pragmatically as a critical therapy for economics. There are forms of anti-realism that can do the same thing but are far more epistemologically modest concerning the ontological status of theories.

In the next section, I will try to sketch out the main characteristics of a methodology that is well suited for theory appraisal and, though modestly instrumentalist in nature, could be even accepted by realists, because it suspends judgement on the truth-status of theories and therefore would neither recommend nor rule out a realistic interpretation.

4.3. What is Left for Theory Evaluation?

At this point, it is time to sum up the results we have found so far; I have argued that several methodological approaches are not useful for theory appraisal. The empiricist claims were rejected mainly for two interdependent

350 The only point of normative difference between this form of instrumentalism and Mäki's recommendation would occur in the unlikely case of a substitute model that generates perfectly correct predictions. In any other case the instrumentalist, too, must look for a better model, and she would be badly advised of doing so by treating all possible assumptions as equally useful. Hence, it is not enough for the instrumentalist to generate merely »testable« predictions when there is a demand for increasing success in tests.

351 A similar argument is made by Carnap (1950).

352 The addition of a semantic correspondence theory of truth to a coherence theory of justification is in fact the only feature that clearly distinguishes Mäki's realism from the anti-realism presented here. See Peter (2001).

353 This suggestion is inspired by Quine/Ullian (1970).

reasons: On the one hand, they ignore the peculiarities of the economic research programme, and on the other hand, they can be easily employed in other research programmes – like psychology or sociology – that tackle with economic questions from a different perspective.[354] The postmodernist methodologies were rejected because they are either completely relativistic, and therefore useless for theory appraisal, or they promote standards (such as McCloskey's persuasiveness) that are not convincing for anyone except the postmodernists themselves. Finally, the realists were not successful in showing why »truth« is a helpful criterion for theory evaluation.

The pragmatic approaches discussed in this section seem to make the most promising candidate for theory appraisal in economics. While they are open to different methods, they do not let in everything. The most important aspect of a pragmatic methodology is taking problem-orientation seriously. In a pragmatic framework, everything that can contribute to the solution of a given problem is acceptable.[355] This requires, of course, an honest description of the problem at hand. Much of formalistic research in economics can be saved from methodological attack when we accept that the question of whether equilibrium is possible under specific formal conditions is a valid problem.[356] By this move, dogmatic claims like those of the empiristic, realistic or rhetorical schools are ruled out as general approaches: As I argued in section 2.3.2.5, formal approaches can be justified in their own right.[357] In many cases, however, economists want to do more than purely formal work: they want to explain and even predict the social world. If this is indeed the aim, problem-focused empirical adequacy is the minimum constraint. A pragmatic methodology does *not* entail the claim that theories are nothing but tools for solving practical problems. Theories can have epistemic value in being empirically adequate answers to very abstract questions.[358] It is important here to restrict empirical adequacy to a given problem, because using empirical adequacy as a general criterion presupposes a clear separation between

[354] This is in fact what happens under the label of behavioural or experimental economics.
[355] Remember that this includes even highly theoretical problems, so that the pragmatism I endorse becomes by no means »atheoretical« or plain.
[356] Some formalistic models in decision theory can be justified against empiricist charges by their *normative* character. This occurs more often in philosophy than in economic models.
[357] The enemies of formalism deny, of course, that solving formal problems is a valid field for research. The rejection of the relevance of a problem is, however, something that cannot be rationally argued for.
[358] Note that the demand for explanation precludes any recommendation to achieve empirical adequacy by mere statistical correlation processing.

what counts as observable (i. e. empirical) and what does not.[359] This separation is notoriously difficult to draw, but fortunately we do not need to draw it for pragmatic theory evaluation. The separation of unobservables from observables is critical if one wants to draw a clear distinction between the epistemic and the pragmatic, as van Fraassen does. As shown in section 4.1.4.1, van Fraassen argues that »observing« is mostly free from theory, whereas »observing, that« is not. Many philosophers of science attack van Fraassen on this separation when they argue that there is no such a thing as theory-free observation. In the pragmatic framework that I suggest is best suited for theory evaluation in economics, theory-free observation is not an issue, because I accept that observation always happens under the framing of a given problem – if the problem is given in some detail, this already defines what counts as empirical data and what does not. For example, when the problem is determining influence factors for economic growth, the GDP counts as observable data – as unobservable as it might seem from a classic perspective, whose adherents might even wonder if entities such as the GDP exist at all. Economists are quite free even to introduce new »observables« when they specify where they get their data from. An empirically adequate theory in the pragmatic sense is therefore *not* one that matches the »sense data« of »pure observation«[360], but one that gives a coherent picture of data and theory that is accepted to be useful for solving a problem that existed *before* the theory was formulated. If this is the case, it poses no harmful difficulty when the data are infected to some degree by theory.

The point can be further generalised: Taking underdetermination seriously, a pragmatic methodology refrains from talking about truth and therefore has no problem with intertwining the pragmatic with the epistemic. We do not need problem-independent knowledge (the realists can have that – if they manage to show what it is, how we achieve it and how we know that we've got it). The pragmatic approach seems very permissive at this point, because it will apparently count the solution of any and even the most absurd problem as success. But the ranking of problems is surely beyond the capabilities of any philosophy of science, because it is something that underlies the judgement of individuals and cannot be answered generally. This is the point where holism enters: The recognition of progress is always dependent on an existing web of beliefs. People may have their individual problems and solutions to them, but as long as there is no community that shares their beliefs, the progress made becomes non-communicable and is therefore irrelevant.

359 See Klee (1997), p. 229.

360 Note that these standards are obsolete since the demise of logical positivism. Yet they are often attacked and claimed to be the basis of any form of empiricism. This is, however, a straw-man fallacy, as my above argument shows.

In Western science, there is an overarching meta-paradigm that roughly defines what counts as a problem and what solutions look like. The »scientific worldview« is nothing more and nothing less than a shared web of beliefs (or values, as Kuhn has it). In section 4.1.4.1 I argued with Laudan that paradigm-shifts under this meta-paradigm can happen quite rationally and quickly. In Western science, the most prominent, overarching standard for empirical theories is currently predictive success.[361] If there were a new but seemingly crazy theory of labour economics that performed better than any other in predicting the net effects of reforms to the labour market, it would soon be used broadly, no matter how crazy it had seemed at first sight.[362]

However, when there is no clear winner in terms of prediction, or when prediction is not even the aim,[363] the methodological discussion gets more complicated. The history of methodology has shown that the postulation of additional criteria is not likely to settle the argument. This is why the pragmatic approach achieves the maximum one can hope for in philosophy of science, since true knowledge of the right rules for good science is unachievable and the proposal of rules that are supposed to characterise good science by definition is of little help. [364] Escaping to relativism is no solution but a mere denial of the project. This is why a pragmatic means-ends analysis of

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[361] Note that the criterion says nothing about the reliability and precision of predictive success. In economics, the most we can expect nowadays is conditioned prediction of tendencies of aggregated values. Nonetheless, those predictions can be empirically adequate (See Schmid (2005), section 4). If we had even less than that, economics could no longer claim to be an empirical science, as the notion of an empirical science is tied to some – however inexact – degree of predictive success. At least rough tendency predictions must be possible – this is the core of Gibbard/Varian (1978), which characterises economic models as caricatures (See section 4.1.4.2.2.) Note also that »prediction« of past events, i. e. retrodiction, is meant to be included in the notion of prediction here.

[362] This is the main reason why I refrain from concrete prescriptions in my work: Good theories can speak for themselves and do not need methodological sermons. E. g. if Hans Albert believes that more sociological work is needed in economics, he must explain why the sociological theories that deal with economic phenomena are far from being a success story. Note that all this does not deny that including data from experiments can very well lead to an increase in predictive success.

[363] Remember that a large class of economic models aims not at prediction but merely at offering new partial, potential invisible-hand explanations for certain observable phenomena. See Aydinonat (2008).

[364] The justification of methodological rules and their results by means of a reflective equilibrium would lie between those two extremes, but it is doubtful if such an equilibrium is generally achievable or stable over time. Some of the most generally acceptable criteria are listed in section 4.1.4.3.

theories seems the most promising approach, if indeed »only« theory evaluation is at issue.

For this pragmatic approach to be workable, a detailed and honest description of the problem, including the expectations for a solution, is required. In other words, the domain of theory must be clearly specified.[365] The first step is classifying a theory as an empirical theory. Only if this is done can the overarching criterion of predictive success be legitimately employed. Note that theories that are not empirical can be of great help for the development of empirical theories. Nonetheless, economics must be considered a partly empirical science if it claims to tell us something about the social world. Where does pragmatic theory evaluation lead us, here?

Suppose some schools of thought in current economics – such as behavioural economics, computational economics or classic rational-choice economics – have no clear winner in terms of prediction. Behavioural economics is probably best at predicting the behaviour of individuals in precisely specified situations; computational economics beats the others, e. g. when it comes to predicting spatial patterns and clustering processes. Rational-choice economics has advantages when deducing the long-term macro-effects of policy reforms. A refined description of problems is a helpful start to a means-ends analysis. Does the respective model live up to its own standards, or does it promise something it cannot fulfil?[366] If behavioural economists claim to replace the neoclassical mainstream, they cannot argue with the higher descriptive accuracy of their fundamental assumptions or with the increase in predictive power of individual decision-making. They must also show why their framework is better (or at least equally well) suited for solving the problems that can be solved by rational-choice reasoning.[367] This exemplifies again how the epistemic is intertwined with the pragmatic: As long as there is no well-confirmed bridge between theories that operate at a lower level and others which work with highly aggregated and long-term data – i. e. if the more abstract theory (rational choice) cannot be reduced to the more concrete (behavioural economics) – there is no problem-independent way of deciding between them. An extreme form of optimism is needed for believing that we are anywhere near a successful reduction of standard economic theory

365 Remember Marcel Boumans' point made in section 4.1.4.2.2.

366 Remember Nancy Cartwright's argument that the idealisations of economic models are often of the wrong kind, i. e. badly chosen for the promises they make. See section 4.1.4.2.1..

367 Remember the point made by several authors – Vilks, Hindriks and Schliesser, as discussed in section 4.1.4.2.2. – who all argue that learning from models based on counterfactual assumptions is possible. Homann/Suchanek (2000) transcend the issue because they reject the notion that basic assumptions in economics are to be judged in terms of their realisticness. See section 4.1.4.2.3.

to behavioural terms. Additionally, as I argued in sections 4.1.2 and 4.1.3, there are even arguments for using the more abstract theory for some problems even if it was constructively falsified by or reduced to a lower-level theory: For some problems the lower-level theory may be more complicated without delivering a decisive increase in precision.

On top of that, there are epistemic advantages of pragmatic criteria, such as simplicity and tractability: Some important effects easily get out of sight when using a theory that is »too far away from the problem«. This would only not be an issue if we had a workable »theory of everything«;[368] as long as such a »theory of everything« is not available, theories must be pragmatically chosen for solving given problems, whether one is dealing with highly theoretical problems of fundamental research or giving policy advice. This shows that classic epistemic goals such as unification are *not* beyond the reach of a pragmatic framework, even if any claim of an exclusive (problem-independent) status of such goals is ruled out.

When it comes to judging single models, the relevant question is whether or not it solves the problem it promised to solve. This can be checked afterwards by a kind of market test: If the theory or model is used and accepted in practice (which includes other theories as well), this is a strong argument in favour of it.[369]

When whole research programmes are at issue, there is no single objective criterion to judge them, either. Past success does not tell much about the current state of a research programme. Rather it is its fruitfulness that is decisive, i.e. the potential of a research heuristic to generate theories or models that will solve a set of different problems.[370] Such an assessment cannot be given by an algorithm – which is a good thing, actually, because differing judgements about the future lead to scientific pluralism and hence to competition in the marketplace of ideas, which is epistemically superior to any form of

368 The advocates of a strong notion of »emergence« between low-level and high-level theories argue that such a »theory of everything« is impossible to achieve even in principle, whereas those advocating a weak notion of emergence argue that a correct »theory of everything« would eliminate all emergent phenomena. As the strong view presupposes knowledge about the impossibility of future theories, I shall stick with the second view. Note that an even weaker notion of emergence would still hold even if we had a »theory of everything«: We may want *explanations* at the higher level even if all phenomena could be reduced to movements of elementary particles. See Stöckler (2008) for this so-called explanatory-pragmatic theory of emergence.

369 See Ahrweiler/Gilbert (2005) for a discussion and an interesting application of this criterion.

370 Remember the discussion in section 4.1.4.2.2. and 4.1.4.2.3. where several authors argued how economic models fruitfully enable learning from experience in the messy domain of the social sciences and thereby may help to solve practical problems.

centralism.[371] The past success of a research programme for empirical theories could be evaluated by its success in generating theories that successfully generated solutions to their problems. This would be nothing less than a means-end analysis on the meta-level, but here incommensurability is a major obstacle: Whether the neoclassical research programme scores high on this test is hard to tell; there are supporters and opponents who argue about the fundamental concepts, and each group claims to be superior in terms of empirical adequacy.

But as long as there is no other research programme that is clearly better off, we do not *need* final judgement, as the supporters are free to proceed with their programme and the opponents can continue looking for alternatives. The empirical underdetermination of theories shows once again why *true* theories are so hard to achieve in economics: If we are not even sure of our judgement about empirical adequacy, how can we know whether theories are true?

Is there any way to assess the future fruitfulness of a research programme, i. e. to give a means-ends analysis of its heuristic? We could allow for induction and accept that research programmes that were successful in the past will stay so in the future. This is, however, a risky step that need not be taken. We can leave the judgement to the process of competition in the scientific community. The time of rule-based methodologies is over, and this means that philosophers cannot claim to prescribe what is »scientific« and what is »unscientific« from an external point of view; rather, they need to connect to practicing scientists with their arguments.

Therefore, the most important tasks left for philosophers are, on the one hand, criticising fundamental points that may be overlooked by practitioners and, on the other hand, clearing up misunderstandings by increasing the conceptual precision of scientific debates – and in that way improving the quality of scientific competition.[372] Of course philosophers are free to sketch out their own ideas or offer suggestions for research, but in these regards they have no general advantage over practicing scientists. Finally, philosophers can productively contribute to the methodological discussion by attacking the views of their colleagues who want to give prescriptions that are deduced from philosophical convictions without paying respect to the peculiarities of the science they deal with.[373]

As I showed in section 4.1.3, *economics* can contribute its mite to methodology by analysing the efficiency of scientific institutions and suggesting reforms. So while external, prescriptive suggestions are ruled out in the prag-

371 See Hayek (1969) for a classic argument.

372 The methodological analysis of the misled confrontation between behavioural and mainstream economics is a good example of this. I will elaborate more closely on the remaining role for philosophy in section 5.2.

373 This point was discussed at the beginning of section 4.1.4.2.1.----

matic framework, there is still room for normativity on the meta-level of institutional reform or on criticising fundamental issues.

After the philosophical characterisation of a pragmatic version of theory evaluation, it will be helpful to briefly summarize and then proceed with some case studies. It should have become clear that my version of pragmatic theory evaluation is quite pluralistic, but there are still cases left which call for criticism. An economic model is to be pragmatically criticised in the following cases:

1. There is no problem that is solved, but only a description of an economic situation, couched in theoretical terms. Such models are merely reproductions of their input.
2. A problem is only seemingly solved. In economic models, there is often a (sometimes implicit) claim about empirical adequacy which gets never substantiated. This has often been attacked as formalism: Pure formal approaches are given credit to solve empirical problems without any further justification.[374]
3. The problem that is adequately solved seems irrelevant to anyone but the author of the solution. While such projects are internally OK, it is doubtful whether they are tackling with something that can be legitimately called a problem at all.[375]

In the final, conclusive section I will present some case studies in order to show how pragmatic theory evaluation can be put into practice.

[374] As I said before, formalism is fine when dealing with formal aspects. When it aspires to something greater, this must be justified empirically.

[375] It is hard to find such cases, because usually there are strong claims of relevance in every paper – which is why criticism tends to concentrate on cases 1 and 2. If a problem is really irrelevant, it tends to get completely ignored.

5. CONCLUSIONS

The conclusions of this thesis are threefold: First, I shall summarise the methodological conclusions and illustrate them with some exemplary cases. After that, I will reflect on how a normative role can be justified for methodological works. Finally, I will refute some doubts critics might raise against my pragmatic framework for theory evaluation.

5.1. Case Studies of Pragmatic Theory Appraisal in Economics

As should be obvious by now, I am promoting a rather defensive methodology that neither prescribes generally what economists should do nor criticises economics as a whole. Economics is a highly complex science, and it should be clear that we cannot expect precise, unconditioned predictions here. This does not make economics a bad science but a rather normal one: just think of biology, which is a perfectly acceptable science that makes vague predictions at best. Therefore, mainstream economics and the rational-choice approach should not be attacked on the basis that they make relatively vague predictions.

Again, I refrain here from general claims that say economics is »too formal« or that it must become »more empirical«. There is no way to know the right balance of formal, empirical and applied work. In this context it is important to notice that the discontent of many methodologists with the amount of formal research in academic economics often does not consider the huge amount of empirical and applied work in government departments, companies, banks and journalism.[1]

Of course, the current mainstream in economics should neither be defended nor attacked dogmatically – but it should not be rejected too easily, either, because a lack of defence by general appeals to pluralism is methodologically dangerous, as it can lead to a vague acceptance of everything without thinking about reasons. And this would mean nothing but a lack of standards and intellectual honesty.[2]

Even if I were to spend considerable time defending mainstream economics, of course heterodox approaches like behavioural economics, or even com-

1 Of course there is also scientific literature on how practitioners work. Mostly, they refrain from theorising and rely on statistical methods. See Sattler/Nitschke (2001) for a comparison of different approaches to estimate optimal prices which do not make the assumption of price being a function of demand.

2 See Colander (2007) for a similar point.

pletely distinct fields like sociology and psychology, are free – and should even be encouraged – to try to beat the economic mainstream.[3] While the mainstream rational-choice approach has often been identified with very abstract and purely formal models that have little relevance for practical problems, Homann and Suchaneck delivered a reinterpretation by their characterisation of the economic man as a heuristic device, which shows this does not need to be the case. So what often has been criticised as too formal (and therefore deemed irrelevant) can be justified as conceptual exploration that delivers a framework for more empirical research. Rational-choice economics and behavioural economics are not necessarily opposed to each other as research directions – as might seem to be the case at first sight – but can fruitfully contribute to better modelling when they understand their methodological status. Where the rational-choice school is dedicated to explaining and predicting changes in macro-rates as reactions to shifts in relative prices, behavioural economists look at the empirical details of individual human behaviour. There is plenty of room for applied research, which can have the power to change established ways of modelling in the long run.[4] To sum up, there is no a priori argument that can decide whether more empirical or more theoretical research is needed, so there is not much to say about theory evaluation at the global level. Therefore, it is helpful to focus on more specific models. Still, it is impossible to criticise a model from an independent standpoint, but nonetheless there remains much room for constructive criticism by focusing on the standards set by the problem in hand and the way it is approached. The main point for criticism is therefore not the model by itself but its incorrect use or exaggerated and unjustified interpretation. In short, the hallmark of a model is whether it really achieves what it claims to do.

With the criteria given at the end of the forgoing section at hand, I shall now present some criticism of economic models along these lines.

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3 Daniel Hausman described the state of economics in the early 1990s nicely: »What is wrong with economic theorizing is not what economists are doing, but what they are not doing and what they refuse to do.« (Hausman (1992a), p. 255.) I believe with the spread of rational choice methodology to sociologists, the growing acceptance of experiments and survey-data in economics and the widespread acknowledgement of the relevance of institutions, the situation has greatly improved since then.

4 See Carrier (2004) for a concise article on this point. See Cartwright (1983) for a classic account against the view that useful models can be derived from an overarching theory. In her view, even physical laws are *false* in their pure form.

5.1.1. First Case: Inadequate Assumptions

The first case makes a model which aims at policy consulting and deals with the question of how to distribute investments into education.[5] The model is an agent-based computer simulation where »policy makers« (in this case the economists who made the model) can decide whether to invest in the education system evenly in two regions or spend all the money in one region and completely neglect the other one. In the language of the model, distributing investments evenly lifts both regions from low skill levels to medium levels, and focusing the money on one region lifts the region invested in to a »high-skilled region« while the other region stays at the low level. The model simulates the labour market and goods market, including commuting costs, in the two regions at a fairly complex level, but a more detailed description of the model's assumptions is not necessary to describe the central problem of the paper.

The main conclusion is summarised by the authors as follows: »if commuting costs are positive but low, than [sic] a spatially concentrated policy [of investments into education] performs better than a uniform approach«.[6] However, this thesis is not well argued for in the paper. After describing the model, it is said to »reproduce several stylised facts«[7]. But the »remarkable finding« that the existence of commuting costs can actually increase the overall output results crucially from the assumption that the model allows for firms employing higher-skilled workers and paying them higher salaries even when they are not any more productive than lower-skilled workers at the beginning. This goes against standard economic reasoning and is surely not accepted as a stylised fact.[8] Yet this assumption is responsible for the conclusion that there is higher growth when education investments are distributed unequally, because the lower-skilled region can produce the same goods at lower prices – and this starts off a number of self-reinforcing effects which lead to continuous growth in the *lower-skilled* region and increasing unemployment in the higher-skilled region.[9]

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5 See Dawid, et al. (2009).

6 Dawid, et al. (2009), 4.1.

7 See Dawid, et al. (2009), 1.0.

8 Following the so-called efficiency wage theory, it is a stylised fact that firms pay wages in excess of market clearing – but only if this reduces the individual firms' production costs, because they can attract a more productive workforce by this. This second condition is ill-represented in the model, because the higher-skilled workers cannot use their skills for being more productive initially, so they become merely a cost factor.

9 See Dawid, et al. (2009), 3.12. Note that the existence of commuting costs is necessary to separate the regions at all. With commuting costs at zero, there is only one global labour market.

This model gives us a clear case of a policy recommendation that is drawn too quickly. Remarkably, in the pre-print version the authors recommended spatially concentrated investments into education, even if this actually harms the region that gets the investments (but leads to higher total growth).[10]

Even if models cannot (and should not) be complete, they must be *adequately* simplified. The model described here is designed in such a way that in some situations firms are unable to profit from the benefits of higher-skilled workers and nonetheless pay higher salaries to them – this seems like a fundamental flaw and not an adequate idealisation when the effects of education to output are the problem tackled with.

Criticism of this model is possible without referring to empiricist standards of theory evaluation. The main flaw of the model can be detected by plausibility considerations that the authors apparently did not make. Sugden's credibility criterion applies here: The assumption that firms employ higher-skilled workers even if this is to their long-run disadvantage does not credibly fit with our current knowledge of the world, *and* it leads to the strange prediction that firms with lower-skilled workers are better off in the long run.

By concentrating only on their model, the authors are surprised by its predictions instead of critically assessing them. As explained in section 2.3.2.4, agent-based models are not well suited for predicting but can only be used for explaining why certain observable results or patterns come about. If the observable patterns are *not* reproduced by the model or if there are no observable patterns that could be compared with the model-results, our trust in agent-based models should be rather low.

A model that is wrong in both its assumptions and its predictions is especially hard to justify if it is not a general concept (such as rational choice) but made for predicting the outcomes of quite specific situations – as is the case with the model discussed here.

Note that neither a claim for tougher falsification nor a demand for more empirical work at the basis was needed to criticise this model.

5.1.2. Second Case: Over-Interpretation

Flawed models like the one described above are hopefully the exception rather than the rule. More often, the model seems prima facie alright and does not produce »remarkable« outputs that can be easily traced to ill-chosen assumptions. In many cases the problems arise one step later, when it comes to the *interpretation* of the results. The following case made by Milberg and Spiegler

[10] See Dawid, et al. (2009), 3.12. (In the published version there is no direct policy recommendation.)

shows this very well.[11] Milberg and Spiegler propose a four-part framework for discussing economic models, which they summarise as follows:

1. »Delimiting, in which the set of social phenomena under study is delimited and a research question is formed;
2. Naming, in which a mathematical construct meant to be analogous to the social phenomena is introduced, along with a »catalog of correspondences« which links elements of the construct with elements of the phenomena under study;
3. Solution, in which the mathematical construct is brought to a solution;
4. Interpretation, in which the mathematical solution and its implications are interpreted with respect to the research question. Empirical testing of the interpretation is also a part of this phase.«[12]

In the cited paper, Milberg and Spiegler discuss three models using the framework described above. While they see no problems with the first and second step of model building, they identify major difficulties in third and fourth phase. They argue that economists tend to overstate their case when interpreting their models. This over-interpretation results from the careless identification of purely mathematical systems with their socially understood counterparts. Throughout their arguments, economists tend to conflate ordinary language and mathematical symbols, which increases the reader's tendency (and probably the author's as well) to forget about the limits and the narrowness of the mathematical representation. Milberg and Spiegler use Acemoglu/James 2006 as the main example for their criticism. Acemoglu and James set the question of why some countries are democracies and others are not as their problem to solve.[13] This is a fairly broad question, and it generates an impression of relevance; they present the formal structure of their model in combination with ordinary terms and a suggestive story.[14] In Mary Hesse's terms, they are concentrating only on the positive analogies – the ones in which the model can be meaningfully transferred to the problem.[15] The conflation of ordinary language terms and mathematical symbols continues in the solution phase.[16] The interpretation phase largely consists of comparing historical case studies to the ordinary-language version of the model. What Acemoglu and James actually do is not answer the question of why countries become democracies but show that a formalised model can be interpreted in such a way that it fits some case studies by analogy. Note that this is not a bad

11 See Milberg/Spiegler (2008).
12 Milberg/Spiegler (2008), p. 8.
13 See Acemoglu/James (2006), p. xi.
14 See Acemoglu/James (2006), p. xii.
15 See Hesse (1963).
16 See Acemoglu/James (2006), p. 185.

thing to do: As I argued above, much of economic reasoning consists in devising a theory for reconstructing the facts. The problem is that economists are often far too confident that their models *actually* describe the real driving forces of phenomena under scrutiny, even if all they can show is the mere *possibility* that their model describes forces that can be found in the social world. Such overstatements of model results may be due to the demand for »policy relevant« outputs.[17] But this merely *explains* the overstatement – it does not justify it. Over-interpretation of economic models can lead to incorrect policy decisions that are hard to reverse, even when the model becomes scientifically obsolete. E. g., Easterly shows how the outdated Harrod-Domar model (see section 2.4.1) is still used today in foreign aid policy, leading to grossly inaccurate predictions.[18] It seems that Friedman's recommendation to tie the domain specification – which precisely defines under which circumstances a model works – inherently to the model[19] would indeed be a step towards more honesty, as the over-interpretation of models seems rather the rule than the exception.[20]

The criticism of Acemoglu/James 2006 showed once again how theory evaluation is possible even if one does not employ the classic empiricist standards.

5.1.3. Third Case: Other Problems with Interpretation

There are many papers in which the presented model is not overinterpreted but rather not interpreted at all. E. g. Etro 2004 tells a general-equilibrium-inspired story about patent races, but gives no hint as to how his models should be compared to the social world. Instead, he merely ends the paper with the standard disclaimer that »much further theoretical and empirical research is needed«.[21] Of course, this is less harmful than over-interpretation is, but the interpretative language that is used throughout the paper actually suggests a stronger conclusion, which the authors finally hesitate to draw. This further reinforces the impression that the crucial point of economic models often does not lie in the models themselves but in their interpretation.[22]

The real problem with formalism is therefore *not* the obsession with mathematical exploration of rational-choice theory or equilibrium analysis and

[17] See Milberg (1996).
[18] See Easterly (2002), p. 6. and p. 22.
[19] See Friedman (1953), p. 15.
[20] See Rodrik (2009). Note that over-interpretation is usually not adopted by the media, which remains quite sceptical when it comes to the assessment of economic models.
[21] Etro (2004), p. 302.
[22] See Frigg/Hartmann (2006) section 3.2.

the neglecting of empirical data. The problem is the unclear status of such formalist analysis. During the 1950s it became common practice among economists to discuss purely conceptual problems, under headings that suggested occupation with empirical questions that in fact never occured.[23] To avoid the resulting confusions, economists should pay more attention to making the methodological status of their models clear. Are they purely formal? Are they a kind of conceptual exploration? Are they making empirical claims? Which empirical claims do they make, exactly? How are they justified? Such reflections are lacking in many economic publications, which makes economics an easy target for those who do not adhere to the principle of charity. E. g., much of the famous criticism of Nicholas Kaldor against equilibrium theorising results from the unclear status of equilibrium models. If economists had been honest, they would have admitted the non-empirical nature of their project. As Kaldor argues, many economists became so fascinated with equilibrium theory that they believed the technical refinement of its highly formalised models would ultimately lead to empirical success.[24] This optimism was hardly justifiable, and indeed turned out to be wrong – if we owe anything to the refinement of equilibrium theory, it is not empirical adequacy but rather conceptual clarity. In section 2.4.1 I showed in a case study how empirical progress was achieved in economic-growth theory and how the first step was actually a turn away from seeing the economy in constant equilibrium, as was the case with Harrod-Domar models. After that, empirically measurable factors such as human capital or institutional settings could be integrated.

Again, in this case study I do not claim that the authors should work more empirically, but rather that they should be more clear about which problem they are solving and which conclusions can be drawn from their paper.

5.1.4. Fourth Case: No Problem is Solved

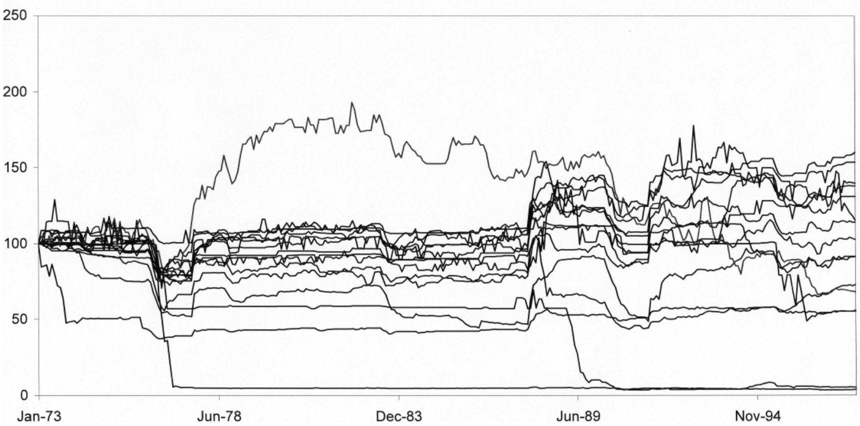
I will conclude this selection of case studies by discussing a model that falls in the first class of the criteria given at the end of section 4.3.: The model may be empirically adequate in many dimensions, but it is doubtful if it solves any problem at all. Edmonds/Moss 2004 aims at outlining a new methodology for agent-based simulations that stresses the need for descriptively accurate assumptions (remember their catchphrase »Keep it descriptive, stupid« (KIDS). At the end of the paper the authors outline a simulation model for

23 Hausman makes a similar point using the better-known example of Samuelson's consumption loan model. See Samuelson (1958) and Hausman (1992a), p. 255 et sqq.

24 See Kaldor (1985), p. 60 et sqq.

aggregate water demand in a region of Great Britain. Here is a short summary of the model: Agents are distributed at random on a grid. Each agent represents a household and is allocated a set of water-consuming devices, in such a way that the distribution resembles empirically found data from the mid-Thames region. The households are influenced in their usage of water-consuming devices by several sources: »Their neighbours and particularly the neighbour most similar to themselves (for publicly observable appliances); the policy agent[25]; what they themselves did in the past; and occasionally the new kinds of appliances that are available (in this case power showers, or watersaving washing machines). The individual household's demands are summed to give the aggregate demand.«[26] There is no need to explain the model in full detail here; the main methodological point can be made by looking at the outcome of many runs of the model starting with the same initial conditions:

Fig. 1: Aggregate Water demand from model runs specified in Edmonds/Moss 2004, p. 10.



Moss and Edmonds note that »significant events include the droughts of 1976 and 1990, which often show up in a (temporarily) reduced water demand, due to agents taking the advice from the policy agent to use less water. Power showers become available in early 1988 and water-saving washing machines in late 1992 which can cause a sudden increase or decrease respectively.«[27]

25 The policy agent suggests a lower usage of water if there is less than a critical amount of rain during a month. This influences the agents to a certain degree.

26 Edmonds/Moss (2004), p. 8.

27 Edmonds/Moss (2004), p. 10.

It is hard to see how this simulation gives any insights at all. The only recognisable effects result from the external shocks that are programmed into the model – and even those are difficult to identify. Additionally, it is doubtful whether the authors achieved their own goal of setting up descriptively adequate assumptions, as there are certainly more factors influencing people's demand for water than those integrated into the model. Moreover, the runs depicted above all start from the same specification and yet widely diverge in their outcomes. It is hard to learn anything from the model, as any outcome is possible – from very low water demand to very high and from great changes in water demand to nearly constant consumption. The authors write that the model is made to capture the *range* of water-demand responses.[28] They indeed show that there is a very wide range, but they fail to show the different results come about. By trying to create a descriptively adequate model, Moss and Edmonds arrive at »a complex model whose behaviour is not fully understood«,[29] which therefore neither explains matters nor succeeds in being descriptively adequate on both the assumption and the implication sides.

The discussion of the Edmonds and Moss model has shown once again that a problem-oriented criticism is much more fruitful for complex economic models than is schematically applying empiricist standards.

5.1.5. Conclusions from the Case Studies

From the four case studies I have briefly outlined here it should have become clear that economic models can be productively criticised by examining whether or not they provide solutions to the problems they set out to solve. There is no need to argue *generally* for more empiricism in economics, for rhetorical analysis, for realism or even for relativism. Productive and helpful criticism can be based solely on the question of whether or not a model is a helpful contribution for solving a problem. In the next section I will reflect on the implications this has for the role of methodological reasoning and its relation to the sciences.

5.2. Reflections on the Role for Methodology

If theory evaluation is executed in the way it was done in the preceding chapter, what role is left for philosophy? Certainly the old days, when philosophers of science judged good science from bad science by an external crite-

28 See Edmonds/Moss (2004), p. 10.
29 Edmonds/Moss (2004), p. 8.

rion, have passed. However, this does not mean that philosophers are forced to remain silent on normative issues. Lawrence Boland has constantly argued for a kind of Socratic role for methodology. Following the later Popper and rejecting naïve falsificationism, he sees an apt role for methodology in criticising economic theories on their own terms, thereby helping to improve our understanding of the theories and (indirectly) even the theories themselves.[30] There are no general external standards for assessing a theory, but there is the legitimate question of whether or not a theory is a useful contribution to the problem it was set out to solve. The role of philosophers is now to adopt a critical attitude and show practitioners the limits of their respective approaches. When a methodologist is attracted by the economics of scientific knowledge (ESK), she can additionally propose institutional reforms that foster a critical but constructive attitude in the respective scientific communities.

As seen in the above case studies, errors often lie not in the theory or model itself but in its interpretation. Within science there are incentives to over-interpret the power of the research programme one has chosen: Once a community has settled on certain standards (i.e. in Kuhn's words established a paradigm), there is nothing to lose and much to gain from claiming that pressing problems can be solved in the near future, even if this claim is hard to defend.

The big advantage held by an external methodologist (and of course, it does not matter whether she works in the philosophy or the economics department) is her relative independence from established scientific communities; she is in a better position to analyse whether or not the promises made by a theory are likely to be kept, because her future does not depend on those promises. In general, this results in a much more sceptical position that often stresses the imperfections of models and recommends caution and modesty concerning the status of our current scientific theories. This does not mean that philosophers have to take a critical attitude against all scientific theorising. On the contrary: As I have shown throughout chapter 4, philosophical reasoning can help to understand the methodological status of economic theories and thereby leads to better defences than those given by practitioners. The characterisation of neoclassical economics as a separate and inexact science, the methodological debate about assumptions and the rejection of realism as a useful term for settling questions of theory appraisal are some examples where methodological reasoning was used against short-sighted attacks criticising the »unrealisticness« and low predictive precision of economics. Even if philosophers cannot claim to have superior insight in the »right« methodological rules, their professional training can help in applying the principle of charity and making economic theorising as strong as it

[30] See e.g. Boland (1994) for a concise summary of this view.

can get – which is a first step towards a methodological discussion that can actually connect to practitioners and give them useful advice for improving their theories.

In a best-case scenario, philosophical analysis could clear up ongoing methodological quarrels in economics, such as the one between experimental economics and the neoclassical tradition. Here methodological arguments have shown that the two research traditions can coexist peacefully when it is made explicit that neoclassical economics, using rational choice for explaining long-term macro-effects and experimental results, describes the way individuals make up their minds. This does not mean that experimental results should have no influence at all on neoclassical economics: Again, by their relative independence from existing communities, philosophers are in a good position to discuss which kinds of dogmatism are justified and which are not. While neoclassical economics can be defended against *some* charges, it is certainly not sacrosanct, as the now widely accepted integration of institutions in the neoclassical models shows.[31] The hard core of economic reasoning has proved to be more flexible than it seemed in the days before Gary Becker, and philosophers or historians of economics can point to past changes when an economist appears to be too dogmatic.[32]

An important precondition for any methodologist to settle methodological disputes among economists is his ability to connect to the daily work of practitioners. In order to do this, two things must be avoided: First, the methodologist should not try to reform economics from the bottom up or claim to have knowledge about a superior methodology without having done any successful research himself. Second, she should not spend too much time on settling subtle philosophical problems that may have no relevance for practicing economists. I have tried to adhere to both demands in this work, and indeed one main argument I leveled against the realists in economic methodology was the irrelevance of the realism vs. anti-realism debate for theory appraisal.

A main philosophical result of this work is the thesis that global relativism about aims is irrefutable, but as I argued in section 4.1.4.1, that does not lead

31 See section 2.4.1.

32 Note that it is of course the right of every scholar to be dogmatic about methodological choices, as a methodology is nothing more than a promise for future research. If an economist does not believe a certain proposal to be fruitful, she does not need to follow it. But dogmatism ends when she actively tries to hinder others from pursuing their different methodologies. Some methodologists argue that this has been the case in economics. See Blaug (1998). As many former heterodox assumptions have made their way into orthodoxy, and as there is plenty of room to tackle economic questions in other social sciences (where neoclassical economists have less of an influence), I am not convinced by these voices. See Colander (2007) for a similar point of view.

to »anything goes« at the local level where the aims are set. It is important to note that even if philosophy can clear up misunderstandings in some cases, the great majority of methodological disputes are settled in the respective sciences themselves: In section 4.1.3 I outlined how the competitive structure of science, combined with its inherent openness, can help to resolve many issues over time without the need for external interference.

If you are already convinced that my pragmatic framework is the best we can have for theory evaluation in economics, you can skip this final section, in which I will try to refute some critical remarks against this concept.

5.3. Taking the Wind out of the (putative) Critics' Sails

The first and most fundamental charge that may be brought against the pragmatic way of theory evaluation is the point that it is, in fact, too pragmatic. Critics arguing along this line would suggest that pragmatic theory evaluation must be rejected because it undermines the traditional aim of science, which is the search for truth. However, this charge is misleading: The pragmatic framework for theory evaluation allows scientists to search for whatever they like, be it truth, empirical adequacy or consistency.[33] The impression that a pragmatic theory evaluation does not allow one to search for truth may be created by the fact that there is no way to know whether or not we have achieved truth – or even if we are close to it.[34] This is why a pragmatic theory evaluation must suspend judgement on the achievement of this aim and propose other, more workable criteria.[35] Those criteria, such as empirical adequacy in the context of a given problem, may well be accepted by those who look for truth, because truth (if ever found) is surely empirically adequate and will lead to the solution of many problems. What pragmatic theory evaluation must reject is any exclusive status for selected aims: If a philosopher of science would state that truth is and always will be the only aim and criterion for scientific theories, the pragmatist cannot accept this, as he generally refuses any evaluation of aims and therefore talks only about the means to achieve them. The plurality of aims is a basic assumption of pragmatic theory evaluation, because if there were only *one* legitimate goal for science, pragmatic instrumental reasoning would collapse with the rule-based methodologies it was designed to replace. The pragmatic rejection of monistic conceptions is of course dogmatic as well (as any fundamental assump-

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33 It even allows for non-scientific aims such as personal happiness. The only aims that are excluded from the start are those that are internally self-contradictory.

34 Remember the discussion in section 4.2.1.4.1.

35 In a way, there is a point to the criticism that pragmatic theory evaluation excludes truth as a criterion: This is because pragmatic theory evaluation needs viable criteria. It is hard to think of any form of theory evaluation that does not.

tion necessarily is), but it can be well argued for: In section 4.1.4.3 I presented some traditional criteria for theory evaluation, and by this it became clear that there is indeed a plurality of norms that can be attributed to scientific theories. Note that while pragmatic theory evaluation suspends judgement on the matter of aims, this does not mean that it counts the achievement of any aim as »scientific«. Whether or not something is called »scientific« depends on how we understand the word »scientific« – so the definition of the predicate »scientific« is first and foremost not a matter of theory evaluation but a matter of defining words.[36]

With the above arguments in mind, it is easy to refute a slight variation of the criticism that pragmatic theory evaluation undermines the search for truth: Critics may also say that the strong problem-orientation of pragmatic theory evaluation leads to an unfair disadvantage for fundamental research. However, a careful reading of my arguments will show that this is a misunderstanding: Focusing on problem-solving by no means restricts the domains of valid problems. Highly abstract theoretical problems of fundamental research are valid problems as well, and there is no need for additional justification; increasing our knowledge is a strong enough reason.[37]

While pragmatic theory evaluation is neutral towards problem selection, a side effect may indeed be harmful to some projects: As pragmatic theory evaluation requires an honest description of the problem a scientist tries to solve, it can cause trouble for those who tend to over-interpret their results and hence cannot keep the promises they are making. Thus pragmatic theory evaluation *can* have destructive effects on science, but for the right reasons: If an economist claims to be doing policy-relevant research and is in fact merely proofing existence theorems for Nash equilibria, pragmatic theory evaluation enforces a higher transparency and will give negative assessments if the claimed objectives are not met. True pluralists may be offended by this, but any methodology that wants to be normative must be able to criticise some aspects of science. The fact that criticism comes from both monists (who think that the pragmatic framework is too permissive) and from pluralists (who fear that some parts of science will be under attack) indicates that pragmatic theory evaluation has in fact found the right balance.

Another criticism against a pragmatic framework might be the claim that it is far too imprecise. For the framework, i. e. for the general concept, this is true. Pragmatic theory evaluation refrains from giving precise prescriptions,

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[36] Again, in section 4.1.4.3 I argued that there is a relatively stable core of values that define Western science which does not provide strong demarcation of science from pseudo-science but still provides a good idea of what science is about.

[37] If fundamental researchers want to justify their work they could point to the fact that even the most abstract theory can lead to varieties of applications when solving practical problems. Just think of the development of philosophical logic – which is the theoretical basis of any computer.

as it recognises that different aims require different methods of criticism. As shown in chapter 2.3, traditional empiricist standards are precise, but ill-suited for theory evaluation in economics. Therefore, the imprecision of the pragmatic framework for theory evaluation can be an advantage rather than a disadvantage, because it allows for applying different standards to different problems. As the case studies in section 5.1 have shown, the imprecision of the general framework allows nonetheless for criticism of individual models that is precise and to the point. So this criticism can be answered by pointing out that precision is *not* to be sought after in the general framework but only when assessing single models.

The point of criticism I wish to answer now is influenced by the postmodern shift discussed in chapter 3. Many postmodernists are convinced that philosophy of science cannot be prescriptive, and therefore, in their view, philosophers or methodologists should not be dealing with appraising the aptness of a theory for solving given problems. As I argued in chapter 5.2, there are several ways that philosophers can contribute to theory evaluation, as their institutional and intellectual independence from practicing scientists allows them to address fundamental issues of theories that practicing scientists usually do not deal with. Of course philosophers must have detailed knowledge about the theories they want to assess, but if this is the case there is no reason why they should completely refrain from normative judgement. Rather the opposite: If philosophy of science were not normative at all, it would be a separate enterprise that had no relevance for anyone working outside philosophy.

The last point of criticism I will address here comes from sociology of science. Similar to postmodernists, some sociologists of science argue that the dynamics of science do not follow rational criteria but are driven by sociological forces such as power, influence, herding-behaviour or desire for fame. They claim that the social nature of science makes any attempt of rational theory evaluation futile. In my opinion, this conclusion is drawn too quickly. Even if philosophers admit that scientists do not follow rational criteria, this does not actually refute the point of rational criticism. An analogy is helpful here: Even if politicians may not be interested in common welfare and are merely trying to win the next election, this does not show that a critical press is useless.[38] In politics as in science, rational criticism *can* lead to improvements, even if only in the long run. As I argued in section 4.1.3, social nature is no reason to step back from normative judgements, even if reform of scientific institutions may be a more efficient means to change sci-

[38] A more radical example is political philosophy. Here critics might argue as well that political leaders do not care about philosophical considerations. However, it is hard to refute that political principals such as the separation of powers have their origin in philosophical arguments.

ence. Those sociologists who fail to see the possible long-term consequences of methodological work do not argue convincingly against pragmatic theory evaluation but rather endanger their own work: Like the postmodernists, they, too, insulate themselves and unintentionally characterise their work as nothing but a strange hobby.[39]

Are you convinced that a pragmatic approach to theory evaluation is the way to go? You might argue that it was easy for me to arrive at a pragmatic way out of the dichotomy between positivist methodology and relativism: This dichotomy, you might suggest, is simply a result of my own over-interpretation of philosophical positions. But even if I were to admit that both the positivists and relativists are merely straw men in my presentation here (which is not the case), and that both positions are in fact much more pragmatic than I show them to be in this work[40], does this really make the pragmatic approach less attractive? Quite the opposite – this would provide it with argumentative support! But hey, what kind of philosophical work would this be without a dichotomy resolved through a magical solution?

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39 Note the well-known saying (attributed to Richard Feynman) that philosophy of science is about as useful to scientists as ornithology is to birds. While the normative claims of early empiricists were clearly too strong, this is no reason to step back from normativity completely and thereby undermine the public image of philosophy of science.

40 See e.g. Richardson (2007) or Price (1997) for pragmatic aspects of Carnap's philosophy. McCloskey even attributes the term »pragmatic« to herself from time to time. See e.g. McCloskey (1983), p. 483.

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7. Appendix

7.1. Quotes Supporting the Analogy of Kuhn's Methodology to the Economics of Standardisation

1. »When we observe an economy in the midst of an extended period of rapid and far reaching technical change, we can usually identify episodes of disruption and episodes of stabilization.«[1]

The analogy to revolutions and normal science is obvious.

2. »An integral part of this stabilizing process is technical standardization, which takes place either through an entire market adopting a single technology, or through modification and adaptation of existing technologies.«[2] »… it is sometimes possible to finesse this issue by observing that this is a case of several distinct markets.«[3]

This is an analogy to the different coverage of paradigms. The second quotation suggests a separation of markets by standards – a clear parallel to the separation of different scientific branches by paradigms.

3. »In this situation, users have two choices: they can either modify their technologies; or they can agree (tacitly perhaps) on a standard technology, requiring that those not already using it switch. Joint modification will take place if the cost of modifying the technologies is less than the cost of this switching. Costs of modification include the development and installation of the technical changes, whereas switching costs include costs of acquisition of new physical and human capital as well as the loss of any function that was unique to the abandoned technology. At the global level, switching costs will exceed modification costs if there is a large installed base of users, if the cost of capital acquisition is very high, or if the current technology is unique in providing a very valuable function. In any of these cases, we observe a group of users who

1 Cowan (1992), p. 1.
2 Cowan (1992), p. 1.
3 Cowan (1992), p. 9, footnote 10.

are effectively locked in to a technology because the costs of switching away from their technology are too high to bear.«[4]

This is easily transferred to paradigm-shift. As long it is not difficult (costly) to adapt the paradigm to new problems it will not change. The cost categories fit perfectly.

4. »There are several sources of increasing returns to adoption, not all of which apply to every technology. But all of which act to increase benefits to or reduce costs of adopting a technology as that technology is more heavily adopted.

Learning by doing refers to the decline in unit costs as the number of units produced increases. As production experience increases, how to organize production efficiently comes to be better understood, and production costs fall. Thus the (social) cost of later adoptions is lower than early adoptions of any particular technology.

Learning by using refers to learning about how to improve the design and use of a technology. Early designs are typically far from optimal, as designers do not know exactly what the capabilities of the new technology are, or exactly the uses to which it will be put. As adopters use the technology, they communicate with the manufacturers, and the design evolves to increase the benefits from its use.

Increasing returns to scale are said to exist if cost of production falls as the scale of production increases. If, for example, the production technology has very high fixed costs, then spreading these costs over a larger number of units produced will lower the average cost of production.

Network externalities differ from the previous sources of increasing returns in that to capture them requires using a technology that many other people are currently using. Many technologies are useful in their own right, but increase in value as other people use them, 'joining the network.' As more people subscribe to the same electronic mail network, the more valuable it becomes to any user because he can communicate (share information) with more people. . . .

The presence of any of these features in a technology is a source of increasing returns to adoption, and will act to encourage standardization by market exclusion. As more agents use a technology the greater is the benefit from using it, and so the stronger are the incentives for other agents to adopt it.«[5]

All these sources of increasing returns to adoption hold as well when science and not technical standards is concerned.

4 Cowan (1992), p. 3.
5 Cowan (1992), p. 6-7, emphasis by S. D.

5. »Typically, competitions for market share take place when technologies are relatively new. This is the time, of course, when there is the most uncertainty about them – about their characteristics, the functions they will perform, and the functions users would like them to perform. Early adoptions take place within this uncertainty, but, through the information they provide, contribute substantially to the formation of beliefs about all of these issues. Reduction of uncertainty through strengthening the beliefs of future adopters is enough to generate market exclusion.… «[6]

There is a clear correspondence between this quote and the lock-in into a paradigm after the preparadigmatic phase (or revolution).

6. »Agents adopt the technology that they believe will yield the highest net benefits in expected value.… This opinion can be erroneous, of course. If a good standard has bad luck early on … opinion forms that it is bad and so it is set aside while an inferior one is taken up. As the good one is left idle, it has no way to prove its superiority – opinions about it do not change, and there is never incentive to switch to it.«

This corresponds to Kuhn's assertion that the paradigm with the »best« promise of success is being established whereas other potential paradigms never fully develop. These are path dependence problems of the second degree.

7. »What is important from the point of view of standard setting … are the relative merits of the competing technologies or standards. In a new technological paradigm, problems arise in making these comparisons.«[7]

As I already noted in section 4.1, this is analagous to the incommensurability problem.

8. There is a passage in *The Structure of Scientific Revolutions* which puts forward typical economic reasoning. Kuhn gives economic reasons for rejecting or ignoring anomalies.[8] Earlier in the book he argues that old paradigms can still be useful for solving problems. Thus we have to use the most advanced paradigm only when it is really worth it.[9]

The collection of the above quotes shows the vast similarities between Kuhn's methodology and the economics of standardisation.

6 Cowan (1992), p. 7-8.
7 Cowan (1992), p. 11-12.
8 See Kuhn (1962), p. 95.
9 See Kuhn (1962), p. 39.

7.2 Harrod-Domar Models

For growth to be on equilibrium, the following conditions must hold:[10]

> (P = production potential, c = marginal product of capital, K = capital, I = investments, s = propensity to save)

$\mathrm{V}P = \frac{1}{c'} * I$; whereas c' is defined by $c' = \frac{I(=\mathrm{V}K)}{\mathrm{V}P}$

Under the »heroic« assumption c' = c = constant this leads to:

$$\mathrm{V}y = \frac{1}{s}\mathrm{V}I;\ \mathrm{V}P = \mathrm{V}y \rightarrow \frac{1}{c'}I = \frac{1}{s}I \rightarrow \frac{\mathrm{V}I}{I} = \frac{s}{c'}$$

Hence, the growth of investments is on equilibrium if it equals the fraction of propensity to save to the marginal product of capital.

7.3. The Basic Solow Model

Here is the basic equation (t = globally neutral technical progress, w = wage rate, W = wage sum):

$$y = t(L^m * K^{1-m})$$

The productive elasticity of each factor equals its marginal rate of substitution, therefore:

$$\overline{m} = \frac{\partial y}{\partial L} : \frac{y}{L};\ \overline{1-m} = \frac{\partial y}{\partial K} : \frac{y}{K}$$

Given perfect competition, the payment to factors (wage rate, interest rate) equals their marginal productivity, hence:

$m = w\frac{L}{y};\ m = \frac{W}{y};$ m is equal to the wage share

10 The short exposé of the basic structure of Harrod-Domar and Solow models is based on a lecture by Egon Görgens in 2006, University of Bayreuth, Germany.

$1-m = i*\frac{K}{y} = \frac{R}{y}$; 1-m is equal to the revenue share.

If m is constant, the ratio of factor usage must equal the ratio of factor prices. By this we can compute m, as l, i, K and L are observable.

$$\frac{l}{i} = \frac{\overline{m}}{1-m} * \frac{K}{L}$$

If one differentiates the basic equation to time, the growth formula emerges (G = growth):

$$G_y = G_F + mG_A + (1-m)G_K$$

Under the assumption that growth is on equilibrium, the following formulas hold:

$$G_y = G_K \rightarrow (1-m)G_k = G_y - mG_y\,,$$

$$G_y = G_t + mG_L + G_y - mG_y \,| -G_y : m$$

$$G_y = \frac{G_t}{m} + G_L\,.$$

Growth is now independent from the growth of the capital stock.

7.4. Kitcher's Argument for the Possibility of the Division of Labour in Science

The division of labour result is derived by Kitcher as property of the noncooperative Nash-equilibrium of a single-prize lottery game played by N self-interested scientists.[11] Let there be two possible theories that scientists might work on: T_1 and T_2. There are N scientists (N>0) and each scientist works on one and only one theory. Thus, if we let n be the number devoted to T_1, then N-n will be devoted to T_2. Let A_1 be the assertion that T_2 will come to be

11 I follow the presentation of Wade Hands. See Hands (2001), p. 370 et sqq.

accepted and A_2 the same for T_2. Because one of the theories will ultimately come to be accepted, we have the following conditional probability relation:

$$P(A_1|n)+P(A_2|n) = 1 \text{ for all } N \leq n$$

We also make the reasonable assumption that if no one works on a theory it will never be accepted, so:

$$P(A_1|0) = P(A_2|N) = 0.$$

Each scientist will maximise their own expected utility and each know that the other N-1 scientists will also do so. To simplify the analysis, assume that the scientists are competing for a prize of 1 unit of utility and that prizes in this (lottery) game are allocated in the following way. Once it is known whether T_1 or T_2 wins (come to be accepted) the scientists who worked on the winning theory get their names thrown in a hat and the (1 unit of utility) winner is selected at random; the others working on the winning theory also get nothing. Thus, if n scientists are working on T_1, the expected utility of any of these n scientists is given by EU_1 and the expected utility of any one of the scientists working on T_2 is given by EU_2, where

$$EU_1 = [1{*}P(A_1|n)]/n \text{ and } EU_2=[1{*}P(A_2|n)/N-n.$$

The equilibrium concept for the lottery game is a Nash equilibrium. A group of n scientists working on T_1 and N-n working on T_2 is a Nash equilibrium if no scientist would defect (change the theory they are working on) given the play of the other scientists. The distribution n will be such a Nash equilibrium if it is both stable upwards and stable downwards; *stable upwards* means that no one will move from T_2 to T_1 (n will not get bigger), whereas *stable downwards* means that no one will move from T_1 to T_2 (n will not get smaller). Thus, the Nash equilibrium distribution n* is characterized by the following two conditions:

$$\frac{1{*}P(A_1|n^*+1)}{n^*+1} \leq \frac{1{*}P(A_2|n^*)}{N-n^*} \qquad (n^* \text{ is stable upwards})$$

$$\frac{1{*}P(A_2|n^*-1)}{N-n^*+1} \leq \frac{1{*}P(A_1|n^*)}{n^*} \qquad (n^* \text{ is stable downwards})$$

The Nash equilibrium will exhibit the cognitive division of labour if some scientists are working on T_1 and some scientists are working on T_2. Thus, we can say that the equilibrium distribution supports the cognitive division of labour if $n^* \neq 0$ and $N^* \neq N$. Notice that $n^* \neq 0$ if $n=0$ is not stable upwards (that is someone will start working on T_1 whenever $n=0$) and that $n^* \neq N$ if $n=N$ is not stable downwards (that is someone will quite working on T_1 whenever $n=N$). Thus, the Nash equilibrium distribution n^* supports the cognitive division of labour when the following two conditions hold:

$$\frac{P(A_1|n^*-1)}{1} > \frac{P(A_2|0)}{N} \Rightarrow P(A_1|1) > \frac{1}{N} \qquad (1)$$

$$\frac{P(A_2|N-1)}{1} > \frac{P(A_1|N)}{N} \Rightarrow P(A_2|N-1) > \frac{1}{N} \qquad (2)$$

The fact that the two conditions on the right hand are relatively easy to satisfy completes the argument that sullied scientists could (acting noncooperatively) bring about a cognitive division of labour.